THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

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

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

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ABRIDGED.

Extract of a Letter of M. Huygens, to the Author of the Journal des Savans, of July 25, 1672, attempting to explain the Cause of that odd Phenomenon of the Quicksilver's remaining suspended far above the usual Height in the Torricellian Experiment. N° 86, p. 5027.

THE Experiment is briefly this: that a tube, being after the Torricellian way, filled with mercury, and before inversion perfectly purged of air, does, when inverted, remain top full, even to the height of 75 inches.

M. Huygens, as a probable cause of this strange effect conceives, that besides the pressure of the air, which keeps the mercury suspended at the height of about 27 * inches, and of the truth of which we are convinced by a great number of other effects; there is yet another pressure stronger than that, of a more subtle matter than air, which without difficulty penetrates glass, water, quicksilver, and all other bodies which we find impenetrable to air. This pressure, he says, being added to that of the air, is capable of sustaining the 75 inches of mercury, and possibly more, as long as it works only against the lower surface, or against that of the mercury, in which stands the open end of the tube: but as soon as it can work also on the other side, (which happens when striking against the tube, or intromitting into it a small bubble of air, you give way to this matter to begin to act) the pressure of it becomes equal on both sides, so that there is no more but the pressure of the air, which sustains the mercury at the ordinary height of 27 inches.

If you ask why the quicksilver in the tube of this experiment does not feel the pressure of this matter, even whilst that vessel is yet full; since M. Huygens supposes, that it pierces without difficulty the glass as well as the mercury, &c? and why the particles of this matter do not join together and begin the

* French measure, or nearly 29 inches English measure.

pressure, as they go and come through the whole extent of the mercury, and that the glass does not hinder their communication with those that are without.

To remove this difficulty, which in M. Huygens's own opinion is very great, he answers, that though the parts of the matter, by him supposed, do find passage between those that compose the glass, quicksilver, &c; yet they there find not sufficiently large ones for many to pass together, nor to move there with that force which is requisite to separate the parts of the quicksilver, that have some connexion together. And this very same connexion, he says, is the cause, that though on the side of the inner surface of the glass, which touches the suspended mercury, many of its parts be pressed by the particles of this matter; yet there being also a great number of them that feel no pressure by reason of the parts of the glass, behind which they are placed; they retain one another, and remain all suspended, because there is much less pressure on the surface of the quicksilver that is contiguous to the glass, than upon that below, which is all exposed to the action of that matter which makes this second pressure.

The ingenious and candid author of this solution acknowledges himself, that it does not so fully satisfy him as not to leave some scruple behind; but then he adds, that that keeps him not from being very well assured of that new pressure, which he has supposed besides that of the air, by reason as well of the experiment already alleged, as of two others, which he subjoins, to this effect:—

First, When two plates of metal or marble, whose surfaces are perfectly plain, are put one upon another, they do so stick together, that the uppermost being lifted up, the undermost follows without quitting it: and the cause thereof is justly ascribed to the pressure of the air against their two external surfaces. He taking then two plates, each of them but about an inch square, being of that matter of which anciently they made looking glasses, and closing so exactly together, that without putting any thing between, the uppermost keeps not only up the other, but sometimes also with it three pounds of lead fastened to the lowermost, and thus they remain together as long as you please. Having thus joined them and charged them with three pounds weight, he suspended them in the recipient of his engine, and exhausted it of air so far as that there remained not enough to sustain by its pressure as much as an inch high of water; and yet his plates disjoined not. He adds, that he made the same experiment by putting spirit of wine between the two plates; and found, that in the recipient evacuated of air they sustained, without being severed, the same weight they did when it was full of air. This he thinks shows clearly enough, that there remains yet in the recipient a pressure great enough after that of the air is thence taken away; and that there is no more reason to doubt of it, than of the pressure of the air itself.

The second experiment is, that whereas the effect of a syphon of unequal legs, by which you make the water of a vessel run over, is no longer ascribed to a fuga vacui, but to the weight of the air, which pressing upon the water of the vessel makes it rise in the syphon, whilst on the other side it descends by its weight; M. Huygens found a means to make the water of the syphon run, after the recipient was exhausted of air, and he saw, that with water purged of air* it did the effect as well as without the recipient. The shortest of the legs of the syphon was 8 inches long, and its aperture, of two lines. And he will not have us doubt, whether the recipient was well exhausted of air; for he did assure himself of that, as well by finding that there came out no more air through the pump, as by other more certain marks.

And this he takes for a further confirmation of his supposition of a pressing matter more subtle than the air. To which he adds, that if you take the pains of searching, to what degree the force of this pressure reaches, (which he says cannot be better made than by pursuing the experiment with tubes full of mercury, yet longer than those employed by M. Boyle,) it will perhaps be found, that this force is great enough to cause the union of the parts of glass and of other sorts of bodies, which hold too well together to be conjoined only by their contiguity and rest, as M. Descartes would have it.†

Extract from Mr. John Templer's Letter of March 30, 1672, *to Dr. Walter Needham, concerning the Structure of the Lungs.* N° 86, p. 5031.

In answer to the request of an ingenious physician, I was lately engaged to give my thoughts on the structure of the lungs, as follows :—

I formerly conceived the lungs to be composed of a multitude of vesicles; into which opinion I was persuaded by inflation in the aspera arteria of fowls; and observing the continuation of many vesicles extended from the bronchia through

* M. Huygens has made this experiment, as well with water as with mercury.

† M. Huygens, though a very great mathematician, in this memoir, very unphilosophically, in order to explain the cause of the adherence of certain bodies together, when in close contact, has recourse to the action of a matter, of whose existence there has never been any kind of evidence. The effects in such instances as above-mentioned, have since that time been more rationally ascribed to the attraction of cohesion; a principle which manifests itself whenever bodies are brought into close contact. Hence arises the considerable adhesion of two perfect planes: hence the compact and firm adherence of masses of matter: hence the ascent of fluids up the sides of their containing vessels: hence their ascent even in open tubes, to considerable heights, so much the higher as the tubes are smaller: hence the necessity of giving a considerable width to the barometer tubes: hence even a drop of water falls not from a mass without a visible reluctance, &c.

the abdomen to the anus, (which I imagine to be the cause of the constant motion of the anus in fowls; the air having ingress and egress there; and also that to be the reason why the anuses of fowls are, in malignant distempers, applied to draw the infection out of the body:) I thence conjectured the substance of the lungs to be a complication of a multitude of vesicles with the sanguineous vessels. And in this opinion I thought myself confirmed, by blowing into the aspera arteria of quadrupeds, when I had cut off part of the exterior membrane of one lobe of the lungs, and found the lungs to rise with unequal protuberances not unlike bladders.

But this second contrivance, which I am going to describe to you, has much shaken that conjecture. March 2, 16$\frac{71}{72}$, I made a ligature about a dog's neck, and opening both the jugular veins with a pretty large orifice, I let him bleed to death, (using this way to prevent being overcharged, either with any quantity of blood, or with blood coagulated; both which would have been hazarded, in case I had either strangled the dog, or cut one or both of the jugulars asunder:) immediately I opened the thorax, and tying the vena cava, with all the passages from the left ventricle of the heart, or its auricle, I cut the lungs with the heart and aspera arteria entirely out. To the aspera arteria I fitted a syphon 7 inches long, which I thrust 2 inches in length into the said artery, and fastened it with a strong binding of packthread. This done, I blew up the lungs, and fitting a cork to the end of the syphon, hung them in a chimney to dry. In a quarter of an hour they subsided about a sixth part; whereupon I ordered a person to watch them, and to blow them up as often as they subsided. Which course continued, they would not the next morning subside a fourth part in three hours. And (excepting three quarters of an inch distance from the circumference of the lobes, where the thinness of the substance of the lungs gave the external heat the advantage of a sudden passage, and quick dispatch of drying those parts least furnished with moisture,) I did not perceive, making a proportionable allowance for the drying of the whole substance of the lungs, any considerable subsiding in two days more. But upon the blowing in at the syphon (whose ligature I was now forced to renew,) I could easily feel the air pass through the external membranes, both on the convex and concave sides, towards the extremity of the circumference of the lobes; but most abundantly on the concave side.

March 5th, I carefully cut off one of the lobes, and the inward structure seemed like a cane or dried flag when transversely cut; and, upon blowing in at the syphon, I fancied the air to come equally out at all the pores I had exposed to view. Whereupon I fixed spittle in several places, and upon fresh blowing found multitudes of bubbles, made in the denudated parts of the lobe. I immediately made a deep transverse incision into that lobe, and blowing in at the syphon, found the air to come so freely out at the larger ramifications of the bronchia, that I could not give the lobe a considerable rise with a strong blast: yet upon stopping with my fingers the larger passages of the bronchia, which I had cut, I found that lobe upon a fresh blast, rise considerably with unequal protuberances (where the incision was made) giving no small suspicion of some latent vesicles. Hereupon I tied that lobe above the incision, and taking off part of the external membrane of another lobe, (having first tied up all the rest of the lobes) I poured water into the syphon, and applied a strong blast, in hopes to have the water come forth in streams at all the pores; but that did not satisfactorily succeed, it coming out in a confused irroration of the external surface, without any ebullition, unless at the larger ramification of the bronchia. Then I tied up this second lobe, and untied a third, pouring in an ounce of the oil of turpentine; at the syphon I gave a small blast, and corked it up. Two hours after I took off the small membrane of that lobe, and upon a gentle blast at the syphon found an ebullition of infinitely small bubbles.

March 10, (having continued it to the chimney) I cut all the lobes in pieces by different and various irregular incisions; whence I could easily observe the several ramifications of the aërial and sanguineous vessels, with their continuation to the circumference of the lobes, and a proportionable diminution as they were at a further distance from their original.

Shall I hence conclude the structure of the lungs to be a complication of a multitude of the ramifications of the bronchia and sanguineous vessels? and that the seeming vesicles were occasioned only by the violence of the blast, and the dryness of the extreme and smallest passages of the aërial vessels; whereupon those, nearest to the bronchia (being moister) were, more than their ordinary proportion, extended, upon hindrance of a free and usual passage to the air in the lesser vessels or their extremities? *

Some Astronomical Observations in part already made, partly to be made. By Mr. John Flamsteed. N° 86, p. 5034.

These prognostications and observations are now no longer of any use.

* The forcible inflation here resorted to is a great objection to deducing any conclusions from these experiments, which indeed appear to be of little value. The bronchial tubes, which are ramifications of the trachea, ultimately lose their cartilaginous structure, and terminate in membranous vesicles or cells, which in the act of inspiration become distended with air. Upon the surfaces of these air-cells are spread the minute ramuli of the pulmonary blood-vessels, so as to have their contained fluid (the blood) subjected to the chemical action of the air; not indeed by immediate contact, but with no other intervening medium than the exquisitely thin coats of the distended vesicles or cells.

An Accurate Description of the Lake of Geneva, not long since made by a Person that had visited it divers Times in the pleasantest Season of the Year; and communicated to the Publisher by one of his Parisian Correspondents. Translated by the Editor. N° 86, p. 5043.

Instead of reprinting the description here given from the original Transactions, we think it better to refer our readers to the description of this lake as given by that accurate Swiss naturalist De Saussure, *(Voyages dans les Alpes et Hist. Naturelle des Environs de Geneve)* and to the travels of our countryman Mr. Coxe.

An Account of some Books. N° 86, p. 5047.

I. Lux Mathematica, Collisionibus Johannis Wallisii et Thomæ Hobbesii excussa: Multis et fulgentissimis aucta radiis, Auth. R. R. Adjunctâ Censura Doctrinæ Wallisianæ de libra, unà cum Roseto Hobbesii. Lond. 1672, in 4to.

The author of this book (probably Mr. Hobbes himself) states that he has deduced the rise and occasion of the disputes between Dr. Wallis and Mr. Hobbes, and commended the many and difficult propositions and demonstrations said to be advanced by the latter of them, and compared these with those of Dr. Wallis. He then proceeds to the controversies themselves, endeavouring to vindicate Mr. Hobbes's assertions from the objections of Dr. Wallis.

II. Optique de Portraiture et Peinture, contenant la Perspective Speculative et Pratique accomplie, &c. Par Gregoire Huret, Desseignateur et Graveur ordinaire de la Maison du Roy, et de l'Academie Royale de Peinture et Sculpture. A Paris, 1670, in fol.

This elegant volume in French states the chief aim of its author to have been, to contribute what he could to the instruction and improvement of youth, studious of these excellent arts, and groundedly to teach them the rules and other means, that are really useful and absolutely necessary to them in the same.

III. Christiani Friderici Germani, Physici Chemnicensis, Academici Curiosi, Homo ex ovo. Chem. 1672, in 4to.

This author having collected what has of late years been asserted and published concerning the generation of other animals, as well as of fowl and fish, out of eggs; and taken with Kerkringius particular notice, tam virgines quàm conjugatas sæpissimè ova excernere (which he no more wonders at, than that hens and other birds are matres et tamen virgines) then proceeds to consider the advantage of this doctrine, and its happiness in explaining many phænomena, hardly explicable without it; such as the production of more fœtuses than one; the production of monsters: the many odd symptoms in women, from the putrefaction or imperfect constitution of the egg or eggs; the production of molas; barrenness, &c. Having dispatched this, he takes occasion to examine the question, an fieri conceptio possit extra uterum? ubi nonnulla disseruntur de homunculo chymico sive paracelsico; quæ apud ipsum vide authorem.

IV. A short and sure Guide in the Practice of raising and ordering of Fruit-trees; by Francis Drope, B.D. late Fellow of Magdalen College in Oxford. Oxford, 1672.

This piece appears by the preface to have been written from the author's own experience. The particulars insisted on in this discourse are principally: 1. Of raising stocks from the seed. 2. Of the nursery. 3. Of grafting. 4. Of innoculating of stocks raised without seed, and trees without incision.

An Extract of a Latin Epistle of Dr. Joel Langelot, Chief Physician to the Duke of Holstein now Regent; wherein is represented, that by these three Chemical Operations, Digestion, Fermentation, and Triture, or Grinding (hitherto, in the Author's Opinion, not sufficiently regarded) many Things of admirable use may be performed. Translated by Mr. Oldenburg. N° 87, p. 5052.

It is sufficient to give the title of this long alchemical paper, without making any extract from it. Philosophers can employ their time better in these days, than by reading accounts of experiments said to yield results not reconcilable with the known and immutable properties of natural bodies.

An Extract of a Letter from Mr. Lister to the Editor, both enlarging and correcting his former Notes about Kermes; and insinuating his Conjecture of Cochineal being a Sort of Kermes. N° 87, p. 5059.

We must correct as well as enlarge our notes concerning kermes.* These things are certain:

1. That we have this year seen the very gum of the apricot and cherry-laurel trees transudated, at least standing in a crystal drop upon some (though very rarely) of the tops of these kermes. 2. That they change colour from a yellow to a dark brown; that they seem to be distended and to wax greater, and from soft to become brittle. 3. That they are filled with a sort of mites; that small

* Compare herewith what was published in Nos. 71, 72, and 73.

powder (which I said to be excrement) being mites as well as that liquamen or softer pulp (which I took to be bee's meat) concerning both which particulars I am pretty well assured by my own, and also by my ingenious friend Dr. Johnson of Pomfret's more accurate microscopical observations. 4. That the bee-grubs actually feed on mites, there being no other food for them. 5. That there are other species of bees or wasps besides those by me described; which are sometimes found to make these mites their food; Dr. Johnson having opened one husk, with one only large maggot in it. 6. That there are probably different sorts of mites in these husks, making possibly different species of kermes; for some I have found to hold carnation-coloured mites, enclosed in a fine white cotton, the whole husk starting from the twig, shrivelling up, and serving only for a cap or cover to that company of mites. Other mites I have seen white, and (which is most usual) the husks continuing entire, and not coming away from the twig they adhere to, and but little cotton at the bottom. Those of the first sort are the white cob-webs on the vine, described by Mr. Hook Micrograph. Obs. 56. 7. That the shrivelled cap to be found upon the mites enclosed in cotton, as also the whole husk itself, if taken early in April, while soft, will, dried in the sun, shrink into the very figure of cochineal; whence we guess that cochineal may be a sort of kermes taken thus early and sun-dried.

Hitherto this summer's notes concerning kermes. This advantage at least we may have by them; that the account taken from M. Verney by Dr. Croon, and published in one of the Transactions,* is made more intelligible; the small scarlet powder there mentioned being to be understood of those mites, and they to be distinguished from the bee-grubs, which are changed into the skipping fly, that is, the bee, (for kind at least) by us described formerly, I am, &c.

York, Oct. 9, 1671.

An Extract of a Letter from Mr. Thomas Platt, from Florence, August 6, 1672, *concerning some Experiments there made upon Vipers, since M. Charas's Reply to the Letter written by Sig. Francesco Redi to M. Bourdelot and M. Morus.* N° 87, p. 5060.

The experiments related in this paper afford a complete refutation of Charas's opinion, that the venom of vipers is in their angered spirits, and confirm in the strongest manner the fact advanced by Redi, that their poison resides in the yellow fluid contained in the vesicles attached to their gums. Mr. Platt states,

* See No. 20.

that being at the house of Sig. Magalotti on the 2d of June, 1672, there came Dr. Francini with a box containing many heads, cut off that morning, of vipers, lately brought from Naples. He immediately desired to have some animals to begin his experiments upon; but there being at that time no other company with Signor Magalotti but his brother and I, it was thought fit to stay till next morning, that those gentlemen, who were at the dispute last winter, might be present.

I, however, desired the doctor to make at least one experiment, which being granted, Signor Magalotti was sent to the public market for a couple of pigeons, which were first wounded with the teeth of a viper's head that had been cut off about seven or eight o'clock the same morning. The way of making the wound was, by thrusting twice the master teeth into the fleshy part of the pigeon's breast, till such time as pressing the upper part of the jaw, the two little bladders that serve as gums to the teeth, did empty out upon the wound some of that yellow liquor, which here is supposed to be the true and only poison of the viper. This pigeon being thus bit, and set upon the ground, began to stagger immediately, and died in less than three or four minutes. The second pigeon was wounded in the same manner, but at the first wound there only entered one of the teeth, which brought forth a great deal of blood; the second time they both entered, and this had the same fate, with this difference only, that he languished half a quarter of an hour.

The next morning there met at Signor Magalotti's chambers, besides the company of the day before, Signor Carlo Dati, Signor Vincenzo Viviani, Signor Paola del Ara, Dr. Savona, Dr. Neri, Dr. Fabrini, and some others. Whereupon six pigeons and a cock having been brought, the first thing that Dr. Francini did, was to thrust several thorns of rose shrubs into the breast of one of those pigeons, to manifest that such accidents as might befal those that should be wounded by the teeth of the dead vipers, were not merely caused by the wound. And whereas one of the company began to make some nice reflections, and take some of the heads to measure the just proportions of their teeth, to see what difference there might be betwixt them and the thorns; this made the doctor lose patience, and soon taking a pin, which was none of the least, he gave to the first pigeon, that he could lay hands on, a very deep wound in the breast, which no sooner was got free but began to leap and frisk about the room, as if it had not been concerned in the least.

After this, a pigeon was bit in the breast by both the master teeth of a viper's head, that had been cut off the morning before; the effect was, that the pigeon had the same shaking fits; after which falling upon his belly he died, giving signs a little before of a painful agony, by his often gaping. His end was not only very sensible to him, but also more tedious than that of the other day; for this lived 5 or 6 minutes after his wound. Another having been served in the same manner with another head, died within a quarter of an hour.

All this appearing as yet to the doctor, to exclude but little the doctrine of spirits, which now began to lose ground after so many experiments of dead vipers heads; he took three stalks out of a broom, and having smoothed them and sharpened them at the ends after the manner of a lancet, he drew from the gums of several heads enough of that yellow juice to daub two of those stalks; which, being thus moistened with that liquor, were both put into the breasts of two pigeons, and there left; the like having been done to another with the 3d stalk not covered with that juice, which was at least one third part larger and longer than the other two. In a word, the first two died within 4 or 5 minutes, and the last continues to this very day in Signor Magalotti's pigeon-house as brisk and as fat as ever; his wound in his breast, instead of having caused an inflammation, is now almost perfectly healed.

Whilst these experiments were making, it came into the heads of some to try another, upon the relation that Signor Paolo dell' Ara (lately come from Paris) had made; which was, that some asserted there, that, to swallow a viper's heard was a most certain preservative and remedy against the biting of a viper. Dr. Francini smiled at that fancy; but to give full satisfaction, he made two experiments. The one was, by making the cock swallow a viper's head, and then causing him to be well bitten in both thighs by a live one. But the cock continuing some time before he gave any signs of sickness; not to lose time, he passed to the other experiment, by thrusting the teeth of a dead viper's head into another pigeon, that had before got down one of those heads into his belly. The conclusion was, that both died, the cock within a quarter of an hour, and the pigeon in less than 4 minutes.

The news of these experiments made many persons curious to see them performed once more; so that, some few days after, a rendezvous was made in Signor Magalotti's garden, where, besides the forenamed persons, met Mr. Thomas Frederick, Mr. John Godscall, Abbot Strozzi, Signor Paolo Falconieri, Signor Luigi del Riccio, Mons. Pelletier, Mons. Morelle, Dr. Gornia, Dr. Bellini, Signor Lorenzo Lorenzini, and Signor Pietro Salvetti.

The assembly having been first informed by Dr. Francini of the grounds of this dispute and of the former observations, he began the same experiments by causing 2 pigeons to be bit by a viper's head that had been dead above ten hours, in such a manner that by pressing the gums some of that yellow liquor might drop into the wound. They both died, one in 6 minutes, and the other in 8;

and not being content with this, with another viper's head they poisoned a chicken, which died in 10 minutes. There appeared afterwards another pigeon, that had been wounded many hours before, by a dead viper's head; but it had been dead so long, that the liquor quite dried up in the gums, was become so hard, that none could come to the teeth; whence this pigeon was very well: and Dr. Francini having caused the same bird to be bit again by the same dried head, it had (after a little fluttering with his wings whilst the pain of the biting lasted) no other harm.

A live viper then being taken, 4 chickens were bit by it one after another. The first two, either because the liquor did not penetrate into the wound, or the blood expelled it, appeared not to have any distemper. The 4th looked as if it would die presently; but a little after coming to himself he got clear off for that time. But the third, which seemed at first to be very lively, died within an hour and a half.

There being afterwards a young bitch brought in, she was bit twice by a live viper in the middle of the hanging part of the ear: whereupon she very soon began to give mortal signs, by staggering, vomiting and being convulsed; after which having a little recovered herself, the same accidents returned upon her, and four hours after her being bit she seemed as if dead, holding out her tongue, and looking very ghastly, without any other sign of life than that of painful breathing; to which she added sometimes a faint barking and howling. In which condition she was still found next morning, only her respiration was yet weaker, and she appeared drawing to her end. It was observed, that no part of her body was swelled, nor had any spot upon it. She had voided backward some matter of a very black colour, of which her hind parts being very foul, a swarm of gnats and wasps were devouring her alive: which moved one of the servants of the house, to knock her on the head.

After this two capons and a pullet were bit by a fresh viper, vexed on purpose; and because they gave not then any signs of being ill; they were sent back to their coops, but were surprised at night by a distemper, which in all likelihood proceeded from the poison; for next morning one of the capons and the pullet were found dead.

Dr. Wallis's Answer to the Book, entitled Lux Mathematica, &c. described in Number 86. N° 87, p. 5067.

More of the controversy with Mr. Hobbes, not worth reprinting.

Ejusdem Doctoris Wallisii non nulla, De Centro Gravitatis Hyperbolæ, Prægressæ Epistolæ subnexa. N° 87, p. 5074.

These intricate algebraical expressions and calculations, relating to the centre of gravity, have been properly superseded by the easy method of fluxions, since happily invented.

An Account of some Books. N° 87, p. 5076.

I. Tractatus de Natura Substantiæ Energetica, seu de Vita Naturæ, ejusque Tribus primis Facultatibus; Perceptiva, Appetitiva, Motiva, &c. Auth. Franc. Glissonio, Med. D. et P. et Coll. Med. Lond. Socio, nec non Societatis Regalis Collegâ. Lond. An. 1672, in 4to.

This metaphysical tract is not interesting to philosophers of the present day.

II. Jeremiæ Horroccii* Angli Opera Posthuma: unà cum Guil. Crabtræi Observationibus Cœlestibus; nec non Joh. Flamstedii de Temporis Æquatione

* This splendid genius was born at Toxteth in Lancashire, about the year 1619. From a grammar school in the country, he was sent to Cambridge, where he spent some time in academical studies. He began at 14 years of age to apply to the study of astronomy; but from his want of books, and the moderate circumstances of his father, he could make but small progress in it. About 3 years after he formed an acquaintance with Mr. Wm. Crabtree, of Broughton near Manchester, who was also engaged in the same studies, and with whom a correspondence was carried on till his death. Thus encouraged, young Horrox assumed new vigour, procured other books and instruments, and was pursuing his studies and observations with great assiduity, when his progress was suddenly arrested by the hand of death, the 3d of Jan. 1640, in the 22d year of his age.

What we see of his writings, in the book noticed in this article, is sufficient to show how great a loss the world had by his death. He had just finished his "Venus in Sole visa," 1639, a little before. This was published in 1668, by Hevelius, as above noticed. His other posthumous works, or rather his imperfect papers, were published by Dr. Wallis, as stated in the article above, with some account of his life; in which we find he first asserts and promotes the Keplarian astronomy against the hypothesis of Lansberg; which he proves to be inconsistent with itself, and neither agreeing with theory nor observations. He vindicates Tycho Brahe from some objections made to his hypothesis, and gives a new theory of the moon; to which are added the lunar numbers of Mr. Flamsteed. There are also extracts from several letters between him and Mr. Crabtree, on various astronomical matters; with a catalogue of astronomical observations.

There are two things particularly which will perpetuate the memory of this very extraordinary young man. The one is, that he was the first person that ever predicted or saw the planet Venus in the sun. Though he was not aware of the great use that was to be made of it, in discovering the parallax and distance of the sun and planets, yet he made from it many useful observations, corrections, and improvements in the theory of the motions of Venus. Secondly, his new theory of lunar motions; which Newton himself made the ground work of all his astronomy, relative to the moon; and who always spoke of our author as a genius of the first order.

Diatriba, Numerisque Lunaribus ad novum Lunæ Systema Horroccii. Lond. An. 1672, in 4to.

Mr. Horrox was also the author of that excellent tract, called Venus in Sole visa, published by Hevelius, together with his Mercurius in Sole visus: who if he had not been snatched away by an untimely death in the flower of his age, would by his industry and exactness, which accompanied his great affection for astronomy, have very considerably advanced that science. Now we have only left us these imperfect papers, digested with great care and labour, by that learned mathematician Dr. John Wallis.

III. Marcelli Malpighii Phil. et Medici Bononiensis Dissertatio Epistolica de Formatione Pulli in Ovo: Lond. 1672, in 4to.

This exercitation of Signor Malpighi, contains an account of his having discovered, that in fecund eggs, as well before as after incubation, the first rudiments and lines of the principal parts of the chick are contained in the eggs; whereas in subventaneous or addle eggs, instead of such a formation, there is found nothing but an unformed globous ash-coloured body, like a mola. Of these prima stamina or beginnings, this author has traced the progress, by observing their changes, after incubation, every six hours, for the first two days; and after that, every 12 or 24, or 48 hours. In doing which, he has observed many very curious and remarkable particulars, especially about the priority of the motion of the heart before the production of true blood, though that liquor, before it becomes red, be, according to him, before the motion of the heart; as also about the said liquor first emerging, viz. whether it be a simple colliquamentum, or a liquor vitalis, or a sanguis inchoatus: concerning which he asserts, that the carina, and the beginnings of the head, brain, and spinal marrow, do manifestly appear before the collection of that liquor, and its motion, and change into the nature of blood.

IV. De Mente Humana Libri quatuor, &c. Auth. J. B. du Hamel P. S. L. Par. An. 1672, in 12mo.

The learned author of this book here treats of the nature, powers, functions, and immortality of the soul.

Mr. Isaac Newton's Answer to some Considerations on his Doctrine of Light and Colours: as printed in Number 80 *of these Tracts.* N° 88, p. 5084.

At the perusal of the considerations you sent me, on my letter concerning refractions and colours, I found nothing that, as I conceived, might not without difficulty be answered. And though I find the considerer somewhat more concerned for an hypothesis than I expected; yet I doubt not but we have one common design; I mean a sincere endeavour after knowledge, without valuing uncertain speculations for their subtleties, or despising certainties for their plainness: and on confidence of this I make this return to his discourse.*

The first thing that offers itself is less agreeable to me, and I begin with it because it is so. The considerer† is pleased to reprehend me for laying aside the thoughts of improving optics by refractions. If he had obliged me by a private letter on this occasion, I would have acquainted him with my successes on the trials I have made of that kind, which I shall now say have been less than I sometimes expected, and perhaps than he at present hopes for. But since he is pleased to take it for granted, that I have let this subject pass without due examination, I shall refer him to my former letter,‡ by which that conjecture will appear to be ungrounded. For, what I said there, was in respect of telescopes of the ordinary construction, signifying, that their improvement is not to be expected from the well-figuring of glasses, as opticians have imagined; but I despaired not of their improvement by other constructions; which made me cautious to insert nothing that might intimate the contrary. For, although successive refractions that are all made the same way, do necessarily more and more augment the errors of the first refraction; yet it seemed not impossible for contrary refractions so to correct each others inequalities, as to make their difference regular; and, if that could be conveniently effected, there would be no further difficulty. Now to this end I examined what may be done, not only by glasses alone, but more especially by a complication of divers successive mediums, as by two or more glasses or crystals, with water or some other fluid between them; all which together may perform the office of one glass, especially of the object-glass, on whose construction the perfection of the instrument chiefly depends. But what the results in theory or by trials have been, I may possibly find a more proper occasion to declare.

To the assertion, that rays are less truly reflected to a point by a concave, than refracted by a convex, I cannot assent; nor do I understand, that the focus of the latter is less a line than that of the former. The truth of the contrary you will rather perceive by this following table, computed for such a reflecting concave, and refracting convex, on supposition that they have equal apertures, and collect parallel rays at an equal distance from their vertex; which distance being divided into 15000 parts, the diameter of the concave sphere will be 60000 of

* Which discourse was thought needless to be here printed at length, because in the body of this answer are to be met with the chief particulars, wherein the answerer was concerned. *(Original.)*

† Mr. Hook.

‡ Printed in Number 80, of these tracts.

those parts, and of the convex 10000; supposing the sines of incidence and refraction to be, in round numbers, as 2 to 3. And this table shows, how much the exterior rays, at several apertures, fall short of their principal focus.

The Diameter of the Aperture.	The parts of the Axis intercepted between the vertex and the rays Reflected,	Refracted.	The Error by Reflection,	Refraction.
2000	$14991\frac{2}{3}$	14865	$8\frac{1}{3}$	135
4000	14966	14449	33	551
6000	14924	13699	76	1301
8000	14865	12475	135	2525
10000	14787	9472	213	5528

By this you may perceive, that the errors of the refracting convex are so far from being less, that they are more than sixteen times greater than the like errors of the reflecting concave, especially in great apertures; and that without respect to the heterogeneous constitution of light. So that, however the contrary supposition might make the author of these animadversions reject reflections, as useless for the promoting of optics; yet I must for this, as well as other considerations, prefer them in the theory, before refractions.

Whether the parabola be more difficult to describe than the hyperbola or ellipsis, may be a quære: but I see no absolute necessity for endeavouring after any of their descriptions. For, if metals can be ground truly spherical, they will bear as great apertures, as I believe men will be able to communicate an exact polish to. And for dioptric telescopes, I told you, that the difficulty consisted not in the figure of the glass, but in the difformity of refractions: which if it did not, I could tell you a better and more easy remedy than the use of the conic sections.

Thus much concerning the practical part of optics. I shall now take a view of the considerations on my theories. And those consist in ascribing an hypothesis to me which is not mine; in asserting an hypothesis, which, as to the principal parts, is not against me; in granting the greatest part of my discourse if explicated by that hypothesis; and in denying some things, the truth of which would have appeared by an experimental examination.

Of these particulars I shall discourse in order. And first of the hypothesis, which is ascribed to me in these words: "But grant his first supposition, that light is a body, and that as many colours or degrees as there may be, so many bodies there may be; all which compounded together would make white, &c." This it seems is taken for my hypothesis. It is true that from my theory I argue the corporeity of light; but I do it without any absolute positiveness, as the word perhaps intimates; and make it at most but a very plausible consequence of the doctrine, and not a fundamental supposition, nor so much as any part of it; which was wholly comprehended in the precedent propositions. And I somewhat wonder how the objector could imagine, that when I had asserted the theory with the greatest rigour, I should be so forgetful as afterwards to assert the fundamental supposition itself with no more than a perhaps. Had I intended any such hypothesis I should somewhere have explained it. But I knew that the properties which I declared of light, were in some measure capable of being explicated, not only by that, but by many other mechanical hypotheses. And therefore I chose to decline them all, and to speak of light in general terms, considering it abstractly, as something or other propagated every way in straight lines from luminous bodies, without determining what that thing is; whether a confused mixture of difform qualities, or modes of bodies, or of bodies themselves, or of any virtues, powers, or beings whatsoever. And for the same reason I chose to speak of colours according to the information of our senses, as if they were qualities of light without us. Whereas by that hypothesis I must have considered them rather as modes of sensation, excited in the mind by various motions, figures, or sizes of the corpuscles of light, making various mechanical impressions on the organ of sense; as I expressed it in that place, where I spake of the corporeity of light.

But supposing I had propounded that hypothesis, I understand not, why the objector should so much endeavour to oppose it. For certainly it has a much greater affinity with his own hypothesis, than he seems to be aware of; the vibrations of the æther being as useful and necessary in this as in his. For assuming the rays of light to be small bodies, emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the æther, as stones do in water when thrown into it. And supposing these vibrations to be of several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities; of what use they will be for explicating the manner of reflection and refraction, the production of heat by the sun beams, the emission of light from burning, putrifying, or other substances whose parts are vehemently agitated, the phænomena of thin transparent plates and bubbles, and of all natural bodies, the manner of vision, and the difference of colours, as also their harmony and discord; I shall leave to their consideration, who may think it worth their endeavour to apply this hypothesis to the solution of phænomena.

In the second place, I told you that the objector's hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is—" That the parts of bodies, when briskly agitated, do excite vibrations in the æther, which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing." Now the most free and natural application of this hypothesis, to the solution of phænomena, I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the æther of various depths or sizes, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal sizes be separated from one another, the largest beget a sensation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various sizes, which, according to those sizes, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several sizes, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction, and so cause the phænomena of prisms and other refracting substances: and that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And since the vibrations which make blue and violet, are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phænomena of those plates or bubbles, and also of all natural bodies, whose parts are like so many fragments of such plates.

These seem to be the most plain, genuine and necessary conditions of this hypothesis: and they agree so justly with my theory, that if the animadversor think fit to apply them, he need not on that account apprehend a divorce from it. But yet how he will defend it from other difficulties I know not. For to me the fundamental supposition itself seems impossible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium, where they are terminated by it. I mistake if there be not both experiment and demonstration to the contrary. And as to the other two or three hypotheses which he mentions, I had rather believe them subject to the like difficulties, than suspect the animadversor should select the worst for his own.

What I have said of this, may be easily applied to all other mechanical hypotheses, in which light is supposed to be caused by any pression or motion whatsoever, excited in the æther by the agitated parts of luminous bodies. For it seems impossible that any of those motions or pressions can be propagated in straight lines without the like spreading every way into the shadowed medium on which they border. But yet, if any man can think it possible, he must at least allow, that those motions, or endeavours to motion, caused in the æther by the several parts of any lucid body, that differ in size, figure, and agitation, must necessarily be unequal: which is enough to denominate light an aggregate of difform rays, according to any of these hypotheses. And if those original inequalities may suffice to differ the rays in colour and refrangibility, I see no reason why they that adhere to any of those hypotheses, should seek for other causes of these effects, unless (to use the objector's argument) they will multiply entities without necessity.

The third thing to be considered, is the conditions of the animadversor's concessions, which is, that I would explicate my theories by his hypothesis: and if I could comply with him in that point, there would be little or no difference between us. For he grants, that without any respect to a different incidence of rays there are different refractions; but he would have it explicated, not by the different refrangibility of several rays, but by the splitting and rarefying of æthereal pulses. He grants my third, fourth, and sixth propositions; the sense of which is, that uncompounded colours are unchangeable, and that compounded ones are changeable only by resolving them into the colours of which they are compounded; and that all the changes, which can be wrought in colours, are effected only by variously mixing or parting them: but he grants them on condition that I will explicate colours by the two sides of a split pulse, and so make but two species of them, accounting all other colours in the world to be but various degrees and dilutings of those two. And he further grants, that whiteness is produced by the convention of colours; but then I must allow it to be not only by mixture of those colours, but by a farther uniting of the parts of the ray supposed to be formerly split.

If I would proceed to examine these his explications, I think it would be no difficult matter to show that they are not only insufficient, but in some respects, to me at least, unintelligible. For though it be easy to conceive how motion may be dilated and spread, or how parallel motions may become diverging; yet I understand not by what artifice any linear motion can, by a refracting superficies be infinitely dilated and rarefied, so as to become superficial; or if that be supposed, yet I understand as little, why it should be split at so small an angle only, and not rather spread and dispersed through the whole angle of refraction. And further, though I can easily imagine how unlike motions may cross one another; yet I cannot well conceive how they should coalesce into one uniform motion, and then part again, and recover their former unlikeness; notwithstanding that I conjecture the ways by which the animadversor may endeavour to explain it. So that the direct, uniform and undisturbed pulses should be split and disturbed by refraction; and yet the oblique and disturbed pulses persist without splitting or further disturbance by following refractions, is to me as unintelligible. And there is as great a difficulty in the number of colours, as you will see hereafter.

But whatever be the advantages or disadvantages of this hypothesis, I hope I may be excused from taking it up, since I do not think it needful to explicate my doctrine by any hypothesis at all. For if light be considered abstractedly without respect to any hypothesis, I can as easily conceive, that the several parts of a shining body may emit rays of different colours and other qualities, of all which light is constituted, as that the several parts of a false or uneven string, or of unevenly agitated water in a brook or cataract, or the several pipes of an organ inspired all at once, or all the variety of sounding bodies in the world together, should produce sounds of several tones, and propagate them through the air confusedly intermixt. And, if there were any natural bodies that could reflect sounds of one tone, and stifle or transmit those of another; then, as the echo of a confused aggregate of all tones would be that particular tone, which the echoing body is disposed to reflect; so, since, even by the animadversor's concessions, there are bodies apt to reflect rays of one colour, and stifle or transmit those of another; I can as easily conceive that those bodies, when illuminated by a mixture of all colours, must appear of that colour only which they reflect.

But when the objector would insinuate a difficulty in these things, by alluding to sounds in the string of a musical instrument before percussion, or in the air of an organ bellows before its arrival at the pipes; I must confess, I understand it as little, as if one had spoken of light in a piece of wood before it be set on fire, or in the oil of a lamp before it ascend up the match to feed the flame.

You see therefore, how much it is beside the business in hand, to dispute about hypotheses. For which reason I shall now in the last place proceed to abstract the difficulties in the animadversor's discourse, and, without having regard to any hypothesis, consider them in general terms. And they may be reduced to these 3 quæries:

1. Whether the unequal refractions made without respect to any inequality of incidence, be caused by the different refrangibility of several rays; or by the splitting, breaking or dissipating the same ray into diverging parts?

2. Whether there be more than two sorts of colours?

3. Whether whiteness be a mixture of all colours?

The first of these quæries you may find already determined by an experiment in my former letter; the design of which was to show, that the length of the coloured image proceeded not from any unevenness in the glass, or any other contingent irregularity in the refractions. Amongst other irregularities, I know not what is more obvious to suspect, than a fortuitous dilating and spreading of light, after some such manner as Descartes has described in his æthereal refractions, for explicating the tail of a comet; or as the animadversor now supposes to be effected by the splitting and rarefying of his æthereal pulses. And to prevent the suspicion of any such irregularities, I told you that I refracted the light contrarywise with two prisms successively, to destroy thereby the regular effects of the first prism by the second, and to discover the irregular effects by augmenting them with iterated refractions. Now, amongst other irregularities, if the first prism had spread and dissipated every ray into an indefinite number of diverging parts, the second should in like manner have spread and dissipated every one of those parts into a further indefinite number, whereby the images would have been still more dilated, contrary to the event. And this ought to have happened, because those linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly and completely rays as the whole was, before its incidence; as may appear by intercepting them severally.

The reasonableness of this proceeding will perhaps better appear by acquainting you with this further circumstance. I sometimes placed the second prism in a position transverse to the first, on design to try, if it would make the long image become four-square, by refractions crossing those that had drawn the round image into a long one. For if, amongst other irregularities, the refraction of the first prism did, by splitting, dilate a linear ray into a superficial, the cross refractions of that second prism ought, by further splitting, to dilate and draw that superficial ray into a pyramidal solid. But, upon trial, I found it otherwise; the image being as regularly oblong as before, and inclined to both the prisms at an angle of 45 degrees.

I tried also all other positions of the second prism, by turning the ends about its middle part; and in no case could observe any such irregularity. The image was ever alike inclined to both prisms, its breadth answering to the sun's diameter, and its length being greater or less accordingly as the refractions more or less agreed, or contradicted one another.

And by these observations, since the breadth of the image was not augmented by the cross refraction of the second prism, that refraction must have been performed without any splitting or dilating of the ray; and therefore at least the light incident on that prism, must be granted an aggregate of rays unequally refrangible in my sense. And since the image was equally inclined to both prisms, and consequently the refractions alike in both, it argues that they were performed according to some constant law without any irregularity.

To determine the second quære, the animadversor refers to an experiment, made with two wedge-like boxes, recited in the Micrography of the ingenious Mr. Hook, observation 10, page 73; the design of which was, to produce all colours out of a mixture of two. But there is, I conceive, a double defect in this instance. For it appears not, that by this experiment all colours can be produced out of two; and, if they could, yet the inference would not follow.

That all colours cannot by that experiment be produced out of two, will appear by considering, that the tincture of aloes, which afforded one of those colours, was not all over of one uniform colour, but appeared yellow near the edge of the box, and red at other places where it was thicker: affording all variety of colours from a pale yellow to a deep red or scarlet, according to the various thickness of the liquor. And so the solution of copper, which afforded the other colour, was of various blue and indigoes. So that, instead of two colours, here is a great variety made use of for the production of all others. Thus, for instance, to produce all sorts of greens, the several degrees of yellow and pale blue must be mixed; but to compound purples, the scarlet and deep blue are to be the ingredients.

Now if the animadversor contend, that all the reds and yellows of the one liquor, or blue and indigoes of the other, are only various degrees and dilutings of the same colour, and not divers colours, that is a begging of the question: And I should as soon grant, that the two thirds or sixths in music are but several degrees of the same sound, and not divers sounds. Certainly it is much better to believe our senses, informing us, that red and yellow are divers colours, and to make it a philosophical quære, why the same liquor does, according to its various thickness, appear of those divers colours, than to suppose them to be the same colour because exhibited by the same liquor? For, if that were a sufficient reason, then blue and yellow must also be the same colour, since they are both exhibited by the same tincture of nephritic wood. But that they are divers colours, you will more fully understand by the reason, which, in my judgment is this: The tincture of aloes is qualified to transmit most easily the rays indued with red, most difficultly the rays indued with violet, and with intermediate degrees of facility the rays indued with intermediate colours. So that where the liquor is very thin, it may suffice to intercept most of the violet, and yet transmit most of the other colours; all which together must compound a middle colour, that is a faint yellow. And where it is so much thicker, as also to intercept most of the blue and green, the remaining green, yellow, and red, it must compound an orange. And where the thickness is so great, that scarcely any rays can pass through it besides those indued with red, it must appear of that colour, and that so much the deeper and obscurer, by how much the liquor is thicker. And the same may be understood of the various degrees of blue, exhibited by the solution of copper, by reason of its disposition to intercept red most easily, and transmit a deep blue or indigo colour most freely.

But supposing that all colours might, according to this experiment, be produced out of two by mixture; yet it follows not, that those two are the only original colours, and that for a double reason. First, because those two are not themselves original colours, but compounded of others; there being no liquor nor any other body in nature, whose colour in day-light is wholly uncompounded. And then, because, though those two were original, and all others might be compounded of them, yet it follows not that they cannot be otherwise produced. For I said that they had a double origin, the same colours to sense being in some cases compounded, and in others uncompounded; and sufficiently declared in my third and fourth propositions, and in the conclusion, by what properties the one might be known and distinguished from the other. But, because I suspect, by some circumstances, that the distinction might not be rightly apprehended, I shall once more declare it, and further explain it by examples.

That colour is primary or original, which cannot by any art be changed, and whose rays are alike refrangible: and that compounded, which is changeable into other colours, and whose rays are not alike refrangible. For instance, to know whether the colour of any green object be compounded or not, view it through a prism, and if it appear confused, and the edges tinged with blue, yellow, or any variety of other colours, then is that green compounded of such colours as at its edges emerge out of it: but if it appear distinct, and well defined, and entirely green to the very edges, without any other colours emerging, it is of an original and uncompounded green. In like manner, if a refracted beam of light, being cast on a white wall, exhibit a green colour, to know whether that be compounded, refract the beam with an interposed prism; and if you find any difformity in the refractions, and the green be transformed into blue, yellow, or any variety of other colours, you may conclude that it was compounded of those which emerge: but if the refractions be uniform, and the green persist without any change of colour, then is it original and uncompounded. And the reason why I call it so, is, because a green indued with such properties cannot be produced by any mixing of other colours.

Now, if two green objects may to the naked eye appear of the same colour, and yet one of them through a prism seem confused and variegated with other colours at the edges, and the other distinct and entirely green; or, if there may be two beams of light, which falling on a white wall, do to the naked eye exhibit the same green colour, and yet one of them, when transmitted through a prism, be uniformly and regularly refracted, and retain its colour unchanged, and the other be irregularly refracted and made to divaricate into a multitude of other colours; I suppose these two greens will in both cases be granted of a different origin and constitution. And if by mixing colours, a green cannot be compounded with the properties of the unchangeable green, I think I may call that an uncompounded colour, especially since its rays are alike refrangible, and uniform in all respects.

The same rule is to be observed in examining, whether red, orange, yellow, blue, or any other colour, be compounded or not. And, by the way, since all white objects through the prism appear confused, and terminated with colours, whiteness must according to this distinction, be ever compounded, and that the most of all colours, because it is the most confused and changed by refractions.

From hence I may take occasion to communicate a way for the improvement of microscopes by refraction. The way is, by illuminating the object in a darkened room with light of any convenient colour not too much compounded: for by that means the microscope will, with distinctness, bear a deeper charge and larger aperture, especially if its construction be such as I may hereafter describe; for the advantage in ordinary microscopes will not be so sensible.

There remains now the third quære to be considered, which is, whether whiteness be an uniform colour, or a dissimilar mixture of all colours? The experiment which I brought to decide it, the animadversor thinks may be otherwise explained, and so concludes nothing. But he might easily have satisfied himself by trying what would be the result of a mixture of all colours. And that very experiment might have satisfied him, if he had pleased to examine it by the various circumstances. One circumstance I there declared, of which I see no notice taken; and it is, that if any colour at the lens be intercepted, the whiteness will be changed into the other colours: if all the colours but red be intercepted, that red alone, in the concourse or crossing of the rays, will not constitute whiteness, but continues as much red as before; and so of the other colours. So that the business is not only to show, how rays, which before the concourse exhibit colours, do in the concourse exhibit white; but to show, how in the same place, where the several sorts of rays apart exhibit several colours, a confusion of all together make white. For instance, if red alone be first transmitted to the paper at the place of concourse, and then the other colours be let fall on that red, the question will be, whether they convert it into white, by mixing with it only, as blue falling on yellow light is supposed to compound green; or, whether there be some further change wrought in the colours by their mutual acting on one another, until like contrary peripatetic qualities, they become assimilated. And he that shall explicate this last case mechanically, must conquer a double impossibility. He must first show, that many unlike motions in a fluid can by clashing so act on one another, and change each other, as to become one uniform motion; and then, that a uniform motion can of itself, without any new unequal impressions, depart into a great variety of motions regularly unequal. And after this he must further tell me, why all objects appear not of the same colour, that is, why their colours in the air, where the rays that convey them every way are confusedly mixed, do not assimilate one another and become uniform, before they arrive at the spectator's eye?

But if there be yet any doubting, it is better to put the event on further circumstances of the experiment, than to acquiesce in the possibility of any hypothetical explication. As, for instance, by trying, what will be the apparition of these colours in a very quick consecution of one another. And this may be easily performed by the rapid gyration of a wheel with many spokes or cogs in its perimeter, whose interstices and thicknesses may be equal, and of such a size, that, if the wheel be interposed between the prism and the white concourse of the colours, one half of the colours may be intercepted by a spoke or cog, and the other half pass through an interstice. The wheel being in this posture, you may first turn it slowly about, to see all the colours fall successively on the same place of the paper, held at their aforesaid concourse; and if you then accelerate its gyration, until the consecution of those colours be so quick, that you cannot distinguish them severally, the resulting colour will be a whiteness perfectly like that, which an unrefracted beam of light exhibits, when in like manner successively interrupted by the spokes or cogs of that circulating wheel. And that this whiteness is produced by a successive intermixture of the colours, without their being assimilated, or reduced to any uniformity, is certainly beyond all doubt, unless things that exist not at the same time may notwithstanding act on one another.

There are yet other circumstances by which the truth might have been decided, as by viewing the white concourse of the colours through another prism placed close to the eye, by whose refraction that whiteness may appear again transformed into colours; and then, to examine their origin, if an assistant intercept any of the colours at the lens before their arrival at the whiteness, the same colours will vanish from amongst those, into which that whiteness is converted by the second prism. Now if the rays which disappear be the same with those that are intercepted, then it must be acknowledged, that the second prism makes no new colours in any rays which were not in them before their concourse at the paper. Which is a plain indication, that the rays of several colours remain distinct from one another in the whiteness, and that from their previous dispositions are derived the colours of the second prism. And, by the way, what is said of their colours may be applied to their refrangibility.

The aforesaid wheel may be also here made use of; and, if its gyration be neither too quick nor too slow, the succession of the colours may be discerned through the prism, whilst to the naked eye of a bystander they exhibit whiteness.

There is something still remaining to be said of this experiment. But this, I conceive, is enough to enforce it, and so to decide the controversy. However, I shall now proceed to show some other ways of producing whiteness by mixtures, since I persuade myself that this assertion, above the rest, appears paradoxical, and is with most difficulty admitted. And because the animadversor desires an instance of it in bodies of divers colours, I shall begin with that. But in order thereto it must be considered, that such coloured bodies reflect but some part of the light incident on them; as is evident by the 13th proposition; and therefore the light reflected from an aggregate of them, will be much weakened by the loss of many rays. Whence a perfect and intense whiteness is not to be expected, but rather a colour between those of light and shadow, or such a grey or dirty colour as may be made by mixing white and black together.

And that such a colour will result may be collected from the colour of dust found in every corner of a house, which has been observed to consist of many coloured particles. There may be also produced the like dirty colour by mixing several painters colours together. And the same may be effected by painting a top (such as boys play with) of divers colours. For, when it is made to circulate by whipping it, it will appear of such a dirty colour.

Now the compounding of these colours is proper to my purpose, because they differ not from whiteness in the species of colour, but only in degree of luminousness; which, did not the animadversor concede it, I might thus evince.

A beam of the sun's light being transmitted into a darkened room, if you illuminate a sheet of white paper by that light, reflected from a body of any colour, the paper will always appear of the colour of that body by whose reflected light it is illuminated. If it be a red body, the paper will be red; if a green body, it will be green, and so of the other colours. The reason is, that the fibres or threads, of which the paper consists, are all transparent and specular; and such substances are known to reflect colours without changing them. To know, therefore, to what species of colour a grey belongs, place any grey body (suppose a mixture of painters colours) in the said light, and the paper, being illuminated by its reflection, shall appear white. And the same thing will happen, if it be illuminated by reflection from a black substance.

These therefore are all of one species; but yet they seem distinguished, not only by degrees of luminousness, but also by some other inequalities, whereby they become more harsh or pleasant. And the distinction seems to be, that greys and perhaps blacks are made by an uneven defect of light, consisting as it were of many little veins or streams, which differ either in luminousness, or in the unequal distribution of diversely coloured rays; such as ought to be caused by reflection from a mixture of white and black, or of diversely coloured corpuscles. But when such imperfectly mixed light is by a second reflection from the paper more evenly and uniformly blended, it becomes more pleasant, and exhibits a faint or shadowed whiteness. And that such little irregularities as these may cause these differences is not improbable, if we consider how much variety may be caused in sounds of the same tone by irregular and uneven jarrings. And besides, these differences are so little that I have sometimes doubted, whether they be any at all, when I have considered that a black and white body being placed together, the one in a strong light and the other in a very faint light, so proportioned that they might appear equally luminous, it has been difficult to distinguish them, when viewed at a distance, unless when the black seemed more bluish, and the white body in a light still fainter, has in comparison of the black body, itself appeared black.

This leads me to another way of compounding whiteness, which is, that if four or five bodies of the more eminent colours, or a paper painted all over, in several parts of it, with those several colours in a due proportion, be placed in the said beam of light, the light reflected from those colours to another white paper, held at a convenient distance, shall make that paper appear white. If it be held too near the colours, its parts will seem of those colours that are nearest them; but by removing it further that all its parts may be equally illuminated by all the colours, they will be more and more diluted, until they become perfectly white. And you may further observe, that if any of the colours be intercepted, the paper will no longer appear white, but of the other colours which are not intercepted. Now, that this whiteness is a mixture of the severally coloured rays, falling confusedly on the paper, I see no reason to doubt; because if the light became uniform and similar before it fell confusedly on the paper, it must much more be uniform when at a greater distance it falls on the spectator's eye, and so the rays, which come from several colours, would in no qualities differ from one another, but all of them exhibit the same colour to the spectator, contrary to what he sees.

Not much unlike this instance it is, that if a polished piece of metal be so placed, that the colours appear in it as in a looking-glass, and then the metal be made rough, that by a confused reflection those apparent colours may be blended together, they shall disappear, and by their mixture cause the metal to look white.

But further to enforce this experiment, if instead of the paper, any white froth, consisting of small bubbles, be illuminated by reflection from the aforesaid colours, it shall to the naked eye seem white, and yet through a good microscope the several colours will appear distinct on the bubbles, as if seen by reflection from so many spherical surfaces. With my naked eye, being very near, I have also discerned the several colours on each bubble: and at a greater distance, where I could not distinguish them apart, the froth has appeared entirely white. And at the same distance, when I looked intently, I have seen the colours distinctly on each bubble; and yet by straining my eyes as if I would look at something far off beyond them, thereby to render the vision confused, the froth has appeared without any other colour than whiteness. And what is here said of froths may easily be understood of the paper or metal in the foregoing experiments. For their parts are specular bodies, like these bubbles; and perhaps with an excellent microscope the colours may be also seen intermixedly reflected from them.

In proportioning the severally coloured bodies to produce these effects, there may be some niceness; and it will be more convenient to make use of the colours of the prism, cast on a wall, by whose reflection the paper, metal, froth, and other white substances, may be illuminated. And I usually made my trials this way, because I could better exclude any scattering light from mixing with the colours to dilate them.

To this way of compounding whiteness may be referred that other, by mixing light after it has been trajected through transparently coloured substances. For instance, if no light be admitted into a room but only through coloured glass, whose several parts are of several colours in a pretty equal proportion; all white things in the room shall appear white, if they be not held too near the glass.

And yet this light, with which they are illuminated, cannot possibly be uniform, because if the rays, which at their entrance are of divers colours, do in their progress through the room suffer any alteration to be reduced to a uniformity; the glass would not in the remotest parts of the room appear of the very same colour, which it does when the spectator's eye is very near it: nor would the rays, when transmitted into another dark room through a little hole in an opposite door or partition wall, project on a paper the species or representation of the glass in its proper colours.

And by the by, this seems a very fit and cogent instance of some other parts of my theory, and particularly of the 13th proposition. For in this room all natural bodies whatever appear in their proper colours. And all the phænomena of colours in nature, made either by refraction or without it, are here the same as in the open air. Now the light in this room being such a dissimilar mixture, as I have described in my theory, the causes of all these phænomena must be the same that I have there assigned. And I see no reason to suspect, that the same phænomena should have other causes in the open air.

The success of this experiment may easily be conjectured by the appearances of things in a church or chapel, whose windows are of coloured glass; or in the open air, when it is illustrated with clouds of various colours.

There are yet other ways by which I have produced whiteness; as by casting several colours from two or more prisms upon the same place; by refracting a beam of light with two or three prisms successively, to make the diverging colours converge again; by reflecting one colour to another; and by looking through a prism on an object of many colours; and (which is equivalent to the above-mentioned way of mixing colours by concave wedges filled with coloured liquors,) I have observed the shadows of a painted glass window to become white, where those of many colours have at a great distance interfered. But yet, for further satisfaction, the animadversor may try, if he please, the effects of four or five of such wedges, filled with liquors of as many several colours.

Besides all these, the colours of water bubbles, and other thin pellucid substances, afford several instances of whiteness produced by their mixture; with one of which I shall conclude this particular. Let some water, in which a convenient quantity of soap or wash-ball is dissolved, be agitated into froth, and, after that froth has stood a while without further agitation, till you see the bubbles of which it consists begin to break, there will appear a great variety of colours all over the top of every bubble, if you view them near at hand; but if you view them at so great a distance, that you cannot distinguish the colours one from another, the froth will appear perfectly white.

Thus much concerning the design and substance of the animadversor's considerations. There are yet some particulars to be taken notice of before I conclude, as the denial of the Experimentum Crucis. On this I chose to lay the whole stress of my discourse; which therefore was the principal thing to have been objected against. But I cannot be convinced of its insufficiency by a bare denial, without assigning a reason for it. I am apt to believe it has been misunderstood; for otherwise it would have prevented the discourses about rarefying and splitting of rays: because the design of it is to show, that rays of divers colours considered apart, do at equal incidences suffer unequal refractions, without being split, rarefied, or any ways dilated.

In the considerations of my first and second propositions, the animadversor has rendered my doctrine of unequal refrangibility very imperfect and maimed, by explicating it wholly by the splitting of rays; whereas I chiefly intended it in those refractions that are performed without that supposed irregularity; such as the Experimentum Crucis might have informed him of. And in general I find, that whilst he has endeavoured to explicate my propositions hypothetically, the more material suggestions by which I designed to recommend them, have escaped his consideration; such as are, the unchangeableness of the degree of refrangibility peculiar to any sort of rays; the strict analogy between the degrees of refrangibility and colours; the distinction between compounded and uncompounded colours; the unchangeableness of uncompounded colours; and the assertion, that if any one of the prismatic colours be wholly intercepted, that colour cannot be new produced out of the remaining light by any further refraction or reflection whatsoever. And of what strength and efficacy these particulars are for enforcing the theory, I desire therefore may be now considered.

An Account of two Books. N° 88, p. 5103.

I. Ottonis de Guericke * Experimenta Nova Magdebrurgica, de Vacuo Spatio, &c. Ams. An. 1672, in fol.

* Otto or Otho Guericke, counsellor to the Elector of Brandenbourg, and burgomaster of Magdebourg, was born in 1602, and died at Hambourg in 1686. He was one of the best philosophers of his time, and produced several useful inventions. Of these, one was the air-pump; and the two brass hemispheres, which being applied to each other, and the air drawn out, 16 horses were not able to draw them asunder; also the virunculus or marmouset, which descended in a tube against rain, and rose again on the return of serene weather; besides several others: though the last machine fell into disuse after the invention of the barometer. He was also the author of several other works on Natural Philosophy, besides that noticed in the article above, which contains his experiments on the vacuum. As Guericke made use of his marmouset to foretel storms, he was considered as a sorcerer by the people; and hence, when the thunder had one day fallen on his house, and broken to pieces some machines which he had employed in his experiments, they failed not to say it was a punishment from heaven, that was angry at his infernal dealings.

After a narrative of the chief hypotheses and opinions of both ancient and recent astronomers, concerning the system of the world, and having represented the great difficulties in the Ptolemaic and Tychonic, and repeated the answers to the objections against the Copernican; the author at large gives us his own thoughts of the frame and constitution of the world; by which world he understands, in his treatise, the complex of the planets, disposed and ordered much after the Copernican way, the sun being seated in the midst, having his spots about him, and moving and influencing all the rest of the planets according to their several distances from him; Saturn making the utmost of all the planets, and the end of this his world being where the diffusive power and virtue of the sun, the king and governor of them all, terminates, which bounds he conjectures to extend themselves, beyond Saturn, to those fixed stars that are of the nearer rank to Saturn's orb.

Concerning the bodies lodged in these planets, he thinks it consonant to the power and wisdom of the Great Creator, that there should be such a variety of them, as to stock each of the said planets with creatures differing from those of others: so that nothing of what is in *(e. g.)* our earth or terraqueous globe, is to be found in any of the other planets, but that every one of them is stored with peculiar creatures, and even with such reasonable ones, as are of another kind from the men of our earth.

In the body of the work, many experiments, contrivances, and effects, are described; in which, however, the ingenious author has been, in many instances, preceded by our illustrious Boyle.

One curious experiment, in which he seems to be singular, may be here noticed, being an early instance of electricity, though he seemed to have some other mysterious ideas about it.

By which experiment he thinks may be represented the chief virtues he enumerates of our earth, performed by a globe of sulphur melted and cooled again, and then perforated, to traject an iron axis through it for circumvolution; whereby attrition being used withal, he affirms that the impulsive, attractive, expulsive, and other virtues of the earth, as he calls them, may be ocularly exhibited.

II. Thesaurus Medicinæ Practicæ; studio et operâ Thomæ Burnet Scoto Britanni, M. D. et Medici Regis Ordinarii. Lond. 1672, in 4to.

A compilation from the ancients, and the principal medical writers up to the 17th century.

Some Observations about Shining Flesh, made by Mr. Boyle, March 15, 1671-72, *in a Letter to the Editor.* N° 89, p. 5108.

Mr. Boyle observed a neck of veal to shine in as many as 20 places, though not all alike, as rotten wood or stinking fish do. The size of these lucid parts was considerably different; nor were their figures uniform; some being round, others almost oval, but for the most part very irregularly shaped. The parts that shone most, were some gristly or soft parts of the bones, where the butcher's cleaver had passed; though they were not the only parts that were luminous; for by moving to and fro the spinal marrow, a part of it emitted light; and he perceived one place in a tendon to afford some light; and lastly, three or four spots in the fleshy parts, at a good distance from the bones, were plainly discovered by their own light, though fainter than in the above-mentioned parts.—When all these lucid parts were surveyed at once, they made a very splendid show; so that applying a printed paper to some of the more luminous spots, he could plainly read divers letters of the title.—The colour of the light was not the same in each, but in those that shone liveliest, it seemed to have such a fine greenish blue as is observed in the tails of glow worms. But notwithstanding the vividness of this light, it did not yield the least degree of heat to the touch; and applying to the most shining places a sealed weather-glass, the tinged spirit of wine was not observed to be sensibly affected; and notwithstanding the great number of lucid parts, not the least degree of stench was perceivable to infer any putrefaction; the meat being judged very fresh and well conditioned, and fit to be dressed.—The floor of the larder where this meat was kept, was almost a story lower than the level of the street, and divided from the kitchen by a partition of boards only, and with one small window looking northward.—The wind, as far as could be observed, was then south-west, and pretty high. The air by the thermometer appeared hot for the season, which was March, the moon was past its last quarter; the mercury in the barometer stood at $29\frac{1}{10}$ inches. One of the luminous parts, which proved to be a tender bone, and of the thickness of a half crown piece, appeared to shine on both sides, though not equally; and the part of the bone whence this had been cut off, was seen to shine, but not near so vividly as the part taken off did before. It yielded no luminous juice, or moist substance, as the tails of glow-worms do; upon compressing a piece of the luminous flesh between two pieces of glass, its light was not extinguished: and putting a luminous piece into a crystalline phial, and pouring on it a little pure spirit of wine, and shaking them together, in about a quarter of an hour or less, the light was vanished. But water could not so easily destroy this light; for putting one of the pieces into a china cup, almost full of cold water, the light did not only appear through that liquor, but above an hour after it was vigorous enough not to be eclipsed by being looked on at no great distance from a burning candle. On conveying one of the larger luminous pieces into a small receiver, the pump was plied in the dark, and on the gradual removal of the air, there was perceived a gradual diminution of the light, though it never quite disappeared, as the light of rotten wood and glow worms were observed to do; but by the hasty increase of light, that disclosed itself in the veal upon admitting the air into the exhausted receiver, it appeared that the decrement, though but slowly made, had been considerable. This experiment was once more repeated with the like success; which, though it was a proof that this luminous matter was more vigorous, or tenacious, than that of most other shining bodies, yet there remained some doubt, whether the light would not have been much more impaired, if not made to vanish quite, if the subject of it could have been kept long enough in the exhausted receiver.—It was also found, that a leg of the same veal had some shining places in it, though but very few and faint, in comparison of those that were conspicuous in the above mentioned neck.—A luminous piece of it included in a phial, after three days shone as vigorously as ever; the 4th day its light was also conspicuous, so that it could be seen even in the day time, in a dark corner of the room; but before the ensuing night the light began to decay, and the offensive smell to grow somewhat strong; which seems to argue, that the disposition, by which the veal became luminous, may very well consist, both with its being, and not being, in a state of putrefaction; and consequently is not likely to be derived from the one or the other. The 5th day, in the morning, looking upon it before the curtains were opened, it seemed to be more luminous than the preceding day; the same night it was manifest enough in the dark, though not vivid. The 6th day, in the morning after sun-rising, there was within the curtains a glimmering light observable; but the seventh day, late at night, no light at all was discernible.—Some time after a pullet was observed to shine in four or five places in the same larder; though not so large as those of the veal, yet almost as vivid; and all these luminous parts were upon or near the rump, and that which appeared most like a spark of fire, shone at the very top of that part; yet the fowl was fresh and sound.

Description of an Odd Kind of Mushroom, yielding a Milky Juice, much hotter on the Tongue than Pepper, &c. Observed by Mr. Lister. N° 89, p. 5116.

The 18th of August last, passing through Marton woods, under Pinno-moor in Craven, I found an immense number of mushrooms; some withered, and others new sprung and flourishing. They were of a large size, something larger than the ordinary red-gilled eatable mushroom or champignon, and very much of their shape, that is, with a perfectly round cap or stool, thick in flesh, and with open gills underneath; having a thick, fleshy, solid, and round foot-stalk, of about six fingers breadth high above ground, and mostly as thick as my thumb. On cutting any part of this mushroom, it bleeds exceeding freely a milk white juice, which tastes much hotter on the tongue than pepper: it is not clammy to the touch; the air does not much discolour it, nor the blade of a knife, as is usual with most vegetable juices: it became in the glass vial, I drew it into, suddenly concrete and stiff, and did in some days dry into a firm cake: it then also, when well dried, retained its fierce biting taste and white colour. Further, I observed these mushrooms to abound with fly-maggots. Also the youngest and tenderest of them, that is, such as were most juicy, were very much eaten by the grey meadow naked snail, lodging themselves within the sides of the plant. *

In another letter of Dec. 17, the same person says:—Mr. Ray returned me this Answer to my letter about the biting Mushroom: " At my return to Middleton I found a letter from you, containing the description of a mushroom discovered by you in Marton woods under Pinno-moor. I doubt not but it is that described in Joh. Bauhin, l. 40, c. 6, under the title of Fungus piperatus albus, lacteo succo turgens. Only he says, that it exceeds the champignon in size; whereas you write, that there are few of them much larger than that: he also says, that they are not so thick as that. In all other points the descriptions agree exactly. For the colour, that it is white, gills and all; for the place, that it grows in woods; and for the taste, that it is hotter than pepper. Several particulars mentioned by you, are not observed or not mentioned by him. I cannot say, that I have as yet met with this mushroom.

* The mushroom here described is the *Agaricus piperatus*. A. stipitatus, pileo planiusculo lactescente, margine deflexo, lamellis incarnato-pallidis. *Lin. sp. pl. p.* 1641. It occurs in several of the Northern parts of England, in Scotland, and probably in most parts of Europe.

An Extract from Mr. Flamsteed's Letter, written to the Editor from Derby, Nov. 16, 1672, *concerning the Appulses, calculated by him for An.* 1673 *of the Moon, and the other Planets, to Fixed Stars, with an Observation of the Planet Mars,* N° 89, p. 5118.

September last I was at Townley. The first week that I intended to have observed ♂ there with Mr. Townley, I twice observed him, but could not make two observations, as I intended, in one night. The first night after my return, I had the good fortune to measure his distances from two stars the same night: whereby I find, that his parallax was very small; certainly not 30 seconds: so that I believe the sun's parallax is not more than 10 seconds.

Having observed the distances and positions of the three stars, by which ♂ made his transit, I find, that Tycho errs five minutes at least both in the places and latitudes compared one with another. And certainly he errs as much in many others; so that the labour of M. Hevelius to rectify their places is very needful. Not that I find fault with Tycho; it is a wonder, considering how difficult it is to set plain sights to a small star, that ever he performed so much, and so well: but if M. Hevelius use not glasses in their room, I fear we shall be but where we were; and yet without this restitution we cannot expect any thing certain, much less accurate, in astronomy.

An Account of some Books. N° 89, p. 5125.

I. Prose de Signori Academici di Bologna; in Bologna, 1672, in 4to.

This is a collection of 15 Discourses, at several times made by several persons of the illustrious Academy of Bologna, published under the Presidentship of the Noble Count Valerio Zani; most of them very ingenious and learned.

II. Relation de divers Voyages Curieux, IV Partie. A Par. 1672, in fol.

The fourth part of the Curious Voyages of M. Thevenot is as yet but begun, there being only printed of it and transmitted to us two Discourses, the one containing a Portraiture of the Indians by D. Juan de Palafox, Bishop de la Puebla de los Angelos; the other a Relation of the Voyages of N. N. into the river Della Plata, and thence over land into Peru.

A Further Account of Veins in Plants, &c. by Mr. Lister, in a Letter of Jan. 8, 1672-73. N° 90, p. 5132.

We have formerly given you certain reasons for the existence of veins, analogous to those in animals, in all plants whatsoever, mushrooms not excepted.—To which we might add, that the skin of a plant may be cut sheer off, with part of the spongy parenchyma, and no signs of milky juice follow, that is, no breach of a vein. Again, we have stripped the plant of its skin, by pulling it up by the roots, and exposing it to the wet weather, until it became flaccid as a wet thong, without any injury to the veins, which yet upon incision would freshly bleed. These experiments make against the general opinion of one only sap loosely pervading the whole plant, like water in a sponge. And though we have made these, and many other experiments, to facilitate an ocular demonstration of these veins; yet we have not been able to effect it to our mind, and subject them as nakedly to our eye as we could wish.

In the transverse cuts of plants, we see as it were a certain order and number of the bloody orifices of dissected veins. We observe also in a leaf, which we take to be the simplest part of a plant; 1. That the veins, &c. keep company with the ribs and nerves, as we vulgarly call them, and are distributed into all the parts of the leaf, according to the subdivisions of those nervous lineaments, and are disposed with them into a certain net-work.—2. That in a transverse cut of a leaf, the middle fibre or nerve, for example, seems to yield one large drop of a milky juice, springing as it were from one vein; yet the microscope plainly shows us, that there are many veins which contribute to the making up of that drop.—3. That if a fibre or nerve be carefully taken out of the leaf, the veins will appear in it like so many small hairs or pipes, running along and stripping the nerve.—4. That those many veins are all of an equal largeness, for ought we have yet discerned to the contrary.—5. That though we seem to be more certain of the ramifications of the fibres, wherein those veins are, we yet are not so, that those veins do any where grow less and smaller, though probably it may be so.—6. That we cannot discern any where, throughout the whole plant, larger or more capacious veins, than those we see adhering to the fibres of the leaves; which do also appear from comparing the bleeding orifices in a transverse cut. I have found it a difficult and laborious task, to trace and unravel them throughout the whole plant.

Our opinion is, that these veins do still keep company with their respective fibres. And as all the fibres of the leaf are joined in the stalk of the leaf, and that stalk explicated in clothing the twig or stem of the plant, so do we think of the veins, their perpetual companions.—But moreover, in the roots of plants, if a simple coat be separated and exposed between your eye and the light, the veins appear to be strangely entangled and implicated, and not in the same simple order as in the leaves.—From what has been said, it may well be doubted, whether there is any sinus or common trunk, into which all the veins are gathered? But rather that there are a multitude of equally large veins, each existing apart by itself. We indeed have found it very difficult so to exhaust the plant of its milky juice, as to kill it, though we have given it very many incisions to that purpose.

The substance of these veins seems to be as truly membranous, as the veins of animals: a leaf will not give way and be extended, but the veins in a leaf, if freed of all the woody fibres, may be stretched out to one third part at least, and vigorously restore themselves again, just like a vein, gut, or any other membranous ductus of an animal. Again these membranous pipes are exceedingly thin and transparent, because they suddenly disappear and subside after their being exhausted of their juice; and particularly in that we see the liquor, they hold, quite through them, no otherwise than the blood through our veins.

Concerning the external figure of these veins and cavities, as well as other accidents, we thought they would have been made more apparent to us, if it were possible to coagulate the juice they hold, without much shrinking the plant. We were in great hopes freezing would have effected this; which though it did not succeed as we promised ourselves, in respect of the manifestation of these accidents; yet it gave us some further light into the nature of the juice of these veins. In the keenest frost we dissected the frozen leaves of the garden spurge; and observed, that all the juice, besides that which these veins hold, was indeed frozen into perfect hard ice, and to be expressed out in the figure of the containing pores; but the milky juice was as liquid as ever, but not so brisk as in open weather.

As to the motion of these juices, these things are certain:

1. That the milky juice always moves and springs briskly on the opening of a vein; the limpid sap but at certain seasons, and as it were by accident.—2. The venal juice has a manifest intestine motion, or fermentation, within itself.

There seem to be in plants manifest acts of sense. We instance in the sudden shrinking of some plants; the frequent closing and opening of flowers; the critical erecting of the heads of poppies from a pendulous posture, and particularly the vermicular motion of the veins when exposed to the air. Again, the veins of plants may indeed be different, though at present we cannot tell wherein they are so. The arteries within our heads are hardly to be known by the eye from the veins. Further there are natural and spontaneous excretions or venting of superfluous moisture in plants, visible and constant in the crown imperial, rorella, pinguicula, &c.

Lastly, we shall not omit to observe, that either we must take that away from the other reasons given of the necessity of the circulation of the blood in animals, viz. the hindering of its breaking and clodding; or we must grant the same motion to the venal juice in plants: we having undeniable experiments to show, that the venal juice of plants and the blood of animals agree in this, that they both, when they are once drawn from their respective veins, do forthwith break and coagulate, and that the serum in the one, as well as in the other, becomes a stiff gelly by a little standing.

Copy of a Letter from Somersetshire, concerning a Strange Frost about Bristol. N° 90, p. 5138.

The freezing rain, which fell here the 9th, 10th, or 11th of December last, has made such a destruction of trees in all the villages and highways, from Bristol towards Wells, Shepton-Mallet, Bath and Bruton, and in other places of the west, that both for the manner and matter it may seem incredible; and is more strange than I have found in any English chronicle.

A credible person thus writes of it, "The late prodigious frost has much disabled many old orchards exposed to the north-east. Had it concluded with some gusts of wind, it might have been of sad importance; I weighed the sprig of an ash-tree of just three quarters of a pound, which was brought to my table; the ice on it weighed 16 pounds, besides what was melted off by the hands of those that brought it. A very small bent at the same time was produced, which had an icicle, encompassing it, of five inches round by measure: yet all this while, when trees and hedges were laden with ice, there was no ice to be seen on our rivers, nor so much as on our standing pools."

Similar or even worse and more extraordinary complaints I received from several other places, and from eye witnesses of credit. Some travellers were almost lost by the coldness of the freezing air, and freezing rain. All the trees, young and old, on the highway from Bristol to Shepton, were so torn and thrown down on both sides the ways, that they were unpassable. By the like obstructions the carriers of Bruton were forced to return back. Some were affrighted with the noise in the air, till they discerned that it was the clatter of icy boughs dashed one against another by the wind. Some told me that riding on the snowy downs, they saw this freezing rain fall upon the snow, and immediately freeze to ice, without sinking at all into the snow; so that the snow was covered with ice all along, and had been dangerous, if the ice had been strong enough to bear them.

On Wednesday, Dec. 11, I saw a young man, who returning home from a journey of five miles, and coming into a warm room, cried out of extreme torments in all parts of his body. He affirmed, that the air, and the winds were so unsufferably cold, that he was in utter despair of coming home alive; yet all that day nothing but moist dew fell under our feet.

As soon as these frosts were over, we had glowing heats, which caused a general complaint amongst us of excessive sweating, by night and day. The bushes and many flowers in the garden appeared in such forwardness, as if it were in April or May. I saw young coleworts growing; roots and leaves; on the top leaves of an older colewort. Not far from my abode, an apple-tree blossomed before Christmas.

In old histories I find, that earthquakes, inundations, droughts, famine, pestilences, were each of them, in their several seasons, and sometimes one close on the heels of the other, almost universal over the known world; sometimes raging from place to place several years together. As the learned Meade relates of a pestilence, which, in the days of Gallus and Volusianus, began in Ethiopia, and for 15 years wasted all the Roman provinces.

A Method of Drawing Tangents to all Geometrical Curves, by M. Ren. Fr. Sluse, Canon of Liege, in a Letter to the Editor. N° 90, p. 5143. *Translated from the Latin.*

I send you, Sir, my method of drawing Tangents to any Geometrical Curve whatever, and submit it to the censure of the learned men of the Royal Society. It appears to me so short and easy, that it may be learned by a novice, and without the labour of any further calculation extended to all kinds of lines: but I wish rather to have the approbation of others, since we are commonly too partial to our own inventions.

Let any curve DO (Fig. 1, Pl. 1,) be given, all the points of which are referred to any given right line EAB, by the right line AD; it matters not, whether EAB be a diameter or any other line, or whether there be also given other lines, which, or their powers, may enter the equation.

In an analytical equation, for greater plainness, let DA be always designed by v, BA by y, and let EB and other known quantities be expressed by consonants; then let DC be supposed drawn, touching the curve in D, and meeting EB produced, if needful, in the point C; and let CA be always called a; then this will be the general rule for finding CA or a. 1. Reject out of the equation all those members, in which neither y nor v is found; then put all those terms that have y, on one side; and all those which have v, on the other, with their signs + or —; and let the latter for distinction and ease sake be called the right, the former, the left side. 2. On the right side let there be prefixed to each member the exponent of the power which v has there; or, which is the same, multiply all the members into that exponent. 3. Let the same be done also on the left side, multiplying each member there by the exponent of the power of y therein; and besides, let one y in each member be always changed into a.—The equation thus transformed shows the method of drawing a tangent to the given point D.

For since that point is given, y and v are likewise given, as also the other quantities expressed by consonants, consequently a becomes known. If there be found any obscurity in this rule, it will be cleared up by some examples. Let this equation $by - y^2 = v^2$ be given; in which let EB be b; BA, y; DA, v; and let a or AC be sought, so that drawing DC, it may touch the curve DQ in D.—According to the rule, nothing is to be rejected out of this equation, since in each of its terms either y or v is found; and it is besides so disposed, that on one side are all the members in which y is, and on the other side all those in which v is found; there is therefore only to be prefixed to each member the exponent of the power of y or v in each, and on the left side one y to be changed into a, that it may be $ba - 2ya = 2vv$; now this equation shows the method of drawing a tangent to the point D, or $a = \frac{2vv}{b - 2y} =$ AC. And if the equation $qq + by - yy = vv$ were given, the equation for the tangent would be exactly the same with the preceding, after rejecting qq according to the rule.

So also from $2by^2 - y^3 = v^3$ arises $4bya - 3yya = 3v^3$, or $a = \frac{3v^3}{4by - 3yy}$. And from $bby + zyy + y^3 = qvv$, is obtained $bba + 2zya + 3yya = 2qvv$, and $a = \frac{2qvv}{bb + 2zy + 3yy}$. Also from $b^4 + by^3 - y^4 = qqvv + zv^3$ comes $3byya - 4y^3a = 2qqvv + 3zv^3$, and hence $a = \frac{2qqvv + 3zv^3}{3byy - 4y^3}$.

In these and such like equations there can be no difficulty. Possibly there may be a little in those, which have some of the terms consisting of the products of y and v: as yv, yyv, y^3vv, &c. Yet that difficulty is but inconsiderable: for suppose we have $y^3 = bvv - yvv$; nothing is to be thrown out of this equation, since either y or v is found in each term. But that it may be disposed according to the rule, yvv must be taken twice, and be put both on the right side, in which are the terms having v, and on the left side, whose members have y, since yvv contains both y and v; then we must make $y^3 + vvy = bvv - yvv$. And changing this equation as before into another, viz. $3yya + vva = 2bvv - 2yvv$, a will be equal to $\frac{2bvv - 2yvv}{3yy + vv}$. For the rule is to be thus understood, viz. that on the left side the power of v is not to be regarded, so that the exponent of vv must not be prefixed to yvv, only that of y; as on the right side, the power of y in yvv must not be regarded, but only that of v, whose exponent is to be prefixed. Thus, if it were $y^5 + by^4 = 2qqv^3 - yyv^3$, it should be $y^5 + by^4 + yyv^3 = 2qqv^3 - yyv^3$; and the equation for the tangent would be $5y^4a + 4by^3a + 2yav^3 = 6qqv^3 - 3yyv^3$, and hence $a = \frac{6qqv^3 - 3yyv^3}{5y^4 + 4by^3 + 2yv^3}$.

And these examples seem to comprehend all possible variety of cases. But perhaps it may be of use to apply what was explained in general to some particular line. Let therefore the curve BD, (fig. 2) be given, of such a nature, that assuming in it any point D, and BD be drawn, and DE erected perpendicular thereto, meeting the right line BE in E, the right line DE may be always equal to the given right line BF. To express the equation analytically, let DA $= v$; BA $= y$; BF or DE $= q$; then will EA be $=$ to $\frac{vv}{y}$; and, the square of DE being equal to the two squares of DA and AE, the equation will be $qq = \frac{v^4}{yy} + vv$, or $qqyy = v^4 + yyvv$; which, according to the rule, is to be thus transformed for the tangent, $qqyy - yyva = v^4 + yyva$, and then $2qqyv - 2vvya = 4v^4 + 2yyvv$, and hence $a = \frac{4v^4 + 2yyvv}{2qqy - 2vvy}$.

A skilful mathematician cannot be ignorant how to reduce such equations to easier expressions for construction. As in this example, seeing the rectangle BAE is supposed equal to the square of AD, if EA be called e, it will be $vv = ye$, and $v^4 = yyee$, and $qq = ye + ee$; therefore substituting these values in the above equation, it gives $a = \frac{2ey + yy}{e}$, that is, $ae = 2ey + yy$; and adding ee to both sides, $ae + ee = ee + 2ey + yy$; therefore the three quantities e, $e + y$, and $e + a$, or EA, EB, and EC will be in continued proportion, and the construction will become easy.

As it has been hitherto supposed, that the tangent is drawn towards B, though it may happen from the data to be either parallel to AB, or to be drawn to the contrary part; so it now remains to determine, how this variety of cases may be distinguished in equations. Take then a fraction for a, as in the above mentioned examples, the parts both of the numerator and denominator with their signs are to be considered. For, 1. If in both parts of the fraction all the signs be either affirmative, or at least the affirmative exceed the negative, the tangent is to be drawn towards B. 2. If the affirmative quantities exceed the negative in the numerator, but be equal to them in the denominator, the right line drawn through D parallel to AB will touch the curve in D; for in that case a is of an infinite length. 3. If both in the numerator and denominator, the affirmative quantities be less than the negative, changing all the signs, the tangent is again to be drawn towards B, and this case coincides with the first. 4. If the affirmative quantities exceed in the denominator, and fall short of the negative in the numerator, or on the contrary, then changing the signs in that part of the fraction where they are less, the tangent must be drawn the contrary way, that is, AC must be taken towards E. 5. But whenever the affirmative and negative quantities are equal in the numerator, let them be how they will in the denominator, a will become nothing; and consequently the tangent is either AD itself, or EA, or parallel thereto; as will easily be found by the data. And these various cases may be explained by the equations for the circle.

Thus, let the diameter of a semicircle be EB, (Fig. 3), and let D be a given point, from which may fall the perpendicular $AD = v$. Let $BA = y$, $BE = b$; then the equation will be $by - yy = vv$; and drawing the tangent DC, it will be AC, or $a = \frac{2vv}{b-2y}$. Now if b be greater than $2y$, the tangent is to be drawn towards B; if less, towards E; if equal to it, it will be parallel to EB; as was said in N° 1, 2, 4.

Let there be any semicircle inverted, as NDD, (fig. 4), the points of whose periphery are to be referred to the right line BE, parallel and equal to the diameter. Making $NB = d$, and all things else as above, gives the equation $by - yy = dd + vv - 2dv$: therefore AC or $a = \frac{2vv - 2dv}{b - 2y}$. Now since v here is supposed to be always less than d; if b be greater than $2y$, then the tangent must be drawn towards E; if equal, it will be parallel to BE; if less, changing all the signs, the tangent must be drawn towards B; as by N° 4, 5, and 3. But there could be no tangent drawn, or at least, EB would be the tangent, if NB had been taken equal to the semidiameter, or $2d = b$, as by N° 5.

Let there be another semicircle, whose diameter NB, (fig. 5), is perpendicular to EB, and to which its points are supposed to be referred. Let NB be called b, and all things standing as before; then the equation will be $yy = bv - vv$, and hence $a = \frac{bv - 2vv}{2y}$ Now if b be greater than $2v$, the tangent must be drawn towards B; if less, towards E; but if equal, then DA will be the tangent; as by N° 1, 4, and 5. And these are all the various cases that the consideration of equations can afford.

But how the limits of equations are derived from this doctrine of tangents, I do not explain, being a thing evident; and the application to the maxima or minima, which are determined both at once by the parallel tangent: concerning which, and other matters I have written to you, and have also treated somewhat on them in my Miscellanies; where I have also shown how to find the points of contrary flexure from the tangents. The same method may be applied to other things also, too long to be shown in this letter.

An Account of some Books. N° 90, p. 5147.

I. A Discourse concerning the Origin and Properties of Wind, &c. By R. Bohun, Fellow of N. Col. in Oxon. 1671, in 8vo.

After deducing several inferences, from such relations as the author has obtained from books and travellers, he concludes, As to the whole matter, that it will be hard to lay down any perfect theory of winds, in regard that the great inequalities in the superficies of the earth; the several obstacles and repercussions from mountains; the different situations of the places and mediums in which they blow; the distance of those countries from the poles of the world; their respects to the course of the sun, whether they comply with, or resist the natural motion of the air from east to west, &c.; have many intricate and nice speculations, not easy to be stated.

II. Deux Machines propres à faire les Quadrans, avec tresgrande facilité; par le P. Ignace Gaston Pardies, S. J. A Par. 1673, in 12mo.

The author of these two machines thinks they are sufficient for any person, by their means, to learn the whole theory and practice of dialing, and that in less than an hour's time; and may practice what he so learns, as it were by play, drawing the dials on walls, and in his chamber, with the greatest ease.

A New Experiment concerning an Effect of the Varying Weight of the Atmosphere on some Bodies in the water. By R. Boyle, Esq. N° 91, p. 5156.

I am prone to suspect, that the alterations of the atmosphere in point of weight may, in some cases, have some considerable effects even on men's health; as when the ambient air, for instance, grows suddenly very much lighter than it was before, or than it was wont to be, the spirituous and aërial particles, that are plentifully harboured in the mass of blood, will naturally swell that liquor, and so may distend the greater vessels, and not a little alter the celerity and manner of the circulation of the blood by the capillary arteries and veins. To countenance this conjecture of mine, I will annex an experiment that you will not perhaps dislike, just as I find it registered among some of my loose papers.

I caused to be blown at the flame of a lamp three small round glass-bubbles, about the size of hazel nuts, and furnished each of them with a short and slender stem, by means of which they were so nicely poised in water, that a very small change of weight would make them either emerge, if they but lightly leaned on the bottom of the vessel, or sink, if they floated on the top of the water.

This being done at a time when the atmosphere was of a convenient weight, I put them in a wide-mouthed glass furnished with common water, and suffering them to continue many weeks, or some months, I observed, as I expected, that sometimes they would be at the top of the water, and remain there for divers days, or perhaps weeks; and sometimes would fall to the bottom, and after having continued there for some time, they would again emerge. And though sometimes they would rise to the top or fall to the bottom of the water, according as the air was hot or cold; yet it was not difficult to distinguish those motions from those produced by the varying gravity of the atmosphere. For when the beams of the sun, or heat of the ambient air, by rarefying the air included in the bubbles, made that air drive out some of the water, and consequently made the whole bubble (consisting of glass, air and water) somewhat lighter than a bulk of water equal to it, though the bubble did necessarily swim as long as the included air was thus rarefied, yet when the absence of the sun, or any other cause made the air lose its adventitious warmth, there would ensue a condensation of the air again, and thereupon an intrusion of more water (to succeed the air) into the glass, and consequently a sinking of the bubble: and this would commonly happen at night, if it did not happen sooner. But when it was upon the account of the varying weight of the atmosphere that the bubbles either rose or fell, it appeared by the baroscope, that the atmosphere was so heavy or so light, that they ought to do so. Insomuch that I divers times predicted, whether I should find the mercury in the baroscope high or low, by observing the situation and posture of the bubbles; and consulting that instrument, it verified my conjectures.

N.B. 1. It being very difficult to poise several bubbles precisely, as well one as another, I thought it not strange, that all the three bubbles did not constantly rise and fall together, but sometimes two of them, and now and then one alone would sink or emerge, when the change of the weight of the atmosphere was not considerable enough to operate sensibly upon the rest. And therefore it is not amiss, to poise a greater number of bubbles together, that, after trial made of all, the fittest may be chosen.

2. I have observed it sometimes to happen, that a bubble, that floated when it was first poised, would after a while subside without any manifest cause; or if it were made to sink by such a cause, it would continue at the bottom of the water, though that cause were removed: which difficult phenomenon seeming to depend upon a kind of imbibition made of certain particles of an aërial nature by the water, the consideration of it belongs to another place, not to this; where it may suffice, that the experiment did sometimes actually answer expectation as that above related did; wherein my main drift was to show, that since, as the atmosphere is heavier or lighter, it is capable of working upon bodies under water so as to procure their sinking, or their emersion; the air (though a fluid a thousand times lighter) must lean or press upon the water itself, by whose intervention it produces these effects; which confirms what I

elsewhere teach, that the atmosphere is incumbent as a heavy body upon the terraqueous globe.

Extract from some Letters of Dr. John Wallis to the Editor, 1672, *Sept.* 26, *&c. concerning the Suspension of Quicksilver well purged of Air, much higher than the ordinary Standard in the Torricellian Experiment.* N° 91, p. 5160.

I am not sorry to find in your Transactions for the last month, that M. Huygens endeavours to account for that odd phenomenon in the Torricellian experiment, of which I give an account in my treatise De Motu, Cap. 14 Schol. prop. 13. The phenomenon is this:

Whereas in the Torricellian experiment, the quicksilver contained in the inverted tube, how long soever, whose open orifice C, (fig. 6, pl. 1) is immerged in stagnant quicksilver, does usually fall down to the height of about 29 inches above the surface of the stagnant quicksilver AB, and there remains suspended, as at I: if the quicksilver be well cleansed from air, it has been found to stand top-full, much higher, even to the height of 75 inches, and how much higher it may stand, we cannot tell; but upon the admission of the least air, or a concussion of the tube, it falls down to the usual standard.

Two reasons I did there hint, though not perfectly satisfied in either: the one of my own, concerning the spring of the air, necessary to put heavy bodies in motion, not impelled by any other force: the other of my Lord Brouncker, that there might be in the air yet a greater weight or pressure than is necessary for the height of 29 inches, in case there be nothing but the bare weight of quicksilver to be supported.

I find M. Huygens falls in with that of my Lord Brouncker, save that what we comprehend under the name of air, he calls a more subtile matter: which alters not the case at all, but only the name. M. Huygens here, by air, seems to understand that feculent matter arising from the earth and water's effluvia, which are intermingled with this subtile matter. We mean by air, the aggregate of both these, or whatever else makes up that heterogeneous fluid wherein we breathe, commonly called air; the purer part of which is Mr. Hobbes's air; and the feculent of it is M. Huygens's air.—And therefore, where I speak of vacuity caused by the Torricellian experiment, or such other ways, I do expressly caution not to be understood as affirming absolute vacuity, but at least an absence of that heterogeneous mixture which we call air, such as that is wherein we breathe; without disputing against the purus æther of Mr. Hobbes, or the materia subtilis of Descartes or M. Huygens; as not necessary to the inquiries in hand.

To the pressure of this purer matter, which they suppose so subtile, as to penetrate the mercury, marble, and glass itself, they ascribe the suspension of the quicksilver to so great a height. And my Lord Brouncker in particular had a design of prosecuting the experiment, as M. Huygens now advises, to see if he could bring it to some determination what was the utmost height at which it might be thus made to stand; thereby to determine the pressure of this purer matter, as that of the common air is determined by the Torricellian experiment. But his leisure not then serving, I only gave that brief account of his notion, as it is there inserted: and, whether he has since had leisure, amidst a great press of other business to pursue it, I am not certain.

Now, though I would not wholly exclude this, if such shall be found to be evinced, yet surely there must be somewhat more in it than that of this subtile matter, to solve the phenomenon, notwithstanding the two experiments now alleged by M. Huygens in favour of it. For, if this matter be so subtile as to press, through the top of the glass, on the quicksilver, and consequently through the upper on the nether of the two marbles, as is acknowledged; I do not see why it should not balance itself, above and below, in the same manner as common air would do, if the tube were pervious to it at both ends, and the quicksilver, by the preponderance of its own weight, fall presently.

And the answer, that, though the glass be penetrated by it, yet not in so copious a manner as where no glass is; does not to me solve the difficulty: because the same obstacle remains just in the same manner when the tube is in part emptied, and when the quicksilver is unpurged: the pores of the glass not being, by either of those, made more open or more pervious. And if we suppose the subtile matter by percolation to be strained through, with some difficulty, as air or water would be through a cloth, this might possibly cause the quicksilver, when it does sink, to sink gradually; but not, as we see it, suddenly to fall to the height of 29 inches; as from D to I.

The connection or cohesion of the parts of quicksilver, either to each other, or to the sides of the glass, which Mr. Huygens supposes to require, for their separation, a greater force than is in these percolated particles, till they have room made for them to combine; seems to me the less considerable, because it is not so necessary to separate them from each other, since they may unseparated slide down by the sides of the glass; to which, it is well known, and visible to the eye, the quicksilver is not at all apt to stick, but doth rather decline that contact; in like manner as we find water not apt to join with oil or grease; though water to glass, and quicksilver to gold, do very readily apply themselves. So that there needs no such force to disjoin the quicksilver from the glass, whatever there may be for disjoining its parts one from another.

If therefore we should suppose the pressure of the grosser air downwards on AB, (fig. 6, pl. 1) the surface of the stagnant quicksilver, and consequently by means thereof, upwards at C, sufficient only to bear up that in the tube to the height of I; but the superadded weight or pressure of the purer air to hold it up as high as D, 75 inches or more, while it is full, and the quicksilver well cleansed; as if so long it could not enter at D; but in case it be not so cleansed, or be already sunk to H, this purer air would enter at D, and thrust it down to I, counterbalancing the pressure (at C) of the purer, but not of the grosser air, which I take to be the sum of the cause assigned by M. Huygens: I am yet to seek, why it may not as well penetrate D at first to begin the descent, as afterwards to pursue it; and why not as well begin the descent when the quicksilver is well cleansed of air, as when it is not so; and why also, if the pure air do freely enter at D, it does not presently fall; or, if not freely, why, when it does fall, it falls suddenly and not leisurely from D to I; especially since so small a weight as DH of pure air (for the grosser cannot enter,) is very inconsiderable; if not at all, or not freely pressed by that incumbent on D; and the adhesion not considerably less, by being separated only at the top, while it yet continues to touch the sides.

I am apt therefore, as heretofore, to ascribe the cause of this phenomenon to the spring that is in air, and the want thereof in quicksilver. For that in air there is a spring or elasticity, is now undoubted; but in water cleansed of air, though many experiments have been attempted to that purpose, it has not yet been found that there is any: and I am apt to think the like of quicksilver; though I do not know that this has been yet so rigorously examined. Now supposing that matter, being at rest, will so continue till it be put in motion by some force; this force may be either that of percussion from some body already in motion, which is the case when the quicksilver falls by shaking or striking the tube; or of pulsion, from a contiguous body beginning to move, as by the expansion of some adjacent spring, which is the case, when the springy parts of the air, either left in unpurged, or re-admitted in the quicksilver, by expanding themselves, put the quicksilver in motion; or some conatus or endeavour of its own, such as is that of a spring; and therefore if water and quicksilver be not such, they will not on this account put themselves in motion.

Gravity or heaviness is, I know reputed to be such a conatus or proneness to move downwards, and so to put itself in motion: and the wonder at present is, why it does not so here. But if this which we call gravity, should chance to be not a positive quality or conatus originally of itself, but only the effect of some pulsion or percussion from without, which possibly may be the case, and principally from the spring of the air about us; then while this pulsion and percussion is wanting, the bodies accounted heavy will not of themselves begin to fall: which seems to be the present case.

And this is the more considerable, because we cannot find what is the utmost height at which the quicksilver, thus accumulated, will remain suspended; there having been no height yet attempted, at which, if cleansed, it will not stand; and that of 75 inches, considering the weightiness of quicksilver, is a very great one, being more than equivalent to 80 feet of water.

My Lord Brouncker a little alters the case, from what I take to be the hypothesis of M. Huygens. For he supposes this purer part of the air to be of like nature with the grosser part, which I think M. Huygens does not; and, though finer than the rest, so as to penetrate glass, which the grosser will not; yet of a springy nature, as the grosser parts are: which therefore acts, not by its weight only, but by its spring; and therefore when once entered, though in a small proportion, acts as effectually, at its first entrance, as if the whole incumbent air had admission; its spring being of a like tensure with that of the outward air. But M. Huygens's more subtile matter than air, though he must allow it weight, yet whether he allow it a spring, I cannot tell; nor does he inform us. And when he says, this more subtile matter without difficulty penetrates glass, water, quicksilver, and all other bodies, which we find impenetrable to air; I know not whether he mean, without any difficulty, or without great difficulty, though with some.

But his Lordship, though he allow his springy subtile matter to penetrate glass, yet not without difficulty; and till it have some room made (as HD) wherein it may recollect itself, cannot exert its spring, and therefore not while top-full of cleansed quicksilver: but, so soon as some room is made for it: whereas if the quicksilver be not purged of air, that little air remaining by its spring begins the motion. He thinks it also not improbable, that a large but low tube of glass, shorter than 29 inches, may stand top-full of quicksilver, though with a small hole in the top, as at K; at least, if immerged in water, in case air be too subtile for our mechanics. He might also, suitably enough to his own hypothesis, have so explained himself, as to allow his more subtile parts of common air to penetrate quicksilver, but not glass; and therefore, in case of room for it at HD, it might through the stagnant quicksilver, and that at C, pass upwards to HD, and there exert its spring.

I shall forbear to dispute against this hypothesis for the present; because I think it more proper to examine by experiment, whether well purged quicksilver may not be made to stand higher than CI, the ordinary standard, suppose at

CH, with a void space about it, as HD. For the issue of this experiment seems very proper for determining this doubt; which therefore I am not willing to prejudge. There is yet another way of explaining the same hypothesis, without allowing this subtile matter to pierce the glass, which is this: our common air being an aggregate of very heterogeneous parts, we may well suppose some of them to be springy, and others not to be so. The springy parts we may conceive to be so many consistent bodies, like small hairs or springy threads, wrapped up in different forms and variously entangled, and so as to form many vacuities capable of admitting some fluid matter, which may insinuate into those vacuities, as water in a bundle of bushes, without disturbing the texture of those springy parts; and which may press as a weight, but not as a spring. Now if in the Torricellian tube, there be a quantity of such springy matter, the spring hereof will be of equal strength with that of external air, and therefore able to counterbalance it, though its weight be much less, because admitted with such a tensure. But if only an unspringy fluid, which presses only as a weight, not as a spring, and this defended by the glass tube from any other pressure, save that of its own weight; it will still be too weak to force its own way, till its single weight be equivalent to that with which it is to encounter; which is, not only the springy part of the air, but also that fluid unspringy part; which though it would give way to a springy body pressing through it; yet not to this fluid, like itself, and destitute of such a spring; and is therefore able to keep it up to a much greater height than it could do if uncleansed of springy air: so long at least as till some springy body be admitted, or some concussion, equivalent to it, put it in motion; but being once in motion, it will so continue, as a bullet impelled by gunpowder, or an arrow out of a bow, till stopped by some positive equivalent force.

I do not deny, but that this explication may be subject to some difficulties and exceptions; but I think fewer than that of allowing the glass penetrable by this subtile matter. But the best way to settle this business, being some suitable experiments; I should recommend these, or some of these experiments, to those of the Royal Society, who are in that kind better provided than I am.

1. That hinted by my Lord Brouncker, whether a large low tube, of less height than the common standard, of about 29 inches English, or 27 inches French, might be made to stand top-full of quicksilver, though a small hole be left open at the top; at least under water? I am apt to think, that it will rather sink slowly and with a hiffing noise, than fall suddenly and silently.

2. Whether of two polished marbles, or metalline plates, the lower will be found to stick to the upper, in the exhausted receiver, longer than is accountable for from the ordinary counter-balance in the Torricellian experiment. For though M. Huygens now, and Mr. Boyle, have long since intimated this from

his own experience; yet I judge the experiment worth repeating. And if it be, as I suppose it may, found to succeed, I should think it may proceed from a want of a spring or elastic power between the plates, to force them asunder; and in particular that spirit of wine is not a springy body.

3. Whether a syphon of unequal legs will be made to run, in an exhausted receiver, with water or quicksilver, at a greater height than is accountable for; which though M. Huygens has tried it, I think it worth repeating in this Society. This when it does succeed, I take to proceed from the spring of that little remaining air in the receiver not quite emptied.

4. Which seems of a like nature with the former, whether a tube of greater length than 29 inches, but so immerged as to be less than so much above the level, as CE, may not, if filled with well cleansed quicksilver, be gently lifted up with the quicksilver in it, not only to I, as when it is unpurged, but to H or D, higher than the usual standard.

5. Which is equivalent, but more easily administered, whether if such a tube, so filled, be at first so inclined, as CF, that its height above AB be less than 29 inches, may not be leisurely and gently erected, so as to remain full, not only to the height of I, but of G or D? 6. Whether cleansed quicksilver will, in the open air, run in a syphon higher than 29 inches? 7. If not in the air, whether it will so run, if the lower leg open into well-cleansed water?

8. Which I do principally recommend; in a tube so filled with cleansed quicksilver, as to stand top-full at a greater height than the usual standard, as CD; in case some part be forced out, not by admission of air, but by jogging the tube, suppose as much as HD, and a stop then made; whether the rest CH, at a greater height than I, the usual standard, may be made so to stand of itself, notwithstanding the voidance of HD? For by this experiment alone, if it succeed, it will appear, that it is not only want of room for the subtile matter to recollect itself, which hinders the suspended quicksilver from falling; but rather the want of a spring to put it in motion. If it will not succeed, I should rather think the springy air makes its way through the quicksilver, than through the glass.

9. Whether cleansed quicksilver will remain suspended in an inverted tube, at least a short one, and with a small orifice, though its orifice C be not immerged in quicksilver, but either in the open air, or at least in water? 10. If so; then whether it will do the like, if, a little being forced out, there be some void room left at the top at HD?

These are nice experiments, and of some difficulty; but if carefully administered, may be of good use in our search after the true nature of gravity: which may possibly have a greater connexion with the spring of the air, than men are aware of; since on the presence or absence thereof doth mainly depend the falling or not falling of bodies accounted heavy. But I am not willing, by interposing my own conjectures, to prejudge the experiments.

Account of Two Books, N° 91, p. 5170.

I. Observations Topographical, Moral and Physiological, made in a Journey through part of the Low Countries, Germany, Italy, and France, by John Ray, Fellow of the Royal Society; whereunto is added a Brief Account of F. Willoughby, Esq.; his Voyage through a great part of Spain, 1673, in 8vo.

This Itinerary contains whatever is remarkable in those places, which the ingenious and inquisitive Author travelled through. Let his reader be a Statesman, an ecclesiastic, a philosopher, an artist, a tradesman, a father of a family, an husbandman, they will all of them find matter in this book very proper for their respective genius, professions, and callings. Here is described the climate, government, revenues, laws, customs, manners, tempers, abilities, studies, arts, trades, and natural productions of the countries spoken of; and besides, divers fabulous relations and ungrounded fancies refuted and rectified.

II. Bernhardi Vareni * M. D. Geographia Generalis; in qua affectiones generales Telluris explicantur, summâ curâ quamplurimis in locis emendata, aucta et illustrata, ab Isaaco Newtono Mathes. Professore Lucasiano apud Cantabrigienses è Societate Regia. Cantabrigiæ 1672, in 8vo.

* Bernard Varenius was a noted Dutch Physician, who died in 1660. The above ingenious Treatise on Universal Geography, which was honoured with the attention of Sir Isaac Newton, in his Lectures at Cambridge, has been also translated into English, in 2 vols. 8vo, with various notes and emendations by Sir Isaac and Dr. Jurin. Varenius was also author of a curious description of Japan, and the kingdom of Siam, in Latin; printed at Cambridge in 1673.

END OF VOLUME SEVENTH OF THE ORIGINAL.

Discovery of Two New Planets about Saturn, and some Fixed Stars. By S. Cassini. N° 92, p. 5178. *(Vol. VIII.)*

About the end of Oct. 1671, Saturn passed close by four small fixed stars, visible only by a telescope, within the sinus of the water of Aquarius, discovered in the same place within the space of 10 minutes, by a telescope of 17 feet, made by Campani, eleven other smaller stars, one of which, by its particular motion, showed itself to be a true planet: which we found by comparing it, not only with Saturn and his ordinary satellite, discovered in 1655 by Mr. Huygens, but also with other fixed stars.

These observations show a motion of this new planet that is very manifest in respect of the fixed stars, but less sensible in respect of Saturn. Yet it appears, that from Oct. 25, to Nov. 1, its distance from Saturn increased westward, and from that time to Nov. 6, it diminished; so that its greatest digression from Saturn happened in the beginning of Nov., and was found to be of 8 minutes, or of $10\frac{1}{2}$ diameters of Saturn's ring. Whence it followed, that if this planet were a satellite of Saturn, it must be about the end of Sept. in the inferior part of its circle, and at the beginning of Nov. in the superior part; and that its revolution about Saturn was of a long duration, since for 12 days together it not only remained on the same western side of Saturn, but there was also little change of apparent distance between them. The greatest digression of this planet was treble to that of the ordinary satellite, and this enabled us to judge the time of its revolution to be quintuple, applying to the satellites that proportion which Kepler noted in the principal planets, between the periodical times and their distances.*

We again got sight of Saturn, Nov. 12, 16, 17, 19, 23; but we could find no appearance of the new planet. Dec. 16, we found that Saturn had resumed his round figure, and that on the east of him there was a small star, far distant, in a straight line with Saturn, and with his ordinary satellite, which was also eastward, and but little distant from Saturn. And Dec. 24, we saw this satellite in the west, and a star on the east, less distant from Saturn than that we had seen the 16th. But the weather did not permit us to ascertain whether it was the same. At length, Jan. 18, 23, 25, of the year 1672, we saw on the west of Saturn, sometimes one star, sometimes many, far distant, almost in a direct line with his ordinary satellite; which made us hope to see again the new planet towards its greatest western digression. But these observations were the last which the weather suffered us to make, before Saturn became lost in the beams of the sun.

After my return from a journey to Provence, having brought with me from Marseilles, in the beginning of Nov. 1672, an excellent telescope of 35 feet, which Campani had made by order of his Majesty; we set it up in the Royal Observatory, directing it to Saturn, as soon as the weather would permit, to look for the new planet. In the first observations, made Dec. 13 and 17, we perceived a star to the west, remote from Saturn, which in both these obser-

* Namely, the squares of the periodical times, proportional to the cubes of the distances.

vations had a southern latitude in respect to the line of his wings; but in the first it was further distant from Saturn than in the second.

We could not see Saturn again till the 23d of Dec. and then, in the presence of Messrs. Huygens, Picard, Mariotte, Romer, and others of the Royal Academy of the Sciences, we found a small star westward of Saturn, between him and his ordinary satellite, which was on the west also, almost at a double distance. And at that time we had no other reason to suppose it to be different from the former, but that it had no latitude at all in respect of the line of Saturn's wings. The weather did not suffer us to see Saturn again till the 30th of Dec. and then we saw a little star on the east of him, without any latitude, between him and his ordinary satellite, which had passed also to the east of him. This observation, compared with the foregoing, kept us yet in suspense, because we know not, whether this, which seemed to us the same with that of the foregoing observation, had passed from one side of Saturn to the other, by only one motion slower than that of the ordinary satellite, and consequently by a little arch of a greater circle; or whether, during this interval of time, it had made one or more turns by a lesser circle; which was much more agreeable to the position in which it had appeared, without latitude, in both observations.

The Heavens were not favourable to us again till the 10th of Jan. 1673; and then this little star appeared to have returned almost to the same position in respect of Saturn, and his ordinary satellite where it had been Dec. 23. We wondered to have found three times successively, this small star between Saturn and his ordinary satellite, always nearly equally distant from them both. But our admiration ceased at the 4th observation, made Jan. 15, in which the ordinary satellite was to the east, and the new one west, as it had been in the foregoing, but a little nearer to Saturn. We had that evening time enough attentively to observe this planet for a whole hour together, during which we perceived it approached to Saturn on the west, and consequently was in the superior part of its circle; which fully confirmed us in the supposition that it was an interior satellite. Thus the pursuit of another satellite, which we knew to be further distant from Saturn, and to have a longer period, made us discover this which is nearer to it, and whose period is shorter.

Then it was, that comparing the observations together, we began to find the nature of the motion of the new interior satellite. For the last two showed us, that in 5 days it had made more than a whole revolution. The first observation compared with the third, showed that in 18 days it had made a number of revolutions, almost whole ones, which certainly were four; each of them of $4\frac{1}{2}$ days: So that between the 10th and 15th it might be, that there had been one revolution of $4\frac{1}{2}$ days, or two revolutions of $2\frac{1}{4}$ days each. But the combination of the first with the second, made us seclude the period of $2\frac{1}{4}$ days. We therefore judged, by these observations, that this last planet finishes its revolution about Saturn in $4\frac{1}{2}$ days; that the semidiameter of this circle is three semidiameters and a fourth of Saturn's ring; and that it was towards its greatest westward digression the 23d of Dec. and Jan. 1, about 7 in the evening.

On these grounds, after the 4th observation, we made an ephemeris of this planet, which has served us since, until the occultation of Saturn, without having found any other difference in the observations, but that, as for the nearest planet, the return to the same place, after one revolution of $4\frac{1}{2}$ days, is made one hour later, so that one circuit is finished in 4 days and 13 hours. We have also learned by the following observations, that when the interior satellite is much distant from its great digressions, it has some southern latitude in respect to the line of the wings in the upper semicircle, and some northern latitude in the inferior; as has also the old satellite, which has more of it in proportion to the diameter of its circle.

Our application to observe the planet nearest to Saturn, in the small time we had at evenings, by reason of his proximity to the sun beams, had diverted us from the other more remote planet. But Feb. 6, we began to see it again, and the weather favoured us well enough to observe it almost all the days following, until the 20th of Feb. except the 9th and 18th. It was conveniently seen by Campani's telescope of 17 feet, by which the first discovery of it had been made; and by another of 20 feet, made by Lebas, with which Mr. Picard observed it also constantly, and sometimes in the company of Mr. Huygens and Mr. Mariotte. The first observations of the distances were made by an estimate of the eye, comparing the exterior satellite with Saturn and with the other satellites. The last were made by the measure of the time between the passage of the planet, and that of the centre of Saturn. This new planet more and more removed from Saturn till the 9th of Feb. when we measured the difference between its passage and that of the centre of Saturn, and found it 30 sec. of an hour, which give at least 10 diameters of Saturn; but on the 20th, it was already too near the beams of the sun to measure its distance; which yet by estimate was judged greater than it had been the 19th.

By the apparent swiftness of its motion during the first days, it is easy to see, that this planet had been seen in conjunction with Saturn Feb. 3; and by its motion on the west it appears, that it was in the inferior part of its circle: and because, during this time of 17 days, it removed more and more from Saturn, it is certain that it remained in the same quadrant of the inferior western circle above 17 days, and that its whole time of revolution is more than 68 days. It was these last days at a distance almost equal to that which it had about the end of Oct. 1671, so that in about 480 days it made a certain number of entire revolutions, which can be no more than 7; since each of them is doubtless of more than 68 days. If we count 7 of them, each would be 68½ days: if we count 6, each would be 80 days; if we count but 5, each would be 96 days. But this last supposition can by no means be made to agree with the two observations of Dec. 1672, and the first does not agree with them so well as the second. The proportion of the apparent distances in the observations of Feb. which are the best, would make us estimate each of its revolutions between 80 and 96 days; but the proportion of the greatest digression of 1671, compared with that of the two other satellites, together with their periods, agrees better with 80 days.* Therefore in the Ephemeris which we give of one revolution, we follow this, until we get a more precise determination, which requires a greater number of observations.

Extract of a Letter of David von der Becke, a German Philosopher and Physician at Minden, concerning the Principles and Causes of the Volatilization of Salt of Tartar and other Fixed Salts. Printed at Hamburgh, 1672. N° 92, p. 5185.

By the volatilization of salt of tartar, is here meant the volatilization of the tartareous acid from cream of tartar, (crystals of tartar, acidulous tartrite of potash). It is unnecessary to notice any farther concerning this uninstructive chemical paper.

Observations on the Nature of Snow. By Dr. Grew. N° 92, p. 5193.

If Aristotle and Descartes, &c. who have written of meteors, and amongst them of snow, have not yet given a full account of it; it will not be needless to inquire further of it. He that will do this, will do it best, not by the pursuit of his fancy in a chair, but with his eyes abroad; where if we use them well fixed, and with caution, and this in a thin, calm, and still snow, we may by degrees observe: 1st, with M. Descartes and Mr. Hook, that many parts of snow are of a regular figure; for the most part, as it were, so many little rowels or stars of 6 points; being perfect and transparent ice, as any we see on a pool or vessel of water. On each of these 6 points are set other collateral points, and those always at the same angles as are the main points themselves. Next, among these irregular figures, though many of them are large and fair; yet from these taking our first item, many others, alike irregular, but much smaller, may likewise be discovered.

* Later observations have served to establish the period of revolution of this satellite, which is the 5th in order, at 73 days 7h. 48m.

Again, among these not only regular, but entire parts of snow, looking still more warily, we shall perceive that there are divers others, indeed irregular, yet chiefly the broken points, parcels and fragments of the regular ones. Lastly, that besides the broken parts, there are some others which seem to have lost their regularity, not so much in being broken, as by various winds, first gently thawed, and then frozen into little irregular clumps again.

From hence the true notion and external nature of snow seems to appear, viz. that not only some few parts of snow, but originally the whole body of it, or of a snowy cloud, is an infinite mass of icicles regularly figured; that is, a cloud of vapours being gathered into drops, the said drops forthwith descend; on which descent, meeting with a soft freezing wind, or at least passing through a colder region of air, each drop is immediately frozen into an icicle, shooting itself forth into several points on each hand outward from its centre: but still continuing their descent, and meeting with some sprinkling and intermixed gales of warmer air, or in their continual motion and waftage to and fro, touching upon each other, some are a little thawed, blunted, frosted, clumpered, others broken, but the most clung in several parcels together, which we call flakes of snow.

It being known what snow is, we perceive why, though it seems to be soft, yet it is truly hard; because true ice; seeming only to be soft; because on the first touch of the finger on any of its sharp edges or points, they instantly thaw; otherwise they would pierce our fingers like so many lancets. Why again, though snow be true ice, and so a hard and dense body, yet very light; because of the extreme thinness of each icicle in comparison of its breadth. Also how it is white, not because hard; for there are many soft bodies white; but because consisting of parts all of them singly transparent, but being mixed together appear white; as the parts of froth, glass, ice, and other transparent bodies, whether soft or hard.

Thus much for the external nature of snow; let us next a little inquire into its essential nature. Now if we would make a judgment of this, I think we may best do it by considering what the general figure of snow is, and comparing the same with such regular figures as we see in divers other bodies. As for the figure of snow, it is generally one, viz. that which is above described: rarely of different ones, which may be reduced chiefly to two generals, circulars and hexagonals, either simple or compounded together. More rarely, either to be seen of more than 6 points; but if so, then not of 8 or 10, but 12. Or in single shoots, as so many short slender cylinders, like those of nitre. Or by one of these shoots, as the axle tree, and touching upon the center of a pair of pointed icicles, joined together as the two wheels. Or the same hexagonal figure, and of the same usual breadth; but continued in thickness or profundity, like the stone which Boetius calls Astroites. All these I say are rare, the first described being the general figure.

As for the configurations of other bodies, we shall find that there are divers which have some a less, others a more near resemblance to snow. Nitre is formed into long cylindrical shoots, as also all lixivial salts for the most part, resembling the several points of each starry icicle of snow. Salt of hartshorn, sal ammoniac, and some other volatile salts, besides their main and longer shoots, have others shorter branched out from them; resembling as those the main, so these the collateral points of snow. But the icicles of urine are still more near: for in salt of hartshorn, although the collateral shoots stand at acute angles with the main, yet not by pairs at equal height: and in sal ammoniac, although they stand diametrically opposite, or at equal height, yet at right angles: whereas in the icicles of urine, they stand at equal height, and at acute angles both; in both, like those of snow. And it is observable, that the configuration of feathers is likewise the same. The reason whereof is, because fowls have no organs for evacuation of urine, and the urinous parts of their blood are evacuated by the habit of skin, where they produce and nourish feathers.

From hence it should seem, that every drop of rain, containing in itself some spirituous particles, and meeting with others in their descent, of a saline, and that partly nitrous, but chiefly urinous, or of an acido-salinous nature; the said spirituous parts are intercepted by them, and with those the watery, and so the whole drop is fixed.

On the Strange Freezing noticed in Numb. 90. *By Dr. Wallis.* N° 92, p. 5196.

The extraordinary freezing which happened in Somersetshire in Dec. last, was similar with us at Oxford. It was rather a raining of ice, or at least rain freezing as it fell; which made strange icicles hanging on trees, and a noise by the rattling of them on the boughs moved by the wind. Yet more in the country about us it seems, than with us here. And the great warmth soon after was also with us; insomuch that not only blossoms, but as it is said, green apples on divers trees; particularly in the parish of Holywell.

An Account of two Books. N° 92, p. 5197.

I. Tracts written by the Honourable Robert Boyle, containing New Experiments touching the Relation betwixt Flame and Air, and about Explosions: An Hydrostatical Discourse, occasioned by some Objections of Dr. Henry Moore, &c.; To which is annexed an hydrostatical Letter, about a way of weighing water in water: new Experiments of the positive or relative Levity of Bodies under water; of the Air's Spring on Bodies under water: and about the different Pressure of heavy Solids and Fluids. London, 1672, in 8vo.

These tracts being well known to philosophers, but little need now be said concerning them, especially after the long and particular title.

In the third part, he discusses at large and solves this problem: whence it is, that urinators or divers are so far from being killed or oppressed by the weight of the incumbent and ambient water, that they are not so much as hurt by it. Concerning which he says it is taken for granted, that divers, though at ever so great a depth, feel no pressure against them by the water, which he says is an affirmation in point of fact, of whose truth he makes some question. To this hydrostatical discourse our author subjoins a letter, illustrating an experiment to weigh water in water, on account of some exceptions made to it by Mr. George Sinclair, in his hydrostatics, lately printed at Edinburgh. On which occasion the editor of these papers finds himself obliged to take notice of a pamphlet annexed to this book of Mr. Sinclair's, called a Vindication of the Preface of the book, intitled Georgii Sinclari, &c. Ars nova et magna Gravitatis et Levitatis, from the challenges and reflections of the publisher of the Phil. Trans. as they are to be found in Numb. 50, Aug. 16, 1669. Not to reflect, as it deserves, on the artifice of leaving this pamphlet out of the copy, that was presented by Mr. Sinclair to Sir R. Moray, a person whom he knows to be very far from allowing his pretences in the preface here questioned; the editor first of all desires the reader to observe, how grossly Mr. Sinclair prevaricates in his pretended vindication, when he asserts that the MS. of Ars nova et magna, &c. was not committed by the author to the judgment of the Royal Society, omitting the main part, contained in these words, which (recording) is yet their constant and careful practice to do in all things of that nature. For if this had been taken in by Mr. Sinclair, he must certainly have thought none but such as are wholly ignorant of the candor and justice of that illustrious body, and of the care of the sworn secretaries thereof, would believe him in what he so boldly and immorally asperses them with, viz. that it was the interest of them, who had taken out the purposes of his MS. to procure it should not be recorded in the register; unless it should be said that the registry had been in this only case purposely omitted at the solicitation of the pretended plagiaries; and who they are, has not yet been declared by M. Sinclair: who, in the next place, might do well to consider, not only how much, before his pompous Ars nova et magna came abroad, had been printed of the doctrine of the air's pressure, and likewise how well was known the way of counterpoising air with quicksilver in glass tubes; but also that in this so generally inquisitive and experimental age, it not unfrequently happens that learned men, proceeding in their researches on solid principles, though they reside in places far distant from one another, and without any mutual communication or knowledge of their respective studies, yet happen to light upon and discover the same things and truths; as may easily be made out by undeniable proofs in the matter of curve lines found equal to straight ones; in the doctrine of motion; in the anatomy of plants, &c. And having said thus much, if M. Sinclair still persists in the good opinion he has of himself, we shall leave him still to enjoy it; though we think it may be much sunk by this time.—In the 4th tract our author endeavours experimentally to show, that, though not only the peripatetic schools, but the generality of philosophers both ancient and modern, as well as the vulgar, ascribe the ascension of lighter bodies in water to an internal principle, by them called positive levity; yet we need not admit any such thing for the true and adequate cause of the emersion of wood and such lighter bodies, put under water.—In the 5th he adds to the proofs, already given of the power of the spring of the air, some of the operations he has discovered it to have on bodies placed under water. In doing which he employs two sorts of trials, showing, that a small quantity of inclosed air may by its pressure have a considerable operation on bodies covered with water, notwithstanding the interposition of the liquor; which pressure may be manifested, both by what it directly and positively operates upon bodies under water; and by the things that regularly ensue upon the removal of the inclosed air, or the weakening of its spring.—In the 6th and last, the author considering that it has proved a great impediment to men's freely acquiescing in the doctrine founded on the phænomena of his physico-mechanical experiments, that if the atmosphere could really exercise so great a pressure, as he ascribes to it, it would unavoidably oppress and crush all the bodies exposed to it: he therefore employs in this tract divers weighty considerations and remarkable experiments, to remove the force of that plausible objection.

II. Esperienze intorno à diverse cose naturali, et particolarmente á quelle che ci son portate dall' Indie; fatte da Francesco Redi. In Fir. 1671, in 4to.

This learned author, desirous of examining many traditions about natural things, begins with certain snake-stones, described by various authors, found in the head of some serpents in the East Indies, and believed to be a sure antidote against the biting or stinging of venomous animals, when applied to the wound, to which it is said they will stick very fast, till they have imbibed the poison; which done, they will fall off: all which is invalidated by the author upon many trials.

He next takes notice of several things that produce real effects, but not always, by reason of some impediments intervening. Ex. gr. That aqua-vitæ swims upon olive oil; effects of the distillations of water in various vessels; of fulminating powders; of the oil of tobacco; of the torpedo fish; of water-newts, eagle-stones, &c.; of the strong digestive power of fowls; of the blood and horn of the rhinoceros, of stags horns; of pimento, Chinese fennel, sassafrass, Peruvian bark, ginseng, &c.

*Description and Management of the Cacao-tree.** N° 93, p. 6007.

The body of the cacao is about 4 inches in diameter, 5 feet in height, and above 12 from the ground to the top of the tree. These trees are very different from each other; for some shoot up in 2 or 3 bodies; others in one. Their leaves are many of them dead, and most discoloured, unless on very young trees. The number of cods the tree produces is uncertain: but we reckon a bearing tree yields from 2 to 8 pounds of nuts a year; and each cod contains from 20 to 30 nuts.

Cacao was originally of these [the West] Indies and wild. Towards Maracajo are divers spots of it in the mountains; and I am informed, the Portuguese have lately discovered whole woods of it up the river of Maranon.

Inquiries concerning Stones and other Materials for the Use of Building; and on the Art of hardening and tempering Steel for cutting Porphyry and other hard Marbles. N° 93, p. 6010.

There is a sort of grey free-stone at Paris, every where on the south-side of the river Seine, rather of a coarse grit, and so soft when first taken out of the quarry, that it is dressed and hewn with broad sharp axes, almost as easily as dried clay; but it grows harder and harder in the air, very durable, and fit for building. The Portland stone is of a fine chalky grit, fit for all curious hewn and carved work, though not for water or fire. On the contrary, the free-stone in Kent, is of a whitish grey colour, lasts well in air and water; its grit less fine and chalky, than that of Portland. The Derbyshire freestone, though it endure the fiercest fire, is brittle, and consequently unfit for fine and curious workmanship.

Concerning marbles. Query, whether Salisbury marble be a true, though coarse, natural marble? Whether blue marbles, coming much from Genoa and Leghorn as ballast, be harder than white marble, but take not so good a polish?

* The *cacao*, or chocolate-tree, is the *theobroma cacao* of Linnæus, and is elegantly figured in Madam Merian's celebrated work on the Surinam insects, pl. 63. It is also represented in Catesby's Carolina, appendix, pl. 6.

Whence comes the best black marble? Whether porphyry differ in nothing from marbles, but in hardness?

Query, the ways of making artificial marble; and whether that with which the Elector of Bavaria has adorned his whole palace at Munich, so as to look as rich and beautiful as any palace in Italy, is made, as some affirm, of such gypsum as composes the plaster of Paris, which being put over the fire and let boil till it cease of itself, after being cooled is kept dry for use; mixing painters colours with it for tinging or colouring it according to pleasure, and using it as the burnt gypsum is at Paris? Beside several other inquiries concering stones, quarries, coal, &c; and then concludes, that it would be useful to retrieve the art of hardening and tempering steel for cutting of porphyry, &c.; which the Egyptians were masters of, of old, and after them the Greeks and Romans: insomuch that the neat and curious hewing and carving of obelisks, colosses, statues, pots, urns, as also porphyry and other hard marbles, is now the object of admiration to the most skilful workmen, who know not which way to rough hew stones of that untractable hardness.

On the Advantage of Virginia for Ship building, by a Gentleman. N° 93, p. 6015.

The country of Virginia abounds every where with large and tall oaks, of at least 50 or 60 feet in height, of clear timber, without boughs or branchings; being very fit to make plank of any size, very tough, and excellently well enduring the water.—With abundance of pines for masts, no country, that we know in the world, is better stored than Virginia. Besides there is another sort of wood, called cypress, which is far better than any pine for masts, it being of as tough and springy a nature as yew-tree; bending beyond credit; when dry, much lighter than fire, and so well lasting in wet and dry, that it seems rather to polish than perish in the weather.—The same country affords great abundance of old pine for making rosin, pitch and tar.—The conveniency of planting hemp for cordage and sail-cloths in that country is so great, that England might in a short time be supplied, without being beholden to other nations for it.—To these particulars add the great abundance of iron-stone in Virginia, which has already been tried and found very good; the conveniency of wood and limestone being a great inducement to the making of iron, which might be done at a much less rate there than here.

To make Vines grow to advantage, all over the Roof of a House. By Mr. John Templer. N° 93, p. 6016.

He lets vines ascend by one single stem to the eaves of his house, cutting off all the luxuriant branches by the way; then gives them liberty to spread upon the tiles, all over one side the roof of his house. Thus he furnishes his dwelling house, and many out-houses; by which means the vines are no hindrance to the other wall-fruit, and the rays of the sun being almost direct upon the vines, he has riper, sweeter and greater plenty of grapes, than when the vines are placed as wall trees.

On the Motion of the Hearts of two Urchins, after their being cut out. By Mr. John Templer. N° 93, p. 6016.

I cut out the hearts of two urchins; whereupon I found the systole and diastole to continue full two hours, while the hearts lay upon a glased earthen white plate, in a cold window. The distance of their diastoles was unequal in time, but very large for half an hour, and then sensibly diminishing until they ceased at the two hours end; and would not then be reinforced by a needle's point, which for the half hour preceding they would answer at any time.

After the hearts had ceased above $\frac{1}{4}$ of an hour, so as a needle pricking them caused no motion; yet upon setting the plate on the hearth in the chimney, in about two minutes they began to beat, though but weakly; and upon eight minutes continuance they beat freely; and when removed into the window again, continued their pulsation, without pricking, above an hour; and might have done longer, could they have been attended to. Perhaps we may hence conjecture the cause of life and death. And when shall we say then, any animal or insect is dead if it has motion?

Observations on Turkey. N° 93, p. 6017.

A disease reigns in the country about Aleppo, and as far as Bagdat, that attacks both sexes, all ages, and strangers as well as natives. It is commonly called Il mal d' Aleppo, and appears to be in the skin a small pustule, hard and red, the head scarcely larger at the beginning than the point of a pin; afterwards growing, and being nourished by five or six little roots or fibres, it goes on to its height for the space of about 6 months; and in as many more declines again. So that the whole period of this disease is generally comprised within the space of one year. This pustule has hitherto yielded to no remedies, neither in the beginning, middle, nor decline. It is wholly to be left to nature; and by doing so, there is no pain or trouble in it. It affects people not once only but often, and it seizes on several parts of the body; and when on the face, causes a remarkable scar, which yet by little and little vanishes.*

* The cause of this species of cutaneous disease remains still a matter of conjecture. Is it produced by some animal belonging to the tribe of insects or worms? Dr. Russell, in his history of Aleppo, has mentioned the mercurial plaster as the best remedy against it.

As to fevers at and about Aleppo, though they have the same type there as in England; yet there are two things peculiar in them. One is, that in acute fevers, cold sweat commonly signifies recovery, but hot sweat portends death. The other is, that in such acute fevers even an intermitting pulse denounces no danger.

The leprosy, which anciently was so frequent a malady in these countries, is now scarcely to be found there; though at Damascus there is still an hospital standing, formerly built for the relief of persons so diseased.

The reason why the city of Constantinople, is so much subject to the plague is, according to some, owing to the multitude of slaves, yearly brought by the Black Sea, and their hard diet and usage: according to others, it is owing to the common people, feeding for the greatest part of summer on cucumbers and melons, and drinking water upon them, without the use of helps to correct the crudities. But the physicians generally conclude, that the air of Constantinople is infected by the north-east winds, which blow commonly for 3 months, beginning about the summer solstice, arising from unwholesome marshes in Tartary and Muscovy.

Besides the other uses of opium in Turkey, it is common in Arabia, to cure horses with it of the griping of the guts.

As to the way of dressing leather in Turkey, it is to be observed, that their leather is not so strong and serviceable as that in England. And though it be commonly said, that the leather in these parts, though thin and supple, will hold out water; yet this is to be understood, that the Turks in their winter-boots, between the lining and the leather, put a sear cloth; which being curiously sowed in the seams, will keep out water, though you put them in it for some hours together. In cleaning their leather, they use lime and album Græcum; and instead of bark of trees, they employ valonia, a sort of acorn. I am persuaded that our acorns in England, if they could be spared for it, would perform the like effect, and perhaps better; seeing that the valonia often burns the leather so much as to make it almost useless.

An Account of two Books. N° 93, p. 6019.

I. Vini Rhenani, imprimis Baccaracensis, Anatomia Chymica, á Joh. Davide, Portzio Phil. et Med. D. Heidel. 1672, in 12mo

The processes followed in the making of foreign wines are detailed in so many publications of modern times, that it cannot be necessary to lay before our readers the description here given of the method of making rhenish wine.

II. De Poematum Cantu et Viribus Rythmi. Oxon. 1673, in 8vo.

The author of this treatise, who is the noted Isaac Vossius, endeavours to prove, that the music of the ancients is far to be preferred to that of our age, forasmuch as speech, how powerful soever at this day, yet, when put into a song, or rendered musical, is not of that efficacy in moving our senses, as it was in times of old. The reasons of which he endeavours to explain.

Among many other remarks, are the following curious ones. He laments the cessation amongst musicians, of that great power of moving the affections, for more than a thousand years ago. Observing further, that those motions have so great a power, that, even without any voice and sound, they can raise affections more strongly than any voice or oration. For the proof of which he alleges the ancient pantomimi, whose feet and hands he makes no less eloquent than the tongues of orators; witness Cicero, who used to contend with Roscius, the stage-player, which of the two should most vary the same sentence, the one by words, the other by gestures; hence our author affirms, that if we employed as much labour and time in learning the pantomimical art, as we do in learning a language, we might possibly come to express our mind and thoughts as clearly by that way, as now we do by the aid of a language: nor does he think that mankind would suffer any thing by it, if the pest and confusion (these are his own words) of so many tongues were banished, and, instead of them, this sole art of the pantomimes were known by all mankind, and men explained every thing by signs, nods, and gestures; on account of which he thinks the condition of brutes to be much better than that of men, seeing they signify without an interpreter their sense and thoughts more readily, and perhaps better, than any men can do.

Also, the skill of exploring the internal affections of the body by touch alone, as we perceive the external motions by the eye. Where our author exceedingly commends the skill of the Chinese physicians in finding out, not only that the body is diseased, (which he says is all that our practitioners know by it) but also, from what cause or from what part the sickness proceeds. In short, to make ourselves masters of this skill, he would have us labour in exploring the nature of men's pulses, till they become as well known and as familiar to us, as a harp or lute is to the players thereon; it not being enough for them to know, that there is something amiss which spoils the tune, but they must also know what string it is which causes that fault.

On this occasion the editor thinks it will not displease the reader here to inform him, that he lately saw a letter written from Java in the East Indies, mentioning an Indian treatise, much talked of, concerning the art and method of knowing diseases and their events by the sole beating of the pulse: and that some curious persons in that island had already written to some religious men in China, desiring them to spare no pains in procuring it: to promote which a sum of money had been sent thither, for a reward to the translators.

The contemptibleness of the modern music to him is such, that he says there is hardly so much as the shadow of the pristine majesty of it remaining; wondering that those, who in this and the former age have written of music, have written nothing of the rhyme, or have done it so that they seem to have been altogether ignorant of what it is; regarding nothing but to please the ear; whereas to affect the mind, it is necessary the sound should signify what may be understood by the mind, without which there can be raised no true pleasure, nor any strong affection. He adverts to the excellence of the Chinese music, though that people complain of the loss of their ancient way of singing, which if they do justly, our author scruples not to affirm, that their music must have been divine, seeing the present state of it is so excellent, that they may easily silence all the music of Europe. The rare contrivance for rendering even and strong sounds, of the old Roman hydraulic organ, described by Hero and Vitruvius, and explained by our author, and by him declared to excel our organs, yielding an unequal and weak blast. The art of the ancients in making such tibiæ or pipes of so many different forms and figure as there are kinds of affections; concerning which he affirms, that there is none to be found at this day, that even know how to make such pipes as are able to produce such motions; since our modern artificers, in his opinion, fail not only in the matter of which those instruments are to be made, but also in the proportion which is to be observed in their form.

*Demonstration of the Synchronism of the Vibrations in a Cycloid. By a Person of Quality.** N° 94, p. 6032. *Translated from the Latin.*

Let ab, bc, cd, de, ef, &c. (fig. 1. pl. 2), be mutually equal; and b1, c2, d3, e4, f5, &c. increase equally as the numbers 1, 3, 5, 7, 9, &c. I say, that any heavy body, falling from any point of this line, will arrive at the lowest point in the same space of time, in which it would arrive at it, if the body should fall from any other point of the line. For if you put $a =$ ab $=$ bc $=$ cd, &c, and $b =$ b1, also x for any number of the others: then, putting xa for af, xxb must represent fδ, and then the time of descent must necessarily be $\frac{xxb}{xx\,aa}$ or $\frac{b}{aa}$; and the same holds in all cases. Therefore, &c.

I say farther, that this curve is the cycloid: which is easily demonstrated from the construction, and from what was just now hinted; viz. that this curve

* Lord Viscount Brouncker. See the Paper, more fully demonstrated in Birch's Hist. of the Royal Society, Vol. I, p. 70.

abcdefz is equal to double the last right line, $2z\omega$; and that $a\omega$ is equal to half the circumference of the circle, whose diameter is $z\omega$; also universally, that the triangle ♈ ♉ ♊ represents the right line $z\omega$; and the square ♈ ♉ ♊ ♋, the curve abcdefz; and the quadrant ♈ ♉ ♋ represents the right line $a\omega$; and the parts of one respectively the parts of the other. As, if ♈♎♍ represents f♌, then ♈♎♌♋ will represent a♌, and ♈♎♏♋ will represent af.—I say lastly, that a ball suspended by a string of a proper length, and vibrating between two cycloids, describes or moves in a cycloid. Consequently these vibrations are synchronous. Q. E. D.

Observations of Jupiter's Transits near some Fixed Stars; useful for determining the Inclination of that Planet to the Ecliptic. By Mr. Flamsteed. N° 94, p. 6033.

The inclosed paper contains some observations of Jupiter, which being made from a more convenient station than I commonly have used, are more accurate than my former ones: and the planet being in a fit place of his orbit, they are the most useful for determining his inclination to the ecliptic, that we can again expect these six years, or perhaps before he returns again to this place. Had the latitudes of the fixed stars of Tycho's constitution been exact and coherent, we should easily have determined the precise quantity of this inclination, and those regular inequalities we find in this and in all the other planets, which are irrepresentable by numbers, only by reason of some latent errors in the places and latitudes of the fixed.

It would be a task deserving the pains and accuracy of the learned Cassini, and of all others that have good observatories and instruments, to endeavour the restoring of the fixed stars, especially of those that are near the ecliptic. Had I only a convenient place for observing, a ready assistant, and other necessary accommodations, I should not doubt in a few nights to rectify many of Tycho's errors; and to add some stars to his catalogue, as well visible to the bare eye, yet omitted, as telescopical ones.

I have made lately some observations of the utmost elongations of the three innermost satellites; which I find greater than Signor Cassini states them, but almost the very same with Mr. Townley's. But I have just cause to suspect some eccentricity in the third; for I find its elongation greater on the one hand of Jupiter, than on the other. I intend, at another opportunity, to make more trials as carefully as I can, either to confirm or destroy this observation.

The observations are omitted, as not now of any use.

Some Observations made by a Microscope, contrived by M. Leewenhoek in Holland, lately communicated by Dr. Regnerus de Graaf.* N° 94, p. 6037.

1. The mould upon skin, flesh, or other things, has been by some represented to be shot out in the form of the stalks of vegetables, so as that some of those stalks appeared with round knobs at the end, some with blossom-like leaves. But I observe that such mould shoots up first with a straight transparent stalk, in which stalk is driven up a globous substance, which for the most part places itself at the top of the stalk, and is followed by another globule, driving out the first either sideways or at the top; and that is succeeded by a third and

* Antony Van Leewenhoek, so highly celebrated for his curious microscopical observations, was a Dutch gentleman, of Delph in Holland. He was born in the year 1632, and died in 1723, aged 91 years. Leewenhoek was not, properly speaking, a man of letters,* but from the extraordinary assiduity with which he pursued his researches into the minuter parts of Nature, and the striking novelty of the curious observations which he published, his name is perhaps more frequently quoted by philosophers and naturalists, than that of any other writer of his time. This celebrated observer had the good fortune to live at a period, when the instrument, by which he obtained his fame, was yet in some degree in its infancy. He applied himself with unremitted care to the grinding and polishing into a state of perfection the simple lens, as being the best calculated for accurate investigation; and less liable to those deceptions which a composition of glasses sometimes occasions. So many and so extraordinary were the discoveries of Leewenhoek, that he may be said to have brought into view a new world in science; and such was the general truth and fidelity of his observations and descriptions, and the respect paid to his communications, that he has been not unaptly complimented with the title of the *Delphic Oracle*.† Yet, if his works be inspected with critical attention, they will be found by no means free from considerable errors; and he sometimes appears to have deceived himself in a very singular manner. Thus, according to Boerhaave, he once maintained that the veins had a pulsation and the arteries none. He is said to have been a person of amiable manners, and of great integrity of character. At his decease he bequeathed, as a legacy to the Royal Society, a curious collection of some of his best microscopes, to the number of twenty six. These were all prepared by his own hand, and were mounted in small silver frames. A description of them may be found in Vols. 32 and 41 of the Philosophical Transactions, as well as in Baker's work, entitled *Employment for the Microscope.*

* This seems admitted by his panegyrical poet.

"Quoque magis mirer, duris exercita fatis,
"Arctaque, nec studiis apta juventa fuit."

It is also confirmed by a letter from a Mr. Molineux, inserted in the 4th volume of Dr. Birch's History of the Royal Society, in which Leewenhoek is expressly said to have understood no language but his own.

† "Rursus apud Batavos fundunt oracula Delphi;
"Hic habitat Phœbus, Græcia muta jacet.
"Hic habitat Phœbus; non iste per ora Sibyllæ
"Doctus apud vanos non nisi vana loqui:
"Clarior et melior, solidas qui condidit artes,
"Delphos ingenii fertilitate beat."

Leewenhoekii, Opera Omnia,
Lugd. Batav. 1722.

more such globules; all which make up at last one great knob on the stalk, a hundred times thicker than the stalk itself. And this knob indeed consists of nothing else than of many small roundish knobs, which being multiplied, the large knob begins to burst asunder, and then represents a kind of blossoms with leaves.

2. The sting of a bee I find of a different form than has been described by others. I have observed in it two other stings, that are lodged within the thickness of the first sting, each having its peculiar sheath.

3. Further, I observe, on the head of a bee before, two artus or limbs with teeth, which I call scrapers, conceiving them to be the organs with which the bee scrapes the waxy substance from the plant. Besides, I find two other limbs, each having two joints, which I call arms, with which I believe this insect performs its work and makes the combs. There is also a little body which I call the wiper, being rough, and exceeding the other limbs in thickness and length, by which I am apt to believe the bee wipes the honey substance from the plant. All which five limbs the bee, when at work, lays in a curious manner close under her head, in very good order.

4. As to the eye of the bee, which I have taken out of the head, exposing its innermost part to the microscope; I find, that the bee receives her light just with the same shadow as we see the honey-combs: whence I collect, that the bee works not by art or knowledge, but only after the pattern of the light received in the eye.

5. In a louse I observe indeed, as others have done, a short tapering nose with a hole in it, out of which that insect, when it will draw food, thrusts its sting, which, to my eye, was at least five and twenty times less than one single hair. But I find the head every where else very close round about, and without any such sutures as some have represented it. The skin of the head is rough, resembling a skin that has many dents in it. In the two horns I find five joints, others having marked but four. One claw of her foot is of the structure of that of an eagle, but the other of the same foot stands out straight and is very small; and between these two claws there is a raised part or knob, the better to clasp and hold fast the hair.

Extract of a Letter from M. Denys, at Paris, announcing an Admirable Liquor, instantly Stopping the Blood of Arteries pricked or cut, without any Suppuration, or without leaving any Scar or Cicatrice. N° 94, p. 6039.

We are now busy, by the king's order, in making experiments, whence the world is likely to receive great benefit. There has been found out here an admirable essence, which being applied to any artery whatsoever, stops the blood instantly without any need of binding up the wound. We first tried it on dogs, cutting the crural and carotid arteries, and the thigh itself; and the blood stopped in less time than it needs to read this letter. This remedy is not corrosive; the wound healing without any scar, suppuration, or cicatrice. We have also made trials on men, of whom the temporal arteries were opened, and on others whose hands and face had been cut: and they succeeded equally with them.

You may judge how useful this essence is likely to prove in armies, where most men die for want of a good remedy to stop the blood. This liquor works not only outwardly, but also being taken inwardly; for it stops the loss of blood in fœminis, inveterate fluxes of blood, open hæmorrhoids, and other hæmorrhages. Now that this remedy has been well tried in the presence of all the court, and many of our best physicians and surgeons that have admired it; the King has given a privilege to sell it in his armies, and throughout the whole kingdom.*

Of a Certain Powder for rendering Metal smooth and close, and of easier Carriage, &c. N° 94, p. 6040.

This was lately communicated to the editor by a German physician, and is as follows:—

1. The powder I speak of, makes the metal so close and smooth, that it leaves not the least pit in the piece, and that a gun so cast needs no boring. 2. One third of the metal may be spared. 3. Such guns remain clean and neat a long while.

In the year 1672, July 9, there was cast a demy cannon, weighing 34 cwt. This being tried with a bullet of 34 lbs. weight, there was employed the first time 12 lbs. of powder, the second time as much, the third time 15 lbs., and the fourth time 24 lbs. strong powder; all which trials it indured very well. Besides, not long since there was cast a small petar of only two lbs. of this metal, with which I broke in pieces a beam of a foot square, the petar remaining entire and perfect.

When there is occasion to carry these pieces over land, there will not need so many horses by far as usual. And in great ships, that are sometimes mounted with 100 guns each, of the matter of 200 you may make 300 guns, performing the same if not a better effect. It is not only easy to make, but also of small expence.

* Various styptics have at different times been proposed for stopping hæmorrhages, both internal and external, but where the latter arise from a puncture or division of a large artery, no remedy can be safely trusted to, but that of passing a ligature round the ruptured or divided vessel. It will be seen, however, in the subsequent pages, that the styptic here mentioned was in great repute for a time.

Extracts of Two Letters from Dr. Swammerdam, concerning some Animals, that having Lungs are yet found to be without the Arterious Vein, (Pulmonary Artery), together with some other curious Particulars. Dated Amsterdam, Jan. 24, 1673. Translated from the Latin. N° 94, p. 6040.

In my late dissections I have met with animals, which, although they are provided with lungs, are nevertheless destitute of the vena arteriosa (pulmonary artery); so that the blood is sent directly from the heart to every part of the body, without previously undergoing a circulation through the lungs.*

I have been much astonished to find, that the genital organs in the scarabæus nasicornis, exactly resemble those of man, in respect to the vasa testicularia; which consist of a single tube of great length, formed into innumerable convolutions; with this difference however from the human subject, that the said tube is closed at its beginning or apex, (principio seu apice cæco). Thus throughout every part of the creation, even in the meanest of animated beings, the marks of Divine Wisdom, and of the most exquisite workmanship are apparent.

[Thus far Mr. Swammerdam in his 1st letter; whereupon being desired to mention those animals that are destitute of the vena arteriosa, (pulmonary artery), he very obligingly sent in a 2d letter, dated Amsterdam, March 14, 1673, the following observations:]

—— As I perceive that my communications are not unacceptable to the Royal Society, I readily comply with their request; and the more so, as I wish to give others an opportunity of investigating a subject so interesting, that thus the hidden treasures of Nature may be the sooner brought to light.

No person, I imagine, after what has been published by Malpighi and Needham on this subject, will deny that frogs have lungs, and that they respire. Yet have they no vena arteriosa: hence their blood is not circulated through the lungs, but is sent directly from the simple sinus of their heart to every part of the body, without having any thing to do with the lungs. This I conceive to be a good argument among others, for restoring, (as I mean to do at some future time), the office of sanguification to the liver.†

But there is evidently an artery (analogous to the bronchial or rather the pulmonary artery), in the investing membrane of the lungs in frogs. This artery is

* It is true, (as Haller observes), that frogs have no artery going directly from the heart to the lungs; but they have evidently pulmonary arteries which arise out of the aorta.

† As these animals have a pulmonary artery, which arises out of the aorta, this argument does not hold.

distributed over the surface of their lungs in a wonderful manner, so as to resemble a beautiful net-work; and its minute branches strike into the interior vesicles, where the eye may trace the anastomosings with the pulmonary vein. This vein is twice as large as the artery. It is situated in the cavity of the lungs, (in pulmonum cavo), and especially in the margins or borders of the vesicles, from whence its capillary and almost invisible ramuli are distributed to all the cells, and even to the investing membrane itself.

The animals which I suspect to possess the same pulmonary structure with frogs, are toads, lizards, serpents, the chamælion, tortoises, the salamandra aquatica, &c. but these I have not yet had opportunities of dissecting and examining. I must therefore be contented with having pointed them out to other abler inquirers.

As I perceive you were pleased with my observations on the organs of generation in the scarabæus nasicornis, I have thought it right to transmit to the Society a drawing thereof; wherein are represented the testicles composed of a single tube, 2 feet 6 inches long; the vasa deferentia, out of which drops a quantity of white seminal liquor, whenever they are cut or punctured; six vesiculæ or rather glandulæ seminales, extremely elegant; and the ducts which proceed from the glandulæ seminales, containing a yellowish seminal liquor, as in men and brutes.

An Account of some Books. N° 94, p. 6042.

I. La Statique, ou La Science des Forces Mouvantes, par le P. Ignace Gaston Pardies. A Par. 1673, in 12mo.

The learned Author of this book had proposed to himself to write a whole body of mechanics, such a one as might be adapted to ordinary capacities; conceiving that there had not been extant hitherto a complete system of that science; or if there had, it was beyond the reach of most readers: which latter he thinks to be the character due to Dr. Wallis's 3 vols. de Motu et Mechanice. But since the publication of this part of it, we understand that he has been prevented and cut off by an untimely death; being regretted by those that knew his frankness and strong inclination to promote philosophical knowledge.

II. Antonii le Grand Historia Naturæ. Lond. 1673, in 8vo.

The learned author of this book, desirous to show that even the common and obvious phænomena of nature can be very well explained and accounted for by those principles he had formerly laid down, and published An. 1672, under the title of Institutio Philosophiæ, described in Numb. 80 of these tracts; he undertakes in this treatise, to pass through the whole body of physiology, and in so doing, to supply in due places what he has omitted in the said institution.

III. The Description and Use of Two Arithmetic Instruments, &c. By S. Moreland. London, 1673, in 12mo.

The ingenious author of this book, having some years since contrived two instruments, whereof the one is for Addition and Subtraction, the other for Multiplication; gives us here both a description of the parts and structure of these instruments, and the way and manner of using them.

IV. A Brief Account of some Travels in Hungaria, Servia, Bulgaria, Macedonia, Thessaly, Austria, Styria, Carinthia, Carniola, Friuli, &c. By Edward Brown, M. D. of the College of London, R. S. and Physician in Ordinary to his Majesty, London, 4to.

The learned Traveller gives so good an account of the voyages he made through those parts named in the title, that thereby he excellently instructs others what great benefit may be made by travelling, if performed with curiosity and judgment.

Experiments made at London, with the Liquor mentioned in Numb. 94, (sent out of France), for Staunching the Blood of Arteries and Veins. N° 95, p. 6052.

Mr. Wiseman * laid bare the jugular vein of a dog, and opened it with a lancet, and immediately applied to it a button-pledget of lint dipped in the liquor: he opened likewise the carotid artery, and applied a pledge after the same manner. These pledgets being kept on by the pressure of the thumb about a quarter of an hour, were then taken off; the vessels bled but not freely: whereupon the pledgets were changed for fresh ones, and kept on a quarter of an hour more; being then first left loose, and afterwards taken off, the vein and artery were united.

The same day a young woman's breast being cut off, the arteries were stopped by holding the like pledgets in their mouths, while the dressings were fitted for the breast. The pledgets being then thrown off, the blood continued staunch, and the mouths of the arteries remained close.

While this latter operation was performing, a patient, whom Surgeon Wiseman had newly dressed with a caustic stone in the neck, on some scrophulous swellings, was brought back to us in a coach, having bled all the way, to the wetting of almost a whole sheet. The vessel lay so deep that it was hard to reach it. However Mr. Wiseman dipped two pledgets in the liquor, and thrust

* Wiseman ranks among the earliest and best English Surgeons. He was Surgeon to Charles II. His Chirurgical Tracts, making a folio volume, were published in 1676; and besides gun-shot wounds and the various surgical diseases, they contain excellent descriptions of scrophula and lues venerea.

them into the two orifices whence the blood came. It was immediately stopped, and the neck dressed up without any considerable bandage.—Walter Needham.

June 11, 1673. Another experiment was made by Dr. Needham, before the R. S. A dog's crural artery was cut quite across with an incision knife, the blood gushing out copiously, a lint dipped in the said liquor was applied to the wound, and held upon it a little while; when, by reason of the great glut of blood, that could not be well wiped away for want of a sponge, the lint was changed for a fresh one dipped in the liquor, and kept on about half an hour; and being then left loose, the blood was soon staunched, the dog being then unbound, licked the wound, and walked away without any ligature, and is still found alive and well.

June 18, 1673. Another trial was made before the same Society by Denys himself. In the crural artery of another dog was made an oblique wide cut, and the liquor in the usual manner being applied to it, the blood was staunched in 7 min. and the dog being then let loose, but yet kept quiet for 23 min. longer, he then rose and let fall the applied compress, and went away without any bandage.

To these experiments were added some that were made at Whitehall, before the King, on two calves, by opening the crural artery, and by cutting off a leg; in which the blood was completely staunched in about a quarter of an hour.

Experiments made at Paris with the same Liquor, as described by M. Denis. N° 95, p. 6054.

This author says, that this essence has none of the usual dangerous inconveniences; it causes no pain at all; being applied to the wound, it easily penetrates through the flesh, to find out and close the mouths of the arteries, without any necessity of cutting away any thing, as you must do, if you will convey other remedies thither; and the effect of it is so quick, that in about a quarter of an hour, if it be well and exactly applied, the operation is performed; nor need you stay several days for the falling off of the eschar to be assured of the staunchness of the blood.

The experiments he recites are these:—

The crural artery of a dog was pricked with a lancet, presently a button-pledget dipped in the essence was put on the wound, and kept on by some lint laid over the pledget, and held on by the thumb. In less than half a quarter of an hour the artery was so closed, that not a drop of blood issued on removing the pledget.

Two days after, the other crural artery of the same dog was laid open, and having cut it quite through with scissars, a compress of lint dipped in the liquor was immediately applied to it; and half a quarter of an hour after, the compress being removed, the wound was found very dry, and the artery yielded not a drop of blood.

Of another dog, the flank was cut with a pen-knife, which penetrated into the liver. The same knife was run into the groin of another dog, and a vein, nerve, and artery cut together, to imitate the slashing of a sword, and to see the effect of this sanative water in that case. All these wounds were speedily cured, without any ill accident to those animals.

To see what this essence would perform in cases of whole limbs quite cut off, which sometimes must be done to prevent gangrenes; a dog was taken and one of his legs altogether cut off, and a compress of lint, wetted with the essence, laid upon the cut veins and arteries. At the end of a quarter of an hour the compress was taken off, and also the bandage, that held it against the stump of the leg cut; and the blood was found so staunched, as if no vessel had been opened in that part.

These experiments having so well succeeded on brutes, and been repeated over and over with the like success, no scruple was then made to try the liquor upon men. First, there were opened veins in the arms, as is done in ordinary phlebotomies, and a lint dipped in the liquor having been held on the wound half a quarter of an hour, the veins were found as perfectly closed, as is usual in the common way after 24 hours. Another being bled in the temporal artery, and the like application made, he went abroad, and took a turn in town without any compress, or bandage, the artery never opening again.

The same essence has also been very successfully used in fluxes of blood, giving it at the mouth in ptisane: and surprising effects of it have been seen in cases of bleeding at the nose: seeing that as soon as a pledget of lint, moistened with this liquor, was put into the nostrils, the blood was stopped.

To show further that extraordinary quality of this essence, Mr. Denis observes, that it heals wounds without any visible cicatrice, and without any suppuration, saying, that by the same property it has of staunching blood, it not only closes the orifices of vessels opened, but it likewise so constricts the pores of the fibres of the flesh uncovered, that it suffers no air to enter, nor any humour to extravasate out of the wound: and by thus defending a wound against all the alterations that may ensue either from without or from within, it preserves from all suppuration, and keeps the flesh entire; and the wound closing without any loss or reproduction of substance, we need not wonder, he says, that it is done in a short time, and without an apparent cicatrice.

He affirms to have received news from Calais, that an officer of the ship called le Tonant, having his shoulder broken by a cannon-bullet, was carried into an hospital, where the axillary artery together with his arm being cut off, the blood of it was stopped with nothing else but a lint dipped in this liquor; left fastened on to the artery till next morning. He adds, that from Maestricht also he is informed, that with the same liquor the blood of a leg had been stopped, the half of which had been carried away by a cannon-ball.

M. Slusius's Demonstration of his Method of Drawing Tangents to all Curves.* N° 95, p. 6059.

My method is the same as I have used for many years, and by help of which I have shown how to determine the contrary flexure of curves, and the limits of problems in my Miscellanies, and also, if I remember right, formerly in my letters to you. How I discovered it, and in what way it is demonstrated, you will easily understand. Indeed I have announced to a few others, like as I did formerly to you, and in the common characters, that I perform it by certain lemmas; which are as follow:

1. The difference of two powers of the same degree, divided by the difference of their roots, gives the several members of the next inferior power of their binomial; as $\frac{y^3 - x^3}{y - x} = yy + yx + xx$. This instance is chosen among many others, as is easily shown.—2. There are as many terms in any power of a binomial, as there are units in the exponent of the next higher power; viz. three in the square, four in the cube, &c. As is commonly known.—3. If the same quantity be divided by two others, of a given ratio, the quotients will be reciprocally in the same given ratio—which is evident.

By these lemmas my method is demonstrated; which you will soon accomplish, as I have disposed them in such order as leads one, as it were by the hand, to the demonstration.

On the Veins in Plants. By Dr. Wallis. N° 95, p. 6060.

As to the veins of plants, which Mr. Lister observes (N° 90) are not ramified, but rather bundles of them divaricated; in this they resemble the nerves, which (as Dr. Willis de Cerebro observes) go together in that which seems the common trunk, like a bunch of threads, which afterward separate and are variously divaricated; and these nerves, being cut, shrink up, as the veins of plants, as much or more than the veins or arteries of animals.

Dr. Wallis observes also, that there are two sorts of nerves; one arising from the Cerebrum, subservient to voluntary motions, and of which we are conscious or take notice, and which properly belong to the functions of the

* Given in No. 90. p. 38.

sensitive soul, at least to the functions of sense; the other from the Cerebrum, subservient to the involuntary motions, and of which we are not conscious or sensible, and which belong rather to the functions of the vegetative soul, (nutrition, &c.) or at least the insensible loco-motive faculty: and to these latter seem reducible those acts of sense, which Mr. Lister speaks of in plants. See Dr. Willis de Cerebro, c. 19, p. 241, edit. in 4to, and c. 15, p. 187.

On the Whiteness of the Chyle within the lacteal Veins; with divers particulars observed in the Guts, especially several Sorts of Worms found in them. By Mr. Lister. N° 95, p. 6060.

I have long wished to discover the actual passage of the chyle into the lacteal veins; of which yet I never doubted, as I find some do at this day. The difficulty lies in the certain and unalterable character of the chyle's whiteness, especially when received into those veins. And yet it is as certain, that in a diabetes the urine retains all the qualities of the liquor drunk. Also in that famous instance of those that eat the fruit called the prickle-pear, their urine has affrighted the eater with the colour of blood, that is, with the unaltered colour of the juice of the fruit. In these instances at least we cannot doubt but the chyle, even in the lacteal veins, was qualified according to the food and drink.

To effect then something to this purpose, we have formerly, and that very often, repeated the experiment of injecting highly tinged liquors into the guts of a live animal. Thus, having laced the skin of the abdomen of a dog loosely for a hand's breadth, and then opening it underneath the stitches, we took out either the duodenum, or any other part of the tenuia intestina. The gut taken out, we opened with a very small orifice, and having ready the tinged liquor lukewarm, we injected it upward and downward: carefully stitching up the gut, and then drawing the lace, we unloosed two of the dogs feet, laying him on his side for what time we thought convenient. The tinged liquors we used, were good Barbadoes indigo, in fair water, and filtrated; also lumps of indigo thrust down his throat; good broth, as they call it, of a blue vat; indigo in milk, saffron in milk. Again, we tried in some dogs fed before hand, and injected the liquors in the very height of the chyle's distribution; into others yet fasting, and that for a longer or shorter time.

The success was so constant, that we cannot say we ever found the least discolouring of the chyle on the other side the guts, that is, within the lacteal veins, but ever white and uniform. Whence we judge it not very feasible to tinge the venal chyle in a well and sound animal. And he that would demonstrate the matter of fact to the eye, must probably do it by giving him some such thing in the food, as shall cause a diabetes, or some distemper equivalent to it.

Of the guts we shall observe as follows:—1. Of the glandulæ miliares of the small guts, which may also in some animals be well called fragiformes, from the figure of the one half of a strawberry, and which yet I take to be excretive glandules, because conglomerate.—2. 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.—3. Of some sorts of vermin, we found in the guts. And first of the lumbrici lati or tape-worms.* Of these, I say, we found in the guts of one dog, perhaps more than a hundred in all. The duodenum was exceedingly stuffed out and extended with them. Which also well agrees with another observation I made in a mouse, where I found the duodenum to be far larger than the stomach itself, by reason of the great numbers of these worms for kind, which were contained in it: for kind, I say; for these tape-worms were of a quite different shape from those of the dog, or any that I have ever yet seen. To proceed, we found them also in the dog's jejunum and Ileon; but not any one lower than the valvula coli, nor any higher than the duodenum or within the pylorus. Below the duodenum they lay at certain distances one from another, though sometimes by pairs or more of them twisted together. Near them was constantly to be observed an excrement of their own, distinct, for colour (more grey) and consistence, from the chyle, (the observation being made in a dog plentifully fed for other purposes;) just as we find in worm-eaten tracks of wood, where the cossi leave behind them the wood which has passed through their bodies: these worms lay mostly with the small ends upward, as feeding upon and expecting the chyle in its descent. These lumbrici lati were none of them above one foot long, and most of them of an equal length and size. The one end was as broad as my little finger-nail, and pointed like a lancet; the other end, coming small gradually for the third part of the whole length of the animal, was knotted, or ended in a small button like a pin-head. They were every where and in all parts of them alike milk white, of a flat and thin substance, like fine tape, divided into infinite rings and incisures; each incisure having sharp angles, on both sides, looking to the broader end standing out beyond each other: from which also I take the small end to be the head; else the sharp corners of the annuli would

* The worms here mentioned belong to the Linnæan genus tænia, which comprehends a great number of species. These have been severally described and figured in the writings of Pallas, Block, Goetze and Werner; and much new light has been thrown upon their structure and economy by a gentleman well skilled in comparative anatomy and physiology, Mr. Carlisle. See Linn. Trans. vol. II.

necessarily hinder the ascent of the animal; whereas, if the contrary be true, they serve to keep it up. Each ring has also on the one side only, and that alternately, one small protuberance, much like the middle feet of the body of some caterpillars.—I was not so happy as to discover any motion in any part of them, in water or out of it, nor did they seem, if pricked or otherwise hurt, much (if at all) to contract themselves or shorten the annuli, so that they then appeared to me as things without motion or sense.

There is another sort of lumbrici lati to be met with very frequently also in dogs, called cucurbitini, from the likeness each annulus or link has to a cucumber seed. I have sometimes found them about half a foot long, but more often broken into shorter pieces. The former by us described is undoubtedly a complete and entire animal; but there is great reason of suspicion, that this is a chain of many animals linked together. These animals for kind have been observed to have been voided by men, and found enclosed in a gut or membrane of a prodigious length: and a person of great integrity and worth, Mr. T. I. affirmed to me, that he once assisted at the opening of a dog, in which one of the kidneys was observed to be quite wasted, and become a perfect bladder, and in that bladder they found something like an animal of a monstrous shape, which being dissected, was nothing else but a skin full of these lumbrici cucurbitini.

And because worms of this sort are sometimes said to be found out of the guts, their most proper place, we shall conclude with a very recent observation of the last month in this city. A surgeon brought me about 20 worms, which he had just then taken out of an ulcerated ancle of a girl of about eight years old. I had the curiosity to go myself and see it. I found the leg sound, all but the ancle, which was vastly swelled, and the girl otherwise hearty and well coloured. She had been in great misery for some months; had been sent up to London, where she was touched and dressed for the evil. Sometime after her return, her pain continuing, a young puppy was opened and applied to the sores. The surgeon, who took off the puppy, found it full of worms, at least 60 in number, taken from the body of the puppy and from the sore ancle together, into which, he said, they crawled down as worms do into the ground. The same puppy was again applied, and it was then that I made the visit, and saw only one worm got into the puppy, but a very live and stirring one. Many were afterwards killed by injections. These worms were of the very species of the lumbrici teretes, which children familiarly void from the guts. They were between 3 and 4 inches long; all about the matter, of an equal largeness, as of one brood; something thicker than a duck's quill; very sharp at both ends; stiff and exactly round; without incisures, visible at least, and yet could move and twist themselves readily enough. All the difference was in the colour, these being much whiter than any I have seen from the guts. Vid. Barthol. in Hist. 60, Cent. 5, where near 20 worms, as long as my finger, were found in a lady's arm, probably of this species too.

The Undertakings of Mr. Henry Bond, senior, a famous Teacher of the Art of Navigation in London, concerning the Variation of the Magnetical Compass and the Inclination of the Inclinatory Needle; as the Result and Conclusion of 38 Years Magnetical Study. N° 95, p. 6065.

Mr. Bond can show the cause of the variation of the magnetical needle, or compass, by the motion of two magnetical poles; how these poles are found; and what their distance is from the poles of the earth; what their annual motion is, and from whence it proceeds.—By calculation he finds all the variations that have been observed at or near London, for above 90 years past; and so by consequence it may be found at London to the end of the world.—He has calculated a table to every 5 minutes of the inclination of the inclinatory needle; so that by the needle's inclination, and that table, and the latitude of the place, he can find the longitude of any place in the world.—By that table also he finds Mr. Robert Norman's inclination, which he found An. 1576; and can show, what will be the greatest and least inclination of the inclinatory needle, in any latitude in the world.—He has 4 examples of finding the longitude by the help of the inclinatory needle; one at Balsora in the East Indies, in the year 1657. Another at Cape Charles, on the coast of Virginia before that time. The third at the Cape of Good Hope. The fourth at the Straits of Magellan.

On a pleasant Way of catching Carps. By Mr. John Templer. N° 95, p. 6066.

A gentleman invited me to walk with him to his fish-ponds, and to see a boy throw out carps with his hands, at any time in the heat of the day. I saw four very large ones, that the boy took. His way was this: he waded into the pond, and then returning to the sides, he would grope them out in the sedge or weeds, and, tickling them with his fingers under the belly, quickly remove his fingers to their gills, and throw them out upon the land.*

* This trifling paper is little worth preserving. The "pleasant way of catching carps" relates merely to what is now well known; viz. that carps, and, it might be added, many other fish, in warm weather, are sometimes so indolent as to suffer themselves to be gently handled, and may consequently be easily seized and taken out of the water.

An easy Way of raising Fruit-trees to any Number desired. By Mr. Lewis of Tottenham High-cross. N° 95, p. 6067.

Take a piece of the root of any apple or pear-tree, &c. about 6 inches long, and tongue-graft, a graft of an apple or pear into the root. The way of tongue-grafting is, to cut the root sloping about 1 inch, and the graft sloping in like manner 1 inch; cutting both very smooth. Then cleave the root and the graft likewise about 1 inch, and enter them into one another, that the sap of the graft may join to the sap of the root as much as you can. Lap the jointed place about with a little hemp or flax-hurds; set the root so grafted into the ground about 10 or 12 inches deep, so as the joint may be covered at least 4 inches under the earth, that it may not be bared at any time, but kept moist by the earth.

An Account of some Books. N° 95, p. 6068.

I. Christiani Hugenii Zulichemii Horologium Oscillatorium. Par. 1673, in folio.

This eminent mathematician divides this treatise into five parts; of which, the first contains his description of the pendulum watch.—The 2d treats of the descent of heavy bodies, and their motion in a cycloid, that is, in a line which a nail, fastened in the circumference of a running wheel, by its continued circum rotation designs in the air.—The 3d of the evolution and dimension of curve lines.—The 4th of the centre of vibration.—The 5th of the construction of another watch, in which the pendulum moves circularly; with some theorems of centrifical force.

A simple pendulum being no certain and equal measure of time, since the larger excursions are observed to be slower than the smaller, the other has, by the aid of geometry, discovered a way of suspending the pendulum, by finding out a certain curve line fit to give it the desired equality, which having applied to watches, their motion has by this means been rendered so constant and certain, that by frequent experiments they are now known to be exceedingly useful, both in astronomy and navigation. This being the cycloid abovementioned, our author gives a very accurate demonstration of it, with some new demonstrations on the natural descent of heavy bodies.

But then that this cycloid might be adapted to the use of pendulums, he thought himself obliged to enter upon a new consideration of curve lines, viz. of those, which by their evolution generate other curves. Whence resulted the comparison of the length of curve lines with straight ones.—Besides, for the clearer explication of the nature of the compounded pendulum, he subjoins a speculation on the centres of agitation, or oscillation, in which occur many considerable theorems, relating to linear, plain and solid figures.

To all which he promises the mechanical structure of the watch, and the application of the pendulum to it; enriching that part with his table of the equation of days, as also with a relation of the several successes of such watches employed in considerable sea-voyages; of which he says the best of all has been that in the expedition of the late Duke of Beaufort into Candia; who, having taken with him in his own ship two of those watches, and appointed a good astronomer to take care of them; the longitudes of the places, which they either touched at in that voyage, or which in passing by they could see, were by means of the said watches exactly measured, so as that the very same differences of longitudes were found by the most accurate maps assigned to those places.

But since those trials, our author says he has improved his watches, by using a pendulum of a triangular figure, and by another way of suspending them; of which he gives an ample description. We may also notice his universal and perpetual measure, which he establishes by exactly taking the measure of the distance from the point of suspension to the centre of oscillation of a simple pendulum vibrating a second of time; which being found to be such a length, as being divided into three equal parts, will make such a measure, as he calls an horary foot; which having such or such a proportion to all other feet, may be used to settle a constant and certain measure every where, and to recover it in all ages, as time will be always and in all places the same; and consequently such a length being taken as exactly equals a second of time, a just universal measure is obtained.

II. Modern Fortification, &c.; by Sir Jonas Moore,* master surveyor of his Majesty's ordnance. Lon. 1673, in 8vo.

* Sir Jonas Moore, F. R. S. was a respectable mathematician, besides a good engineer, and became surveyor general of the ordnance, or master surveyor as it was then officially called. He was born at Whitbee, near the confines of Lancashire and Cheshire, about the year 1615, and enjoyed a liberal education, but bent his studies chiefly to the mathematics and astronomy, to which he was always strongly attached. In the expeditions of King Charles the Ist in the northern parts of England, Mr. Moore was introduced to him, as a person learned in those sciences; which laid the foundation of his fortune. In 1647 he was appointed mathematical preceptor to the king's second son, James the Duke of York. During Cromwell's government it seems he followed the profession of a public teacher of mathematics, when his loyalty was a great prejudice to his fortune. After the return of Charles the IId, he found great favour both with the king and duke of York, who often consulted him, and were advised by him on many occasions. And it must be owned that he used to employ his interest with the court to the advancement of learning and the encouragement of merit. Thus, he procured the royal observatory at Greenwich to be built in 1675, recommending Mr. Flamsteed as the king's astronomer there. Also, being a governor of Christ's Hospital, by his interest the king founded the mathematical school there: and for the use of this school Sir Jonas com-

The worthy and intelligent author of this book comprehends in a small volume whatever has been designed and practised by the latest and most experienced engineers of this age, Italian, French, Dutch and English; and the manner of defending and besieging forts and other places; with the use of a joint-ruler or sector, for the speedy description of any fortification. The whole performed by very easy rules.

III. The Elements of that Mathematical Science called Algebra, by John Kersey. Lon. An. 1673, in fol.

A remarkably full and complete body of the algebraic art; perhaps the most so of any book of its time.

Description of a Bee-house, used in Scotland. N° 96, p. 6076.

In fig. 4, plate 2, A represents the bee-house, lying on one side, with the frame BBBBBBBB placed in it; CCCC, the screw pins that hold the frame fast; D, the square hole at top open; E, the windows; F, the door for the bees to go in and out; G, the place by which the knife enters to cut the honey-combs asunder upon occasion; HH, the inward crease at the bottom.

In fig. 5, A, is the bee-house set upright; B, the square hole through which the bees work downward; C, the shutter that covers the hole upon occasion; D, the door for the bees. E, a sliding shutter that covers the door in winter; F, the window; GG, the handles for lifting all; HH, the crease for fastening one bee-house over another.

In fig. 6, A is the frame for the bees to fasten their work upon; BB the screw nails.

The bee-house is made of wainscot, about 16 inches in height, and 23 in breadth, between opposite sides. It has 8 sides, each almost 9 inches in breadth. It is close covered at top with boards, having a square hole in the middle, 5 inches long, and about 4 inches broad; with a shutter that slides to and fro in a groove, about half an inch longer than the hole. It has 2 windows opposite to one another, and may have more, of any figure, with panes of glass and shutters. The door for the bees is divided into three or four holes, about half an

piled his New System of Mathematics, in 2 volumes 4to. In 1673 he was sent by the government in some public capacity to Tangier; and after his return he was advanced to be surveyor of the ordnance, and had the honour of knighthood. In 1674 he became a very useful member of the Royal Society. Sir Jonas died in 1681, the year in which was published his Course of Mathematic, abovementioned. Besides that work, we find him author of several others: as, 1. Arithmetic, vulgar and algebraical, in 8vo. 2d, A Mathematical Compendium; or Useful Practices in Arithmetic, Geometry, Astronomy, Geography, Navigation, &c. in 12mo, the 4th edition in 1705, 3d, Modern Fortification, &c. in 8vo, 1673. 4to, General Treatise of Artillery, in 8vo, 1683.

inch square; with a shutter that slides in a groove to cover them in winter. It has two iron handles with joints, to be placed about the middle, if there be no windows on the sides where they are; or above them if there be. At top it has a crease all round it, about half an inch in depth on the outside, and $1\frac{1}{2}$ inch high; and another on the inside at the bottom, which serves to fix them when set upon one another. It has also a hole about two inches square, on one side at bottom, by which the knife is put in to cut the bees work, that passes through the hole from one bee-house into another, as they work downwards into the empty house; which has a sliding shutter to cover it. Within the bee-house there is a square frame, made of 4 posts joined at top, at bottom, and in the middle, with 4 sticks for the bees to fasten their work upon; which though they will serve, yet it may be securer to have two more added in each of their places crossing the frame, either from the middle of the opposite side-sticks, or from angles where the posts are placed.

This manner of bee-house is useful for preventing the swarming of bees. For when the bee-house wants room for the young bees, they swarm, and fly away to find a house for themselves, which is prevented by placing an empty one made thus, under the full one, having the door at top open, that they may work downwards into it. And when both are full, the bees will all be in the lowest house; and then, to get the honey and wax without destroying or troubling the bees, with a thin long knife, broad at the end, and sharp on both sides, the bees work is to be cut as low as can be, and the uppermost bee-house to be lifted off by the handles, and being reversed, the screws are to be taken out, and then the frame with all the bees work upon it will easily slip out; after which, the empty bee-house may be set under the other, if need be, and the uppermost having the square hole above covered with the shutter, some other cover may be set over it to keep the bees from the injuries of the weather. And if this separation be made in the spring or summer, the bees will like their new house the better that it has been used before.

Account of Further Experiments concerning the Wonderful Effects of the Blood-Staunching Liquor. N° 96, p. 6078.

The King having in his presence caused some considerable experiments to be made with the new blood-stopping liquor on brutes, and there remaining yet some persons here doubting, whether it would as well succeed on men; his Majesty gave order to his chirurgeons to go and see in the hospitals, whether there were not some wounded persons whose blood had need to be stopped. Hereupon there were found two very fit patients in the hospital of St. Thomas. The first was a woman labouring under an inveterate scurvy and the King's evil, whose leg was to be cut off, because of a malignant ulcer, not suffering her to sleep day or night. The other was a seaman, whose leg was also to be cut off, because of a wound accompanied with a fracture made by a cannon-ball in the last sea fight.

The first experiment was made July 3, the King having sent some of his physicians and chirurgeons to the hospital, to be present at the operation, and faithfully to report to his Majesty what should pass there. The leg of the woman being cut off, immediately the arteries were dressed with some linen pledgets dipped in the astringent liquor, with a compress upon it, and a bandage keeping all close against the arteries. The success was, that the blood was staunched without any other dressing; and instead of complaining, as those are wont to do who have a limb cut off, and the mouths of the arteries burnt with a hot iron, or a caustic to stop the blood, this patient looked very cheerful, and was free from pain, and slept two hours after, and also the night following; and from that time has found herself still better and better, without any return of bleeding, or any ill accident.

The 4th of July the leg of the seaman was cut off, and after the part was dressed as above, with linen dipped in the essence, the blood was stopped in less than half a quarter of an hour. There was made a bandage, that pressed the linen against the cut arteries; and without any other thing the patient found himself so eased of the pains he felt before, that he slept two or three hours after, and all the night following.

Next morning the dressings of the woman, as well as of the man, were taken off in the presence of the same persons, and all the physicians and chirurgeons there present acknowledged, that no wounds could look more fair and ruddy; there appearing no eschar at all, nor any more blood than if there had never been any veins or arteries opened in that part. These two patients have found themselves very well ever since the operation; and forasmuch as no ill accident has befallen them since, they have served to convince the most incredulous of the goodness of this remedy.

The King easily concluding, from these and the former experiments, how useful this medicine would be in his armies and fleets, and understanding that those who before opposed it did now highly praise it, gave order that M. Denis should be desired to communicate the secret of it, which being done, his Majesty commanded a quantity of it to be made in his own laboratory, of which trials were made on three calves in Whitehall, the 12th of July; a leg of each of them having been cut off, as high as was possible, and the blood of them stopped with this new liquor, to the admiration of all the spectators. For this water having been prepared with more exactness than ever, the effect of it was

M 2

so quick and effectual, that the blood was stopped in four minutes of time, the calves by their motion making the pledgets to fall off, that had been put on the parts cut, and not a drop of blood appearing.

The King hereupon caused the quantities that had been thus prepared, which were very considerable, to be immediately dispatched away to his Majesty's fleet; and it is not doubted, but that upon occasion all that shall happen to be wounded will receive great relief and benefit by it.

Description of the Prosopographic Parallelogram, for delineating Orthographically, by Parallel Visual Rays, the Attitudes and Gestures of the Human Body, observing exactly the Proportion and Symmetry of the Parts. By Mr. John Sinclair. N° 96, p. 6084. *Translated from the Latin.*

Let ABCD (fig. 2, pl. 2,) be a prosopographic or delineating parallelogram; HF a central style or pin; LC a drawing pencil; RA an index, or oblong ruler, fitted perpendicularly to the plane of the parallelogram, by means of a brass screw nail; on this ruler are fixed two sights, PR, SV; in the middle of PR is a hole O, and in the middle of SV a thread stretched perpendicular to the ruler, on the middle of which is a small globule, by which, and the hole O, a ray from the object reaches the eye, which in delineating must not be fixed, but free and loose.

It is to be observed, 1. That a ray passing through the hole O and the globule, will be always perpendicular to the plane of the parallelogram, or its diameter, which is a right line passing through the pencil LC, and the fixed centre HF, and the said globule; in which line the globule always is, whatever be the motion of the parallelogram. 2. That QYXT is the sensible delineatory plane on which the point L of the pencil moves, describing the image by the motion of the ruler AR, into which plane the central style is fixed; and that $\epsilon\delta\beta\gamma$ is the rational or mathematical plane, being a continuation of the former. 3. That all rays will be parallel to each other coming from the object, by the globule and the hole, to the eye, placed according to the direction of the ruler, in as many points of the diaphanous medium, as there are points in the visible surface of the object to be described, which indeed are infinite; as is thus proved: lines parallel to the same line, though not in the same plane, are parallel to each other, by Eucl. xi, 10; but all the rays extending from the object, through the sights to the eye, are parallel to the same right line, viz. to the ray passing from the object through the globule and the hole O; consequently they are parallel to each other. Again, if two right lines be perpendicular to the same plane, those lines will also be parallel, Eucl. xi, 6; but the primary ray passing from the object through the sights to the eye, and all the other secondary, are perpendicular to the rational or mathematical plane $\varepsilon\delta\beta\gamma$, by the hypothesis; therefore all the rays, passing from the object through the sights to the eye, are parallel to the same right line, viz. to the ray passing through the globule and hole O. Q. E. D.

Some Considerations on Mr. Newton's Doctrine of Colours, and on the Effects of the different Refractions of the Rays in Telescopical Glasses. In a Letter from Paris. N° 96, p. 6086.

I have seen how Mr. Newton endeavours to maintain his new theory of colours. It seems that the most important objection, which is made against him by way of quære is, that whether there be more than two sorts of colours. For my part, I believe that an hypothesis, that should explain mechanically, and by the nature of motion, the colours yellow and blue, would be sufficient for all the rest, since those others, being only more deeply charged, (as appears by the prisms of Mr. Hook,) produce the dark or deep red and blue, and that, of these four all the other colours may be compounded. Neither do I see why Mr. Newton does not content himself with the two colours yellow and blue; for it will be much more easy to find an hypothesis by motion, that may explain these two differences, than for so many diversities as there are of other colours. And till he has found this hypothesis, he has not taught us in what consists the nature and difference of colours, but only this accident, (which is certainly very considerable,) of their different refrangibility.

As for the composition of white made by all the colours together, it may possibly be, that yellow and blue might also be sufficient for that: which is worth while to try; and it may be done by the experiment which Mr. Newton proposes by receiving against a wall of a darkened room the colours of the prism, and to cast their reflected light on white paper. Here you must hinder the colours of the extremities, viz. the red and purple, from striking against the wall, and leave only the intermediate colours, yellow, green, and blue, to see whether the light of these alone would not make the paper appear white, as well as when they all give light. I even doubt whether the lightest place of the yellow colour may not alone produce that effect, and I mean to try it at the first conveniency; for this thought never came into my mind but just now. Mean time you may see, that if these experiments succeed, it can no more be said that all the colours are necessary to compound white, and that it is very probable that all the rest are nothing but degrees of yellow and blue, more or less charged.

Lastly, touching the effect of the different refractions of the rays in telescopical glasses; it is certain that experience agrees not with what Mr. Newton holds. For to consider only a picture, which is made by an object-glass of 12 feet in a dark room, we see, it is too distinct, and too well defined, to be produced by rays that should stray the 50th part* of the aperture. So that, as I believe I have told you heretofore, the difference of the refrangibility does not, it may be, always follow the same proportion in the great and small inclinations of the rays on the surface of the glass.

Mr. Newton's Answer to the foregoing Letter, further explaining his Theory of Light and Colours, and particularly that of Whiteness; with his continued Hopes of perfecting Telescopes by Reflections, rather than Refractions. N° 96, p. 6087.

Concerning the business of colours; in my saying that when Mons. N. has shown how white may be produced out of two uncompounded colours, I will tell him, why he can conclude nothing from that; my meaning was, that such a white, were there any such, would have different properties from the white which I had respect to, when I described my theory, that is, from the white of the sun's immediate light, of the ordinary objects of our senses, and of all white phænomena that have hitherto fallen under my observation. And those different properties would evince it to be of a different constitution: insomuch that such a production of white would be so far from contradicting, that it would rather illustrate and confirm my theory: because by the difference of that from other whites it would appear, that other whites are not compounded of only two colours like that. And therefore if Mons. N. would prove any thing, it is requisite that he do not only produce out of two primitive colours a white, which to the naked eye shall appear like other whites, but also shall agree with them in all other properties.

But to let you understand wherein such a white would differ from other whites, and why from thence it would follow that other whites are otherwise compounded, I shall lay down this position: that a compounded colour can be resolved into no more simple colours than those of which it is compounded.

This seems to be self evident, and I have also tried it several ways, and particularly by this which follows. Let α represent an oblong piece of white paper, about $\frac{1}{2}$ or $\frac{1}{4}$ of an inch broad, (fig. 3, pl. 2,) and illuminated in a dark room with a mixture of two colours cast upon it from two prisms, suppose a deep blue and scarlet, which must severally be as uncompounded as they can conveniently be made. Then at a convenient distance, suppose of 6 or 8 yards, view it through a clear triangular glass or crystal prism, held parallel to the paper, and you shall

* Compare this with what Mr. Newton says in Numb. 80, page 681, vol. I.

see the two colours parted from one another in the fashion of two images of the paper, as they are represented at β and γ, where suppose β the scarlet, and γ the blue, without green or any other colour between them.

Now from the aforesaid position I deduce these two conclusions. 1. That if there were found out a way to compound white of two simple colours only, that white would be again resolvable into no more than two. 2. That if other whites, as that of the sun's light, &c. be resolvable into more than two simple colours, as I find by experiment that they are, then they must be compounded of more than two.

To make this plainer, suppose that A represents a white body, illuminated by a direct beam of the sun, transmitted through a small hole into a dark room, and α such another body illuminated by a mixture of two simple colours, which if possible may make it also appear of a white colour, exactly like A. Then at a convenient distance view these two whites through a prism, and A will be changed into a series of all colours, red, yellow, green, blue, purple, with their intermediate degrees succeeding in order from B to C. But α, according to the aforesaid experiment, will only yield those two colours of which it was compounded, and those not conterminate like the colours at BC, but separate from one another as at β and γ, by means of the different refrangibility of the rays to which they belong. And thus by comparing these two whites, they would appear to be of a different constitution, and A to consist of more colours than α. So that what Mons. N. contends for, would rather advance my theory by the access of a new kind of white than conclude against it. But I see no hopes of compounding such a white.

As for Mons. N.'s expression, that I maintain my doctrine with some concern, I confess it was a little ungrateful to me to meet with objections which had been answered before, without having the least reason given me why those answers were insufficient. The answers which I speak of, are in the Transactions from p. 20 to p. 29 of this vol. And particularly in p. 22; to show that there are other simple colours besides blue and yellow, I instance in a simple or homogeneal green, such as cannot be made by mixing blue and yellow, or any other colours. And there also I show why, supposing that all colours might be produced out of two, yet it would not follow that those two are the only original colours. The reasons I desire you would compare with what has been now said of white. And so the necessity of all colours to produce white might have appeared by the experiment p. 23, where I say, that if any colour at the lens be intercepted, the whiteness which is compounded of them all will be changed into (the result of) the other colours.

However, since there seems to have happened some misunderstanding between us, I shall endeavour to explain myself a little further in these things according to the following method.

Definitions.

1. I call that light homogeneal, similar or uniform, whose rays are equally refrangible. 2. And that heterogeneal, whose rays are unequally refrangible.

Note. There are but three affections of light in which I have observed its rays to differ, viz. refrangibility, reflexibility, and colour; and those rays which agree in refrangibility agree also in the other two, and therefore may well be defined homogeneal, especially since men usually call those things homogeneal, which are so in all qualities that come under their knowledge, though in other qualities that their knowledge extends not to, there may possibly be some heterogeneity.

3. Those colours I call simple, or homogeneal, which are exhibited by homogeneal light. 4. And those compound or heterogeneal, which are exhibited by heterogeneal light.

5. Different colours I call not only the more eminent species, red, yellow, green, blue, purple, but all other the minutest gradations; much after the same manner that, not only the more eminent degrees in music, but all the least gradations, are esteemed different sounds.

Propositions.

1. The sun's light consists of rays differing by indefinite degrees of refrangibility. 2. Rays which differ in refrangibility, when parted from one another, do proportionally differ in the colours which they exhibit. These two propositions are matter of fact. 3. There are as many simple or homogeneal colours as degrees of refrangibility. For to every degree of refrangibility belongs a different colour, by prop. 2. And that colour is simple by def. 1 and 3.— 4. Whiteness, in all respects like that of the sun's immediate light, and of all the usual objects of our senses, cannot be compounded of two simple colours. For such a composition must be made by rays that have only two degrees of refrangibility, by def. 1 and 3; and therefore it cannot be like that of the sun's light, by prop. 1; Nor, for the same reason, like that of ordinary white objects.

5. Whiteness, in all respects like that of the sun's immediate light, cannot be compounded of simple colours without an indefinite variety of them. For to such a composition there are requisite rays indued with all the indefinite degrees of refrangibility, by prop. 1. And those infer as many simple colours, by def. 1 and 3, and prop. 2 and 3.

To make these a little plainer, I have added also the propositions that follow.

6. The rays of light do not act on one another in passing through the same

medium. This appears by several passages in the Transactions p. 23, 24, 25, 26, and 27, and is capable of further proof.

7. The rays of light suffer not any change of their qualities from refraction.

8. Nor afterwards from the adjacent quiet medium. These two propositions are manifest de facto in homogeneal light, whose colour and refrangibility is not at all changeable, either by refraction or by the contermination of a quiet medium. And as for heterogeneal light, it is but an aggregate of several sorts of homogeneal light, no one sort of which suffers any more alteration than if it were alone, because the rays act not on one another by prop. 6. And therefore the aggregate can suffer none. These two propositions also might be further proved apart by experiments, too long to be here described.

9. There can no homogeneal colours be educed out of light by refraction, which were not commixed in it before: because, by prop. 7 and 8, refraction changes not the qualities of the rays, but only separates those which have divers qualities, by means of their different refrangibility.

10. The sun's light is an aggregate of an indefinite variety of homogeneal colours; by prop. 1, 3, and 9. And hence it is that I call homogeneal colours also primitive, or original. And thus much concerning colours.

Mons. N. has thought fit to insinuate, that the aberration of rays, by their different refrangibility, is not so considerable a disadvantage in glasses as I seemed to be willing to make men believe, when I propounded concave mirrors as the only hopes of perfecting telescopes. But if he please to take his pen, and compute the errors of a glass and speculum that collect rays at equal distances, he will find how much he is mistaken, and that I have not been extravagant, as he imagines, in preferring reflexions. And as for what he says of the difficulty of the praxis, I know it is very difficult, and by those ways which he attempted it I believe it impracticable. But there is a way insinuated in the Transactions, by which it is not improbable but that as much may be done in large telescopes, as I have thereby done in short ones, but yet not without more than ordinary diligence and curiosity.

Strange Effect of Thunder and Lightning, on Wheat and Rye in the Granaries of Dantzick. By M. Christ. Kirkby. N° 96, p. 6092.

You doubtless know how much this city is famed for its numerous and convenient granaries, it being the repository of all sorts of grain the fruitful kingdom of Poland affords. In those granaries are laid up chiefly wheat and rye, in parcels of 20 to 30 and 60 lasts in one chamber, according to its size, and the dryness of the corn; which they turn over 3, 4, 5, 6 times a week, as need requires, to keep it sweet and fit for shipping. Now it happened, that about the latter end of March and April last, we had much and violent thunder and lightning, which had this unhappy effect on all the parcels of wheat and rye of the last year's growth, that, though over night they were dry, sweet, and fit for shipping, the next morning they had lost all these good qualities, and were become clammy and stinking, and consequently unfit to be shipped away for the present: so that the owners were forced to cause it to be turned over two or three times a day; and yet it required six weeks, if not longer, before it was recovered.

This is a thing which often happens to corn that has not lain in the granary a whole year, or not sweat thoroughly in the straw before it be thrashed out. An accident little noted, yet in my judgment worth the enquiring into. For, though the alterations caused by thunder in liquors, be taken notice of, and probable reasons given for them; yet I judge this somewhat more abstruse, and therefore more worth while to be considered.

An Uncommon Case in Physic, communicated by the Same from Dantzick. N° 96, p. 6093.

In this narrative it is stated, that a minister, 50 years of age, was troubled with an indisposition, attended with sickness and vomiting; of which he was liable to relapses after the exertion of preaching. What he brought up in one of these relapses resembled tallow, 4 pieces of which weighed $\frac{1}{2}$ an ounce.

Johannis Flamstedii Derbiensis Angli, ad Clarissimum Cassinum Epistola. N° 96, p. 6094.

This letter of Mr. Flamsteed, to M. Cassini, contains an account of some observations made by the former, on the planet Jupiter and his satellites, also on the planet Mars, stating the motions, periods, and distances of the satellites, from Jupiter; though in a less accurate degree than that which they are known at present. Mr. F. deduces from his observations, that the parallax of Mars is 25″; and that consequently the solar parallax is about 10″; and therefore the sun's distance from the earth is 21000 of the earth's semi-diameter; this being too little, or the parallax too great, by about the 5th part, according to later and more exact observations.

An Account of a Book. N° 96, p. 6101.

Viz. Several Tracts written by the Honourable Robert Boyle; of the strange Subtilty, Efficacy, and determinate Nature of Effluviums; of New Experiments to make the parts of Fire and Flame, Stable and Ponderable; with some Additional Experiments about Arresting and Weighing of Igneous corpuscles; as also a Discovery of the Perviousness of Glass to Ponderable parts of Flame, with some reflexions on it by way of Corollary.

This long title sufficiently shows the nature and contents of these Tracts, which are in the hands of the public.

On the Number of Colours, and the Necessity of mixing them all for the production of White; also why a Picture cast by Glasses into a darkened Room appears so distinct, notwithstanding its irregular Refractions. In a Letter of April 3, 1673, *from Mr. Newton. Being an Answer to the Remarks in the last Number.* N° 97, p. 6108.

It seems to me, that N. takes an improper way of examining the nature of colours, whilst he proceeds upon compounding those that are already compounded, as he does in the former part of his letter. Perhaps he would sooner satisfy himself by resolving light into colours, as far as may be done by art, and then by examining the properties of those colours apart, and afterwards by trying the effects of re-conjoining two or more, or all of those; and lastly, by separating them again, to examine what changes that re-conjunction had wrought in them. This I confess will prove a tedious and difficult task, to do it as it ought to be done; but I could not be satisfied till I had gone through it. However, I only propound it, and leave every man to his own method.

As to the contents of his letter, I conceive my former answer to the quære about the number of colours is sufficient, which was to this effect; that all colours cannot practically be derived out of the yellow and blue, and consequently that those hypotheses are groundless which imply they may. If you ask, what colours cannot be derived out of yellow and blue? I answer, none of all those which I defined to be original; and if he can show by experiment, how they may, I will acknowledge myself in an error. Nor is it easier to frame an hypothesis by assuming only two original colours, rather than an indefinite variety; unless it be easier to suppose, that there are but two figures, sizes and degrees of velocity, or force, of the æthereal corpuscles or pulses, rather than indefinite variety; which certainly would be a harsh supposition. No man wonders at the indefinite variety of waves of the sea, or of sands on the shore; but were they all but two sizes, it would be a very puzzling phenomenon. And I should think it as unaccountable, if the several parts or corpuscles, of which a shining body consists, which must be supposed of various figures, sizes and motions, should impress but two sorts of motion on the adjacent æthereal medium, or any other way beget but two sorts of rays. But to examine how colours may be explained hypothetically, is besides my purpose. I never intended to show wherein consists the nature and difference of colours, but only to show, that de facto they are original and immutable qualities of the rays which exhibit them; and to leave it to others to explicate by mechanical hypotheses the nature and difference of those qualities: which I take to be no difficult matter. But I would not be understood, as if their difference consisted in the different refrangibility of those rays; for that different refrangibility conduces to their production no otherwise, than by separating the rays whose qualities they are. Whence it is, that the same rays exhibit the same colours when separated by any other means; as by their different reflexibility, a quality not yet discoursed of.

In the next particular, where N. would show, that it is not necessary to mix all colours for the production of white; the mixture of yellow, green, and blue, without red and violet, which he propounds for that end, will not produce white, but green; and the brightest part of the yellow will afford no other colour but yellow, if the experiment be made in a room well darkened, as it ought; because the coloured light is much weakened by the reflexion, and so apt to be diluted by the mixing of any other scattering light. But yet there is an experiment or two mentioned in my letter in the Transactions, Numb. 88, by which I have produced white out of two colours alone, and that variously, as out of orange and a full blue, and out of red and pale blue, and out of yellow and violet, as also out of other pairs of intermediate colours. The most convenient experiment for performing this, was that of casting the colours of one prism upon those of another, after a due manner. But what N. can deduce from hence I see not. For the two colours were compounded of all others, and so the resulting white, to speak properly, was compounded of them all, and only decompounded of those two. For instance, the orange was compounded of red, orange, yellow, and some green; and the blue of violet, full blue, light blue, and some green, with all their intermediate degrees; and consequently the orange and blue together made an aggregate of all colours to constitute the white. Thus if one mix red, orange, and yellow powders, to make an orange; and green, blue, and violet colours, to make a blue; and lastly, the two mixtures, to make a grey; that grey, though decompounded of no more than two mixtures, is yet compounded of all the six powders, as truly as if the powders had been all mixed at once.

This is so plain, that I conceive there can be no further scruple; especially to those who know how to examine whether a colour be simple or compounded, and of what colours it is compounded; which having explained in another place, I need not now repeat. If therefore N. would conclude any thing, he must show, how white may be produced out of two uncompounded colours; which when he has done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind; because as I remember I once tried, by gradual succession, the mixture of all pairs of uncompounded colours; and, though some of them were paler, and nearer to white than others, yet none could be truly called white. But it being some years since this trial was made, I remember not well the circumstances, and therefore recommend it to others to be tried again.

In the last place, had I thought the distinctness of the picture, which, for instance, a 12 foot object-glass casts into a darkened room, to be so contrary to me as N. is pleased to affirm, I should have waved my theory in that point, before I propounded it. For, that I had thought on that difficulty, you may easily guess by an expression, somewhere in my first letter (N° 80) to this purpose; that I wondered how telescopes could be brought to so great perfection by refractions, which were so irregular. But to take away the difficulty, I must acquaint you first, that though I put the greatest lateral error of the rays from one another, to be about $\frac{1}{50}$ of the glass's diameter; yet their greater error from the points on which they ought to fall, will be but $\frac{1}{100}$ of that diameter: and then that the rays, whose error is so great, are but very few in comparison to those which are refracted more justly; for, the rays which fall upon the middle parts of the glass, are refracted with sufficient exactness, as also are those that fall near the perimeter, and have a mean degree of refrangibility; so that there remain only the rays which fall near the perimeter, and are most or least refrangible, to cause any sensible confusion in the picture. And these are yet so much further weakened by the greater space, through which they are scattered, that the light which falls on the due point, is infinitely more dense than that which falls on any other point round about it. Which though it may seem a paradox, yet is certainly demonstrable. Yea, although the light, which passes through the middle parts of the glass, were wholly intercepted, yet would the remaining light convene infinitely more dense at the due points, than at other places. And by this excess of density, the light which falls in or invisibly near the just point, may, I conceive, strike the sensorium so vigorously, that the impress of the weak light, which errs round about it, shall, in comparison, not be strong enough to be animadverted, or to cause any more sensible confusion in the picture than is found by experience.

This I conceive is enough to show why the picture appears so distinct, notwithstanding the irregular refraction. But if this satisfy not, N. may try, if he please, how distinct the picture will appear, when all the lens is covered excepting a little hole next its edge on one side only: and if in this case he please to measure the breadth of the colours, thus made at the edge of the sun's picture, he will perhaps find it to approach nearer to my proportion than he expects.

An Answer to the former Letter, written June 10, 1673, *by the same Parisian Philosopher, who wrote the Letter already extant in* N° 96, p. 85, N° 97, p. 6112.

Touching the solutions, given by M. Newton to the scruples by me proposed, about his theory of colours, there were matter to answer them, and to form new difficulties; but seeing that he maintains his opinion with so much concern, I list not to dispute. But what means it, I pray, that he says; though I should show him, that the white could be produced of only two uncompounded colours, yet I could conclude nothing from that. And yet he has affirmed in N° 80, of the Transactions, that to compose the white, all primitive colours are necessary.

As to the manner in which he reconciles the effect of convex glasses for so well assembling the rays, with what he establishes concerning the different refrangibility, I am satisfied with it; but then he is also to acknowledge, that this aberration of the rays is not so disadvantageous to optic glasses, as he seems to have been willing to make us believe, when he proposed concave speculums as the only hopes of perfecting telescopes. His invention certainly was very good; but, as far as I could perceive by experience, the defect of the matter renders it as impossible to execute, as the difficulty of the form obstructs the use of the hyperbola of M. Descartes: so that, in my opinion, we must stick to our spheric glasses, which we are already so much obliged to, and that are yet capable of greater perfection, as well by increasing the length of telescopes, as by correcting the nature of glass itself.

To this letter is to be referred that, which is already extant in N° 96, p. 86, as being an answer thereto.

On Ambergris. By Mr. Boyle. N° 97, p. 6113.

On Ambergris. Extracted from a Dutch Journal, belonging to the Dutch East India Company. And communicated by Mr. Boyle.* N° 97, p. 6115.

Ambergris is not the scum or excrement of the whale, &c. but issues out of the root of a tree; which tree, how far soever it stands on the land, always shoots forth its roots towards the sea, seeking the warmth of it, thereby to deliver the fattest gum that comes out of it: which tree otherwise by its copious fatness might be burnt and destroyed. Wherever that fat gum is shot into the

* Ambergris or ambergrease, is now pretty generally allowed to be no other than the hardened fæces of the sperma-ceti whale, or physeter macrocephalus of Linnæus. Of this particular an ample account occurs in volumes 33, 38, and 90 of the Transactions, and will of course be abridged in a future volume of the present work.

sea, it is so tough, that it is not easily broken from the root, unless its own weight and the working of the warm sea do it; and so it floats on the sea.—There was found by a soldier $\frac{7}{8}$ of a pound; and by the chief, two pieces weighing 5 pounds. If you plant the trees where the stream sets to the shore, then the stream will cast it up to great advantage. March 1, 1672, in Batavia Journal Advice from——

On the further success of the Blood-staunching Liquor. By a Surgeon of his Majesty's Fleet. N° 97, p. 6115.

Sir—I doubt not but you have heard, with what admirable success the royal styptic liquor was used in the last engagement against the Dutch, by the chirurgeons of the Earl of Ossory, Sir Edward Spragg, and Sir John Berry, and others. A very good physician in Yarmouth, several credible persons also in London and other places, some of whom have taken it inwardly themselves, give the like commendation of it for stopping bleeding upon eruption or apertion of a vessel in the lungs, or other internal parts, being administered according to the printed direction.

The Figures of some of Mr. Leewenhoek's Microscopical Observations, formerly published in N° 94, p. 66, *with their Explication.* N° 97, p. 6116.

In fig. 7, pl. 2, AB is the great sting, or rather the sheath or case of the bee, out of which were taken the two stings which he formerly noticed; E is the cavity of the sheath, in which the two stings, by and by to be described, lie; like a quill pulled out of a fowl's wing, and of that cut off a third part in length, and by its sides bent a little inwards towards E. D is the thickness of the case beneath: and about DA the two stings show themselves, each in a place by itself.

In fig. 8, HI is part of the sting taken out of the sheath AB, which appears a little sideways; whence it is, that the crooks or barbs KK do not show so large nor sharp, as indeed they are. L is the back of the sting without barbs; which side or back is almost as broad as one of the sides of the sting, when the barbs appear.

In fig. 9, MN is the whole sting, taken also out of the sheath AB in fig. 7, and with its back, which is without barbs (as has been shown in fig. 8, by L,) turned to the eye. Here the barbs show themselves, though turned from the eye, through the sting, as appears by R. The upper part of the sting NQ is closed round about, and hollow within; and the lower part QS is open. SM is a part of the broken sinew, which is very near as long as the whole sting;

and when it can be taken whole out of the body, it contracts itself into the shape of a half moon, and appears of the colour of a tortoise-shell, as also does the sting itself. OP is the body fastened to the sting, and placed in the thicker part of the case DCA (in fig. 7), viz. S about A, and T towards D.—In fig. 10, abc are both the stings, as they lie together before, close against the sheath; yet is one of them a little higher than the other: and forasmuch as at a there is yet seen a little of the sheath, here both the stings seem to be one, furnished on both sides with barbs.—In fig. 11, edgfh are both the stings, in part out of their sheath; yet the sting edh stands a little higher out of the case than the sting gfh. Thus they are found to lie in their sheath, when they are at rest.—In fig. 12, two stings, standing also a little out of the sheath. As to the motion of these stings, I conceive it to be thus made: first the bee draws her sheath with its stings out of the body, and endeavours to thrust it as far as she can into the body she would sting, together with one of the stings, which at that time she draws out of the case: which sting, when she is drawing back again, but it not being able, by reason of the barbs to return, she pulls the sheath and the other sting deeper into the body. Now it is that she uses her other sting, which she then thrusts also into the body as deep as she can, and then endeavours to pull that back also; by which pulling back she thrusts her sheath and first sting yet deeper into the body: and this she continues so long till she gets both the stings and the sheath as far as to the thick part of the sheath, into the body; which done, the stings need no more motion out of the sheath, when the body of the sting (in fig. 9,) OTP in the thickness of the sheath CDA (in fig. 7,) can move from C to D.

And so much for the stings of the bee. Let us now see how our observer explains his figures representing the several members he has taken notice of about the head of a bee; which were also briefly mentioned in the aforesaid N° 94.—See then fig. 13, where LDABC is one of the two small limbs, which the bee has on the forepart of her head, and which he calls arms, wherewith he judges she makes her honey-combs, each furnished with three peculiar joints, as at D, A, B.—In fig. 14, EF is one of the two small limbs, which the bee has likewise on the forepart of her head, by him called scrapers, by the help of which, he conceives, she scrapes the wax from flowers.—In fig. 15, GH is the small limb, which is also placed before on her head, and is by him called the wiper, wherewith he conjectures she wipes off the honey from the flowers.—Fig. 16, IK represents the scraper of a wild bee, which he exhibits here with the rest, because it is of a different make from the scraper of a tame bee, above in fig. 14.

A Problem of Alhazen solved: by M. Huygens and M. Slusius. Translated from the Latin. N° 97, p. 6119.

Mr. Huygens writes: By this opportunity I send you a solution of Alhazen's problem, which I have lately discovered, and which our colleges approve of very much. The problem is this: Having given a concave or a convex speculum, as also the place of the eye and the visual point; to find the point of reflexion.

Let the point A be the centre of the spherical speculum (fig. 1, pl. 3), B the eye, and C the visible point, and let a plane drawn through A, B, C, cut the sphere in the circle Dd, in which are the points of reflexion to be found. Through the three points A, B, C, describe the circumference of a circle, whose centre is Z, and meeting EI produced perp. to BC, in R; and let NA be a third proportional to RA and OA; then NM, parallel to BC, will be one of the asymptotes. Again, let EA, $\frac{1}{2}$ OA, IA be proportionals, and taking IY equal to IN, draw YM parallel to AZ; and it will be the other asymptote. Lastly, taking IX, IS, which shall each be in power equal to half the square of AO, with the square of AI; then will the points X and S be in the hyperbola, or the opposite sections Dd, to be described to those asymptotes, the intersections of which with the circumference DO, will show the points of reflexion sought. This construction takes place in every case in which the problem is solid, except in that in which not an hyperbola but a parabola is to be described; viz. when the circumference described through the points A, B, C, touches the right line AE.

The editor having sent a copy of this to M. Slusius the 24th of September 1670, he sent the following answer Nov. 22, the same year.—When I reduced the construction of the celebrated Huygens to calculation, I found that he had followed the same analysis as myself. But since two solutions are derived from it, both of them by the hyperbola about the asymptotes, he made choice of the one, and I of the other, as being the easier. Now it is evident, that nothing more is required in this problem, if we reduce it to mere geometrical terms, than in a given circle, whose centre is A and radius AP (fig. 2, 3, 4), to find some point P such, that drawing the lines PE, PB, to the given points EB, unequally distant from the centre A, the line AP produced, may bisect the angle EPB. Now this admits of various cases. For, either the perpendicular from A on the right line EB, that is AO, falls between E and B, or beyond B. If beyond it, either the rectangle EOB is equal to the square of AO, or is greater or less. Of the case of equality we shall speak below. Now the other three cases we shall comprehend nearly under the same construction. Let a circle pass through the three points A, E, B; and AO be produced to its circumference at D. Then if the point O fall between E and B, the right line AO is to be produced towards O; but if beyond B, and the rectangle EOB be greater than the square of AO, it must be produced towards A; but if the rectangle be less than that square, the circle will cut the right line AO in the very point D. Then drawing AX parallel to EB, cutting the given circle in N, make the rectangle DAO to the square of AN, as $\frac{1}{2}$AX is to AH; which must be taken towards X, if O fall between E and B, or if the rectangle EOB be less than the square of OA; but on the contrary side, if it be greater. Now put OQ equal to AH, in the direction EB in the first or second case, but towards E in the third. Then let XA, NA, HK be proportionals, and all taken in every case towards X; and let AO be cut in V, so as that KA may be to AV as AD to AX; join KV, and produce it to meet the right line QM, parallel to OA, indefinitely produced, in the point L. Then in every case KL and QL will be the asymptotes of the hyperbola, which drawn through the point O will satisfy the question. Yet with this difference, that in the first or second case, the hyperbola passing through O will answer for the convex speculum, and the opposite section for the concave one; but, on the contrary, in the third case, the hyperbola through O will answer the concave speculum, and its opposite the convex. And thus it will be when the point V falls between A and O; for if it should fall beyond O, one hyperbola alone, drawn between the same lines QL and KL, will serve for both the convex and concave specula. But if V should fall in the point O, then it would be a plane problem, and the right lines LQ and LK would resolve it. Whence it appears that this problem admits of infinite cases, which may be solved by a plane locus: and hence those are excusable who have thought the problem might be solved universally by the same locus, because thus sometimes the calculus has been successful. For there can be given no position of the three points A, E, B, (still reserving the case of equality of the rectangle EOB and the square of OA for future consideration), which admits not of some circle to be described from the centre A, at the circumference of which the problem may be solved by a plane locus. As to the radius of this circle, if worth while, it might be thus found: In the first and second case of the above construction, make as AX^2 + double the rectangle OAD to $2AD^2$, so AO^2 to AN^2, then AN will be the radius sought. But in the third case, there is to be made as AX^2 — the double rectangle OAD to $2AD^2$, so is AO^2 to AN^2.

There now remains to be constructed the other case, viz. when the rectangle EOB is equal to AO^2, or in which the circle described through the points A. B, E, touches the right line AO. And here M. Huygens has rightly observed that in this case a parabola is to be described; which, however, is not to be so understood, as if it could not be solved by an hyperbola, since it may admit both of an hyperbola and an ellipsis, even an infinite number, proceeding by our method; only this case admits of the parabola, which the other cases do not. And the same limitation is to be made when he says, that his construction takes place in every case when the problem is a solid one; for his meaning is, that by a small alteration an hyperbola may always be found that will answer the question: which will be evident on comparing the cases as above constructed with his construction. But to return to the case of equality: and that I may not seem to have made a rash assertion, you have not one, but two parabolas, and opposite hyperbolas besides, that answer the problem. As before, let EB be the given points, (fig. 5) and a circle from the centre A, also another through the three points A, E, B, whose tangent is AO, and centre D. Drawing the diameter NADX, constitute the three proportionals XA, NA, ZA, the half of which is AL. Make these other three proportionals 2OA, NA, IA, the half of which is KA, and complete the rectangle LAOV; also produce LV to S, till VS be a third proportional to AI, OV; then with the axis SL, the latus rectum AI, and vertex S, describe a parabola; so will this cut the circle in the points P, P, as required. The same thing may be effected by another parabola, if, having completed the rectangle DAHC, and produced KC to T, so that CT be a third proportional to AZ, DC, it be described about the axis TK, with the vertex T, and latus rectum ZA; for this will meet the circle in the same points P, P (fig. 6). But the construction by the opposite sections is still easier; for making, as before, the three proportionals XA, NA, ZA, demit the perpendicular ZI a third proportional to 2AO and AN. Hence ZI will be greater than ZA, since 2AO is less than XA; then at the point I let the right lines IQ, IM be inclined to IZ in half a right angle, and indefinitely produced both ways; lastly, about these as asymptotes through A describe an hyperbola, and the other opposite one; then the latter will answer the problem for a convex speculum, and the former for a concave one. For since, as was shown, ZI is always greater than ZA, the line IM will never pass through A. Therefore a case will not here occur, in which from this construction, as in the foregoing, the problem may be solved by the asymptotes themselves: and yet this also sometimes admits of a plane locus, viz. when it happens that the right line XO drawn to the centre D touches the circle NPP; for then the point of contact itself solves the question. And so much concerning a problem, which has exercised the genius of many persons, the solution of which I completed some years since.

After some other communications with these two learned men, Huygens and Slusius, in which the former acquiesces in the solution of the latter, but yet thinks his own the more natural, Slusius sends another solution of the problem, as follows:

Let there be given a circle whose centre is A (fig. 7), and let the given points be D and d. Suppose the thing done; and let DE be the incident, and Ed the reflected ray; and from the point of reflexion E let fall on DA the perpendicular EI, and from d on the same line the perpendicular dN, also let the tangent EC and ray dE produced meet the same line in C and B. Now put $DA = z$, $AI = a$, $NA = n$, $EI = e$, $dN = b$, $BA = y$, $AE = q$, $CA = x$. Then, since the angle DEC = CEB, and CEA a right angle by the hypothesis, the three lines DA, CA, BA are harmonical proportionals, as is easily shown. Therefore DA will be to BA, as DC to CB; or, in algebraic terms, $z : y :: z - x : x - y$; hence $2zy - xy = zx$, or $\frac{2zy}{z+y} = x$. And since the rectangle CAI, or xa is equal to AE^2 or qq, it will be $x = \frac{qq}{a}$ and consequently $\frac{2zy}{z+y} = \frac{qq}{a}$, or $\frac{zqq}{2za - qq} = y$. Again, it is, as dN to EI, so is NB to IB; or as $b : e :: y - n : y - a$; therefore $ye - ne = by - ba$, and $y = \frac{ba - ne}{b - e}$. Consequently $\frac{zqq}{2za - qq} = \frac{ba - ne}{b - e}$, or $2zbaa - 2znae - qqba + qqne = bzqq - zqqe$, which is the equation to an hyperbola about its asymptotes, the construction of which, with the given circle, answers the problem. For since, from the nature of the circle, it is $qq = aa + ee$, if instead of $2bzaa$ be substituted its value $2bzqq - 2bzee$, there will result another similar equation to the hyperbola about its asymptotes, viz. $bzqq - 2bzee - 2znae - qqba + qqne = - zqqe$. And by this method, as also by that which is explained in my book on analysis, there will arise infinite equations to hyperbolas and ellipses, which with the given circle will solve the problem; only the effections will become so much more intricate, that it may not be worth while to try them: but they may be constructed by that method used for the ellipsis at page 62 of the same book.

The whole of the calculation, as you see, is referred to the line DA; but you will perceive that it might be as easily adapted to the line dA, which is likewise given, viz. by drawing the dotted lines in the scheme. But there is no occasion for the trouble of a new calculation. For if to the line dA, and its parts, there be adapted the same symbols, viz. making $dA = z$, $Dn = b$, $nA = n$, $Ai = a$, $iE = e$, &c.; there will then arise the same equation as before; and you will obtain infinite other hyperbolas and ellipses, which with the given circle will satisfy the problem. It would be useless to prosecute the several cases, since their equations may be discerned by only the variations of the signs + and —. One case only is excepted, viz. when dAB is a right angle; the equation for which is obtained, after expunging out of the former equation all the terms in which n is found, since it vanishes, viz. the equation $2zbaa - qqba = bzqq - zqqe$; or, substituting for $2zbaa$ its value $zbqq - qqba = 2zbee - zqqe$.

But it is to be observed, that though by referring the analysis to the line DA, two hyperbolas offer themselves in the equation; and as many more different from the former, when the calculation is referred to dA; yet in both cases the very same parabola arises: the reason of which you will easily perceive on a little consideration.

Let us now, Sir, apply the foregoing analysis to all the problems which are usually proposed about the reflexion of spherical specula. Let there be then, as before, a circle, whose centre is A, (fig. 8), D a given point, from which DE is an incident ray, and EQ its reflected one. Joining DA, to which draw the tangent EC, and the perpendicular EI, also the line QEB produced; then denoting the parts as before, viz. DA $= z$, CA $= x$, AE $= q$, BA $= y$, AI $= a$, IE $= e$; because the three DA, CA, BA are harmonical proportionals, and the three CA, AE, AI are geometricals, we have always the equation $y = \frac{zqq}{2za - qq}$, on whatever point of the circle the ray DE falls. So that, if the point E be required, on which if the ray DE fall, it may be reflected parallel to the diameter LAV, a perpendicular to DA; the reflected ray QE produced will pass through I, as is plain; and I and B will coincide. Hence $a = y = \frac{zqq}{2za - qq}$, or $aa - \frac{qqa}{2z} = \frac{1}{2}qq$, and the problem is solved by plain loci.

If the point be required from which a ray may be reflected parallel to any other line, as AK, drawn from the centre A; from the point L draw the tangent KL $= d$; then it is plain that the triangles AKL, EIB will be similar, since all the sides of the one are parallel to all those of the other. Therefore AL is to LK as EI to IB, or $q : d :: e : a - y$, and $\frac{qa - de}{q} = y = \frac{zqq}{2za - qq}$; hence $zq^3 = 2qzaa - 2zdae - q^3a + qqde$; or, putting $qq - ee$ for aa, $zq^3 = 2zq^3 - 2zqee - 2zdae - q^3a + qqde$. And each equation belongs to an hyperbola about the asymptotes, which with the given circle solves the problem.

Let it be now proposed to make the reflected ray pass through the given point N, as in Alhazen's problem, or that being produced towards the point of reflexion E, it may pass through the given point N. From N draw NO $= n$ perpendicular to AL, and put AO $= b$. Then it is plain that AO will be to the difference between ON and AB, as EI to IB; that is, $b : n - y :: e : a - y$, or $b : y - n :: e : y - a$. Therefore $\frac{ba - ne}{b - e} = y = \frac{zqq}{za - qq}$. Hence $2zbaa - 2znae - qqba + qqne = zbqq - zqqe$, which is the very equation to Alhazen's problem above formed: or, in the second case, $\frac{ba + ne}{b + e} = y = \frac{zqq}{2za - qq}$, or $2zbaa + 2znae - qqba - qqne = zbqq + zqqe$.

And these are the problems commonly proposed concerning the point of reflexion in which the distance of the given point D is supposed to be finite. But the analysis will be easier if it be supposed infinite. For bisecting CA in G, it appears from the property of the three harmonical proportionals, DA, CA, BA, that the three DG, CG, BG will be geometrical proportionals, supposing the point D at any distance whatever. Therefore if it be supposed infinite, BG will become nothing, and the point B coincide with G. Consequently AB will be always equal to BC: therefore CA $= 2y$, and the rectangle CAI equal to AE^2 will give in symbols $2ay = qq$, or $y = \frac{qq}{2a}$: and since the distance of the point D is supposed infinite, ED will be parallel to AC. Therefore, if there be required the reflected ray parallel to AL, because in that case a and y coincide, it will be $a = y = \frac{qq}{2a}$, or $aa = \frac{1}{2}qq$. If it be required to be parallel to AK, it will again be, as $q : d :: e : a - y$, and $\frac{qa - de}{q} = y = \frac{qq}{2a}$, or $2qaa - 2dae = q^3$: if it be required to pass through N, it will be as above, $\frac{ba \pm ne}{b \pm e} = y = \frac{qq}{2a}$, and $2baa \pm 2nae = bqq \pm qqe$, which are also equations to the hyperbolas about the asymptotes, unless the point N be supposed to be in AL; for, as in that case n becomes nothing, by taking out of the equation the terms in which n is contained, the remainder will give the equation to the parabola, as we before observed.

You will not expect, learned sir, that as a I have hitherto given examples for the concave specula, so I should now proceed to the convex. For you know that the analysis is the same in both, and their equations only differ in the variations of the signs + and —. You know that the parabola or ellipse which solves the one, will satisfy the other also; and if the hyperbola solve the problem in the convex, the opposite hyperbola will do the same in the concave. Omitting these then, I shall only add, that by the same analysis in concave speculums, we may find their foci, and the spaces occupied by the rays in the axis, for any given distance of the lucid point: but with great facility when the rays are supposed parallel; which yet I have seen demonstrated by some in a very tedious way. For in the concave speculum EE (fig. 9), whose centre is A, if the extreme ray be supposed as reflected to the axis AR at B, drawing the tangent EC, it will be CB = BA. Bisect the semi-axis AR in Q; then Q will be the focus, and QB will be the space required. Now QB is half CR, because of the equals AQ, QR, and AB, BC; that is, half the excess of the secant of the arc ER above the radius. Therefore if the arc ER be, for example 9 degrees, then AC will be 101246, and B = $\frac{623}{1000000}$ of AR.

Account of Four Books, N° 97, p. 6127.

I. Tracts, consisting of Observations about the Saltness of the Sea: An Account of a Statical Hygroscope and its Uses; with an Appendix on the Force of the Air's Moisture; and a Fragment on the Natural and Preternatural State of Bodies: By R. Boyle, Esq. to which is prefixed a Sceptical Dialogue on the Positive or Privative Nature of Cold. By a Member of the R. S. London, 1673, in 8vo.

This very long and particular title, and the circumstance that the book is in the possession of all philosophers, make it unnecessary to give any farther account of it.

II. Principia et Problemata aliquot Geometrica, antè desperata, nunc breviter explicata et demonstrata; Auth. T. H. Malmesburiensi. Lond. An. 1673, in 4to.

The famous author of this tract, (Mr. Hobbes), having entertained the reader with some remarks concerning the subject, the principles and the method of mathematics, and with his doctrine of ratio, as also his sense of algebraical operations, with two chapters of square figures, square numbers, and angles; undertakes to confirm his former doctrine; 1. Of the ratio of the circumference to the radius of a circle; 2. Of mean proportionals; 3. Of the ratio of a square to the quadrant of a circle inscribed in it; 4. Of solids and their superficies: to which last he subjoins another method of demonstrating solids and their superficies by their efficient causes. Which done, he concludes the book with a discourse touching demonstrations; the principal and most frequent cause of fallacies in the mathematics; and the notion of the word Infinite. Complaining very much, that geometry has received its greatest prejudice from those, that discourse of a line without latitude; that take the side of a square for the root of a number; that understand not the true nature of ratio; and that speak unfavourably of Infinity! The whole being chiefly directed against Dr. Wallis.

III. An Idea of a Phytological History propounded; with a Continuation of the Anatomy of Vegetables, particularly prosecuted upon Roots; and an Account of the Vegetation of Roots grounded chiefly thereupon. By Nehemiah Grew, M. D. and F. R. S. London, 1673, in 8vo.

This learned and inquisitive author, after the publication of his first endeavours about the anatomy of plants, (of which an account was given in Num. 78 of these Tracts,) being resolved upon a further prosecution of them, has given us the series of his thoughts and observations following thereupon in this his second book, distributed in three parts.

The first contains the author's idea or design of a phytological history. In order to this, five requisites are proposed. The first is a particular and comparative survey of what relates to the more external consideration about vegetables; as of their figures, proportions, seasons, places, motions. The second a like survey of the organical parts by anatomy, and how that is to be prosecuted, both without and with a microscope; from thence we may come to know, what the communities of vegetables are as belonging to all; what their distinctions to such a kind; their properties to such a species; and their particularities to such particular ones. The third, another similar survey of the contents of vegetables; of their several kinds, as spirits, airs and vapors, clear saps, milks, oils, gums, sugars, salts, &c. examined in all manner of ways. The fourth means a like survey of the principles as well as the contents of the organical parts. Whence will be attainable a further knowledge of the modes of vegetation, and of the sensible natures of vegetables, as also of their more recluse faculties and powers. The fifth, a like survey of those bodies, either from which these principles are derived, or wherewith they have any communion; which are earth, and all solid receptacles; water, and all liquid receptacles; air and sun. All which our author concludes with putting the question once more, viz. In what manner these principles are so adapted as to become capable of being assembled together in such a number, connection, proportion and union, as to make a vegetable body?

The second part is a continuation of the anatomy of vegetables, particularly prosecuted upon roots; as 1. The skin, its external accidents and original, its compounding parts, the one parenchymous, consisting of bubbles, the other ligneous, consisting of tubulary vessels. 2. The bark; its original and external accidents, its compounding parts, likewise parenchymous, and ligneous; which latter consists of succiferous vessels, conjoined in threads, but no where inosculated, nor ramified, but distinct as the fibres of a nerve, which vessels are of various kinds, denominated from their contents, as lymphæducts, lacteals, &c. 3. That portion of the root within the bark, of the like composition with the former, and its ligneous part, compounded of succiferous and air-vessels. Where the structure of the bark, and more visibly of this portion of the root, is compared with that of a muscle; and the air-vessels with nerves: concerning which latter vessels he observes, with the excellent Malpighi, the spiral position of their parts; adding to that observation, that that spiral zone, as Signor Malpighi calls it, is not one absolutely entire piece, but consists of two or more round and perfect fibres standing collaterally together. 4. The pith, not common to all roots; its origin, parenchymous nature, and texture, like to a rete mirabile, or an infinite number of small fibres admirably complicated together; its contents being sometimes a limpid liquor, sometimes a vaporous air.

The third part contains an account of the vegetation of roots, grounded chiefly on the foregoing anatomy. Where our author considers nature as one universal monarchy, visible in vegetables, if we take notice, that the soil is prepared by rain, sun, wind, air, and their several successions; that the parenchyma of the bark of the root, standing in the soil thus prepared, receives the watery parts of the soil, that the skin strains the water, and renders it more pure; that the sap thus strained, yet being compounded of heterogeneous parts, and they received into the said parenchyma, they will now ferment; whereby being yet further prepared, they will more easily insinuate themselves into all the bubbles of that parenchyma, which being in no place openly and visibly pervious, but every where composed of an infinite number of small bubbles, the sap therefore is not only fermented therein, and fitted for separation, but as it passes through it, is every part of it strained a hundred times over from bubble to bubble; in a word, how the whole progress of vegetation is performed. Besides the explication of a great variety of phænomena, occurring in the contemplation of vegetables; as why the organical parts are void of taste, smell and colour: whence the succiferous vessels are tough, and the parenchymous parts friable? how the said succiferous vessels grow in length, cylindrical and hollow; the lactiferous how and why wider? how the air-vessels are formed? how the parenchymous parts become fibrous, and the fibres disposed into bubbles, &c.

All which is concluded with excellent observations of the odours of vegetables, of their colours and tastes; and the whole piece illustrated with 7 tables of cuts, representing the figures of several roots, as the author had viewed them, both with the naked eye and the microscope; together with an explication of these figures.

IV. Thomæ Bartholini Acta Medica et Philosophica Ann. 1671 et 1672, Hafn. 1673, in 4to.

We do not think it necessary to enumerate the contents of this miscellaneous collection; which, however, like all the other works of this author, contains many things interesting to physicians and philosophers.

An Account of the Current of the Tides about the Orcades. Communicated by Sir Robert Moray, * *Knt. deceased, lately one of the Vice Presidents of the Royal Society, of which he had been President formerly.* N° 98, p. 6139.

In Fairy Sound, between the isles of Fairy and Ætha in Orkney, the sea runs north-east, for the space of only three hours in flowing, and nine hours south-west in ebbing. This is the course of the tide only in the middle of the Sound, which is but one mile broad.

The next isle to Fairy, towards the south-west is Westra, about five miles in length, and three or four in breadth. On the south-east side of this island, within a mile of the shore lies another little isle, not half a mile in circumference. South and south-west from these two islands, is Westra-Frith, eight miles in breadth, running between them, and the isle called Pansa. Through this Frith the English ships ordinarily pass in their course to Iceland.

While the sea runs from west to east in flowing, through this Westra-Frith, there are no greater surges, than in any other place of the sea; and in a calm day it is as smooth as any lake, though there is constantly a great current in the flux and reflux of the sea. Yet at the south-east end of the fore-mentioned little island, the sea no sooner begins to run westward in ebbing, but there begins a surge to appear, which continually increases until the ebb be half spent, after which it decreases until it be low water, when it ends. East and west from

* This respectable member of the Royal Society, was descended of an ancient and noble family in the Highlands of Scotland, and was educated partly in the University of St. Andrews, and partly in France, where he had afterwards a military employment in the service of Lewis the 13th, being also n high favour with Cardinal Richelieu. He came over to England for recruits, when King Charles I. was along with the Scotch army at Newcastle. Here he grew in great esteem with the King, for whose escape, about Dec. 1646, he laid a design in the following manner: Mr. Wm. Moray, afterwards Earl of Dysart, had provided a vessel at Tinmouth, whither our author, Sir Robert, was to conduct the King in a disguise. The plot proceeded so far, that his Majesty put himself in the disguise, and went down the back stairs with Sir Robert: but fearing he should not pass all the guards without discovery, and deeming it very indecent to be taken in such a condition, his heart failed him, and he went back.

On the restorarion of King Charles II. Sir Robert was made one of his Privy Council, and was one of the first and most active members of the R. S. attending assiduously to their concerns, and communicating several papers. In the first charters of the Society he was named of the Council; and he had, before that, been several times the President, when it was the custom to elect one monthly. Sir Robert died suddenly at Whitehall, July 4, 1673, and was interred in Westminster Abbey, near the monument of Sir Wm. Davenant. Sir Robert was well skilled in Mathematics and Natural History; and he was universally beloved and esteemed, for his respectable talents and the amiable disposition of his mind.

this great surge there are some few lesser surges seen, which are gradually less towards both the east and west.

Further Solutions of Alhazen's Problem; being a Continuation of the Extracts of Letters from M. Huygens and M. Slusius. N° 98, p. 6140. *Translated from the Latin.*

And 1st, M. Huygens, in a Letter of April 9, 1672, says: The following is the compendium which I discovered for the first construction, communicated to you before. Drawing AT parallel to CB (fig. 1. pl. 4,) which being bisected in V, gives the point through which one of the opposite hyperbolas ought to pass, the asymptotes of which were found to be YM, MN.—But the construction which obtains in all cases is as follows: Let ED (fig. 2,) be the given circle, whose centre is A; also B and C the given points. Drawing the lines AB, AC, make the following lines proportionals, viz. BA, the radius of the circle, and FA; also CA, the radius of the circle, and GA. Then join FG, and bisect it in H; through which draw LHK, MHN intersecting at right angles, the former LHK being parallel to that which bisects the angle BAC. Then are these the two asymptotes of the hyperbolas to be described through the points F and G, and one of which will pass through the centre A; and their intersections with the periphery of the given circle will be the points of reflexion sought.

Again by Slusius, who, in consequence of the above, writes as follows.—The excellent Huygens's construction is very simple and ingenious. And he has well observed how the equilateral hyperbola may be extended to all the cases, which, as I before hinted, immediately offered itself in the case of a right angle. Also that one ellipsis of an easy construction, might be selected out of the infinite number which might be used: but it is tedious to dwell so long on the same problem. But one thing still remains of no unpleasant speculation: that is, since the sections which, with the given circle, are employed for the solution of the given problem, cut it in four points, of which only two can serve for the reflexion: it may be inquired, what problem is solved by the two others; and how the proposition may be expressed that shall include all the four cases; and whether these four cases occur when the given points are equally distant from the centre.

M. Huygens employs no other than my analysis, which admits of a parabola only in one case; as appears from the two equations for the hyperbola about its asymptotes, which I before sent you;

viz. $2zbaa - 2znae - qqba + qqne = bzqq - zqqe$,

and $bzqq - 2znae - qqba + qqne = 2zbee - zqqe$:

And I added, that by a small variation, viz. substituting for example, for qq its value $aa + ee$, there may be found an infinite number of hyperbolas and ellipses' which, with the given circle, would solve the problem. Now, in the former of these equations, for $bzqq$ put its value, and it gives $zbaa - 2znae - qqba + qqne = bzee - zqqe$, or $aa - \frac{qqa}{z} = ee - \frac{qqe}{b} + \frac{2nae}{b} - \frac{qqne}{zb}$.

And this is the very equation, which that learned gentleman has constructed with so much ease and ingenuity.

Again, writes M. Slusius, I happened lately on the following construction, which I submit to your judgment and censure, as I think a shorter can hardly be found. Let the given points be E, B, (fig. 3,) and the circle whose centre is A; join EA, BA, cutting the circle in F and C; make the three proportionals EA, FA, VA, and the three others BA, CA, XA: then VX being joined, and produced at pleasure, with the vertex X, diameter VX, and latus rectum equal to it, describe the hyperbola XP, whose ordinates to the diameter VXG are parallel to AB. For this satisfies the proposition in the case of the convex speculum, and its opposite in the case of the concave one. As to the asymptotes, they will be easily found by producing VX till it meets EB produced in L; then bisecting VX in I, and taking LD equal to LI; for joining DI, it will be one of the asymptotes, which the other meets perpendicularly at I.

But perhaps it will not be unacceptable to you to know how I arrived at this construction. This then I deduced from my former analysis as follows: the same things being given as before, demit AO perpendicular to EB, (fig. 4,) and let P be the required point, from which draw PR perpendicular to AO. Put $AO = b$, $EO = z$, $OB = d$, $AP = q$, $PR = e$, $AR = a$; then is easily derived this equation, $\frac{2zdae + 2bbae - 2bqqe}{zb - bd} + ee = aa - \frac{qqa}{b}$, which may be changed into these $\frac{zdae + bbae - bqqe}{zb - bd} = aa - \frac{1}{2}qq - \frac{qqa}{2b}$,

and $\frac{zdae + bbae - bqqe}{zb - bd} + ee = \frac{1}{2}qq - \frac{qqa}{2b}$.

I formerly sent you the construction of this last; and M. Huygens has sent you the construction of the other. As to this one indeed, though I at once perceived it, yet I rather neglected it, as the construction seemed to be difficult. But I find I had been deterred by a groundless fear, as I have lately found it lead me to this construction, which I now send you. To abridge the calculation, make $z - d = k$, and $zd + bb = bm$; then it will be $ee + \frac{2mae - 2qqe}{k} = aa - \frac{qqa}{b}$; and adding $\frac{q^4 + mmaa - 2qqma}{kk}$ on both sides, it is $ee + \frac{2mae - 2qqe}{k} + \frac{q^4 + mmaa - 2qqma}{kk}$, that is, the square of $e + \frac{ma - qq}{k}$, equal to $aa - \frac{qqa}{b} + \frac{q^4 + mmaa - 2qqma}{kk}$; which gives this proportion, $kk : kk + mm ::$

$aa - \frac{kkqqa}{bkk + bmm} + \frac{q^4 - 2qqma}{kk + mm}$: the square of $e + \frac{ma - qq}{k}$; which may be reduced to a simpler equation, by putting $kk + mm = pp$, and $\frac{ky}{p} = a$, for it then becomes the square of $e - \frac{qq}{k} + \frac{my}{p} = yy - \frac{qqky}{bp} - \frac{2qqmy}{kp} + \frac{q^4}{kk}$; which equation you will find to answer to the above construction, by applying the calculation; and you will at the same time observe, that to which ever of the lines EA, AB, BE, the analysis is referred, the same sections may be always obtained, though by a longer process, and by very different equations.

From this construction, by analogy, may be deduced the solution of the other problem, viz. to determine a point, from which a ray may be reflected parallel to any given line. Thus, if B be the given luminous point, A the centre of the circle, (fig. 5,) and the reflected ray be parallel to AE. For this is the same thing as if, in the other problem, the distance of the points A and E be supposed infinite; in which case the third proportional to EA, FA, becomes nothing, and the points A and V coincide: therefore VX will be equal to AX, and AE parallel to PE. Apply therefore the former construction, and you solve the problem: viz. to the vertex X, transverse diameter VX or AX, and latus rectum equal to it, describe the hyperbola XP, whose ordinates to the diameter AX are parallel to AE.

Again, M. Huygens, July 1, 1672, writes as follows.

It is true, and indeed wonderful, that the construction I formerly sent you, may also be found by M. Slusius's calculation, after changing qq for $aa + ee$. But this seems only to happen by chance, nor does the simplicity of the construction here appear till after close application to it.

To repeat Alhazen's Problem, viz. Having given a circle, whose centre is A, its radius AD, and two points B, C; to find a point H in the circumference of the given circle, whence drawing the lines HB, HC, they shall make equal angles at the circumference (fig. 6).

Suppose it found; and drawing AM, which bisects the angle BAC, and perpendicular to it draw HT, BM, CL. Then join AH, and make HE perpendicular to it, and let AM meet BH and HC in the points K, G.

Now put AM $= a$
MB $= b$
AL $= c$
LC $= n$
Radius AD $= d$
AF $= x$
FH $= y$

Then because the angles KHE and CHZ or EGH are equal, and as EHA is a right angle, it will be, as KE to EG, so is KA to AG. And because BM is to MD, as HF to FK, it will be as BM + HF to HF, so MF to FK, or $b + y : y :: a - x : \frac{ay - xy}{b + y}$; add FA or x, and it makes KA $= \frac{ay + bx}{b + y}$.

Again, because CL is to LG as HF to FG, it will be, by permutation and division, as CL — HF to HF so LF to FG, that is $n - y : y :: c - x : \frac{cy - xy}{n - y}$, which taken from AF $= x$, leaves GA $= \frac{nx - cy}{n - y}$. And EA $= \frac{dd}{x}$, because FA, AH, AE are proportionals: Therefore EA — GA, that is EG $= \frac{dd}{x} - \frac{nx - cy}{n - y}$; and KA — EA or KE $= \frac{ay + bx}{b + y} - \frac{dd}{x}$. But we said that KE is to EG as KA to AG,

that is $\frac{ay + bx}{b + y} - \frac{dd}{x} : \frac{dd}{x} - \frac{nx - cy}{n - y} :: \frac{ay + bx}{b + y} : \frac{nx - cy}{n - x}$;

hence is found $\left\{ \begin{array}{l} 2anxxy + 2bnx^3 - ddbnx - ddnxy \\ - 2acxyy - 2bcxxy + ddbcy + ddcyy \end{array} \right\} = \begin{array}{l} naddy + nbddx \\ - addyy - bddxy. \end{array}$

And because $n = \frac{bc}{a}$, it is $\frac{2bbcx^3}{a} - \frac{bbddcx}{a} - \frac{2bbcyyx}{a}$, because $xx = dd - yy$.

But it is $\frac{2bbcx^3}{a} = \frac{2bbcddx}{a} - \frac{2bbcyyx}{a}$, because $xx = dd - yy$.

Therefore $- \frac{2bbcxyy}{a} - \frac{ddbcxy}{a} - 2acxyy + ddcyy = - addyy - bddxy$.

Then dividing all by y and multiplying by a, it

is $- 2bbcxy - ddbcx - 2aacxy + ddcay = - aaddy - bddax$,

or $abddx - cbddx + acddy + aaddy = 2aacxy + 2bbcxy$,

And $\frac{abddx - cbddx + acddy + aaddy}{2aac + 2bbc} = xy$ an equation to the hyperbola.

Or, because $bc = na$, $\frac{abddx - anddx + acddy + aaddy}{2aac + 2bbc} = xy$.

Put $\frac{add}{aa + bb} = p$; then $\frac{pbx - pnx + pcy + pay}{2c} = xy$.

Hence also is easily found the following construction: Join BA, AC (fig. 7), and applying to each separately the square of the radius AD, they give AP, AQ; then joining PQ, bisect it in R, through which draw RD parallel to AD, (which bisects the angle BAC) and RN perpendicular to it. Then will RD, RN be the asymptotes of the opposite hyperbola, the one of which ought to pass through the centre A, and they will cut the circumference in the points H sought. The hyperbolas will also pass through the points P, Q.

The reason of the construction is manifest, by drawing Pγ and Qζ perpendicular to AM: for it makes A$\gamma = \frac{add}{aa + bb}$ or p; and A$\zeta = \frac{ap}{c}$. Also P$\gamma = \frac{pn}{c}$, and Q$\zeta = \frac{pb}{c}$. Therefore AO $= \frac{pc + pa}{2c}$, and OR $= \frac{pb - pn}{2c}$. Hence the rest is easy.

To the above M. Slusius replies as below.

You will not wonder, learned Sir, that in Alhazen's problem, the same construction may be deduced from different equations, since all those that we have as yet used, are contained in one and the same general analysis. Now to show this, let there be given the circle, whose centre is A, (fig. 8), also the points H and I; and let there be required the point K, to which, from the points H and I, let there be drawn HK, IK, and the tangent KD. Then, from A draw any line AG, meeting HK in E, IK in B, and the tangent KD in D, producing such of the lines as are necessary. This done, it is plain, because of the equal angles EKD, DKB, and the right angle AKD, that the three lines AE, BE, DE, will be always harmonical proportionals. Therefore drawing KC, IF, HG perpendicular to AE, and calling

AK, q — AC, a — CK, e — HG, b — AG, d — FA, z — FI, n,

then by the method I formerly used in the second analysis of this problem, we have this general equation, $ndaa - bzaa - nqqa + bqaa = ndee - zbee + 2bnae + 2zdae - dqqe - zqqe$. Suppose now AG to be perpendicular to HI, there will be no variation in the equation, except that AF and AG, that is d and z, will be equal. Putting therefore d for z, it makes $ndaa - bdaa - nqqa + bqqa = ndee - dbee + 2bnae + 2ddae - 2dqqe$; or dividing all by $nd - db$, then $aa - \frac{qqa}{d} = ee + \frac{2bnae + 2ddae - 2dqqe}{ad - db}$; which is the very same that I deduced from my first analysis, though by a different way, and which I lately sent you, constructed after an easy manner.

Next suppose AG to coincide with AH; then HG or b will become nothing. Expunging therefore out of the equation all the terms in which b is found, there will remain $ndaa - nqqa = ndee + 2zdae - dqqe - qqze$. And this, you may recollect, I gave as my second thoughts, and another similar to it, for the case in which AG passes through I.

Again, let us suppose AG to bisect the angle HAI; then, because of the similar triangles HAG, IAF, it will be, as HG to GA, so IF to FA, or as $b : d :: n : z$, and $nd = bz$. Then, taking away equals, it is $bqqa - nqqa = 2bnae + 2zdae - dqqe - qqze$: the very same, as I perceive by your letter, which M. Huygens has constructed.

Lastly, suppose HG to bisect HI; therefore HG = IG, or $b = n$; and, by taking away equals, it is $bdaa - bzaa = bdee - bzee + 2bbae + 2zdae - dqqe - qqze$; which, though not very difficult, none of us has yet constructed. But these, as well as the general equation itself, may be divided into two others, by putting, as you know, for aa or ee, its value $qq - ee$ or $qq - aa$.

You see then, whatever has hitherto been done, is resolved into the same analysis; which comprehends also infinite other constructions by the given circle and an hyperbola. But it is of no great consequence to investigate them, since in this problem, though formerly solutions were wanting, yet now there are plenty. I shall add only a construction by a parabola, and that in two ways; which though it may seem more operose than by an hyperbola, yet the simplicity of this curve compensates for the labour, as in that respect the parabola has an advantage over the other conic sections.

The same things then being given; join AI (fig. 9), and produce it to S, till AS be equal to AH; then joining HS, and bisecting IS in M, through M draw RMQ perpendicular to HS, on which demit the perpendicular AQ, parallel to which draw the radius AC. Then, forming the three proportionals AI, AC, AE, make as SA to AE so MQ to AD and so RS to AP (towards Q in the line AQ); and in the same line, on the other side, take DO = DC. Then, bisecting PD in X, let VXL, drawn through X, make with AX half a right angle, meeting in V the perpendicular erected at D, and on which demit the perpendicular OB. I say, if there be made as VX to XB, so XB to BL; then L is the vertex, LV the axis, and XV the latus rectum of the parabola, which satisfies the problem in every case; viz. cutting the given circle in the points K, of which the highest and lowest belong to Alhazen's problem, and the rest to another problem, concerning which I lately wrote to you.

There may also be given another parabola, as was hinted above, which will perform the same thing as this, and the description of which is so easily deduced from this, that there is no occasion for a new one. For let Aδ be taken = DA and in that direction, and Aω = OA and in that direction. Then bisecting Pδ in ξ, draw $\upsilon\xi\beta$ perpendicular to XB, meeting $\delta\upsilon$, perpendicular to OA, in υ, and on it demit the perpendicular $\omega\beta$; also make as $\upsilon\xi$ to $\xi\beta$, so $\xi\beta$ to $\beta\lambda$: Then λ will be the vertex, $\lambda\xi$ the axis, and $\upsilon\xi$ the lastus rectum of the parabola, which will cut the given circle in the same points with the former.

On the first Invention and Demonstration of a Right Line equal to a Curve. In a Letter from Dr. Wallis, Oxf. Oct. 4, 1673. Translated from the Latin. N° 98, p. 6146.

Learned Sir—As to the rectification of that curve, which I have used to call the semicubical paraboloid, M. Huygens must be under a great mistake, when (in his horologium oscillatorium, p. 71, 72,) he ascribes the first invention of it to John Heuraet of Harlem, in the year 1659. For it is certain, that it was discovered and demonstrated two years before, by Mr. Wm. Neil,* an English-

* This ingenious young gentleman was the eldest son of Sir Paul Neil, Knt. one of the ushers of the privy chamber to King Cha. I. and was grandson of Dr. Rd. Neil, archbishop of York. He was born Dec. 7, 1637, and was educated at Oxford, where he became gentleman commoner of Wadham-college, in 1652, for the sake of Dr. Wilkins the warden; by whose instructions, and those of

man, son of Sir Paul Neil. And after him, the same was demonstrated by the Lord Brouncker, and by Sir Christopher Wren, about the months of June and July, 1657: which was a thing well known here; and it was published and received with applause by those gentlemen, who, before the institution of the Royal Society, used to meet on stated days at the mathematical lectures in Gresham College. And the Lord Brouncker communicated the same to me at Oxford, by letter the ensuing August; and at the same time he sent me his own demonstration of it. This I communicated exactly by letter to M. Huygens, and afterwards annexed it to my treatise on the cycloid, in 1659, with an impartial account of the whole affair. So that it is surprising how M. Huygens should make Heuraet the first inventor of it.

In the year 1658, Sir Chr. Wren had discovered a right line equal to the cycloidal curve, and its several parts; as was then well known, not in England only, but also in France and Holland; and in particular M. Huygens himself was acquainted with it, even before he knew any thing of Heuraet's discovery, as appears by his letter to me. And though it be allowed that Sir Chr. Wren was the inventor of this, yet he never pretended that he was the first who found out a right line equal to a curve; for he knew, and he owns as much, that Mr. Neil had discovered it the year before him. Only he assumes this prerogative to himself, that he had rectified the first curve that offered; whereas Mr. Neil applied himself rather to find out a curve that should admit of rectification. It is true that this curve is of the tribe of the paraboloids; but no one before Mr. Neil took it into consideration. And it is as true that both Mr. Neil's and M. Heuraet's is the very same curve.

Two others on the same Subject. The first of the Right Hon. Lord Visct. Brouncker, Chancellor to her Majesty, and P. R. S. &c. N° 98, p. 6149.

Sir,—Mr. William Neil, in the year 1657, found out and demonstrated a straight line equal to a paraboloid; and communicated the same to others, who used to meet at Gresham College, and it was there received with good approbation; and the same was soon after demonstrated by myself and others; and therefore earlier than that of M. Heuraet, which it seems

Dr. Seth Ward, he greatly cultivated and improved his genius in mathematics. His success in that study appeared as early as 1657, at 19 years of age, when he, first of all men, accurately rectified a curve line, as appears by the above letter of Dr. Wallis, and the two next following ones, of Lord Brouncker and Sir Chr. Wren.—Mr. Neil became an early member of the Royal Society, of which he was elected a fellow in Jan. 1663. His theory of motion was communicated to the Society, April 29, 1669. But the further expectations, which had been conceived of his genius in mathematical and philosophical subjects, were disappointed by his early death, which happened the 24th of August, 1670, in the 33d year of his age.

is not pretended to have been done before the year 1659; and earlier too than that of Sir Ch. Wren, finding a straight line equal to a cycloid in the year 1658; and by him admitted so to be. Nor ought it at all to prejudice Mr. Neil, that M. Heuraet's was somewhat sooner abroad in print, than that of M. Neil, though both in the same year 1659; since it is well known to many of us, that Mr. Neil's was done before. Otherwise M. Huygens, by the same reason, will grant the precedency to Heuraet, of that which he now claims to be his own invention (that rectifying the parabolical line, and squaring the hyperbolical space, do mutually depend on each other:) for this was published in print by M. Heuraet (or M. Schooten for him) in the year 1659, and not by M. Huygens till now, 1673: and yet M. Huygens thinks, he may well claim that invention to be his own, because he now tells us, that he found it out about the end of the year 1657, and did, some time after, communicate it privately to some friends. And whereas, he supposes that this invention of his might give occasion to that other of Heuraet; we may also as well suppose, that he might have taken such occasion from hearing of Mr. Neil having done the like, for this had been then commonly known for a great while: or might have taken occasion, as well as Mr. Neil, from that of Dr. Wallis Schol. prop. 38. Arith. Infin. or from that of Sir Ch. Wren having found a straight equal to another curve the year before: or if it were necessary to know the symbolization between the parabolical line and the hyperbolical space, he might have had it earlier from Dr. Wallis. For when he had demonstrated (Schol. prop. 38. Ar. Infin.) that the particles which compose the parabolical line, are in power equal to a series of squares increased by a series of equals, suppose $\surd : A^2 + b^2 :$ and (prop. 35, 41, conic. sect.) that *c* the ordinates to the conjugate diameter of an hyperbola, that is, the particles of which that hyperbolical space consists, are so also, viz. $\surd : \frac{1}{4}T^2 + \frac{T}{L}h^2 :$ where A, T, L, are permanent quantities, and b, h, taken successively in arithmetical progression; it was easy for M. Heuraet, or M. Huygens, or any other, to infer, that if we can rectify the one, we may square the other, and vice versa. But from whencesoever M. Heuraet had it, we may, as before, reasonably conclude, that Mr. Neil had it before him; and M. Huygens is a person of that ingenuity, that when he shall better consider of it, he will I doubt not be of the same mind. London, Oct. 8, 1673.

The other Letter is of Sir Chr. Wren, Knt. Surveyor General of his Majesty's Buildings, &c.

Sir, That I did, in the year 1658, find a straight line equal to that of a cycloid, and the parts thereof, was then very well known, not in England only, but in France and Holland. And I have not yet heard of any, who do pretend to have known it before I discovered it: which was the same year acknowledged in print by those of France. But I do not pretend to have been the first that did ever find a straight line equal to a crooked. For I very well know, that Mr. William Neil had, the year before, found out and demonstrated, how to construct a crooked line so as to be equal to a straight, by a certain series of numbers after the method of Dr. Wallis. And though he did not therein demonstrate the other properties of that line; yet the same were presently after demonstrated by myself and others, and the nature of the line fully discovered, being a certain paraboloid. And that which M. Heuraet is said afterwards to have found out, in the year 1659, and M. Fermat in the year 1660, are but the same with that of Mr. Neil.

An Account of a Book. N° 98, p. 6151. *viz.*

De Corpore Animato Libri quatuor, seu promotæ per Experimenta Philosophiæ Specimen alterum; Auth. Johanne Baptista Du Hamel P. S. L. Par. 1673, in 12mo.

A work for the most part metaphysical; and though written with great learning, yet superseded by the essays of later writers on these subjects.

Of Stones in the Bladder. By Mr. Chr. Kirkby. N° 99, p. 6155.

Although I find, in Numb. 26, of your Phil. Trans. an account of 96 stones taken out of one bladder; yet I hope this of 38 stones will not be unacceptable, since several of them were large, and of the lesser sort some were triangular and quadrangular; their flats worn to a great smoothness, and their corners blunted. The greatest stone weighed 206 grains; the least 3 grains; and all the 38 weighed $4\frac{7}{8}$ ounces. The matter of the stones is very compact, and like white clay; and though the several coats may be discerned in one of them which I broke, yet they are not easily separable. But what I wonder at most is, that in the dissection of the kidneys and ureters, not any sign of stone or gravel was to be found.

The following is the Relation as made by Casparus Wendland, Surgeon of Dantzic. Translated from the German.

Mr. John Braun, a gentleman of 71 years of age, being dead, I was desired to open his body, to see whether we could find the cause of the excessive pains he had endured, for two years and a half, in the penis, with a continual cutting, burning, and pressing of his urine, coming from him drop by drop; until at last it came to a constant endeavour of going to stool and of making water, which, a few weeks before his death, ended in a continual running of urine, with very sharp pain; after which, about four days before his death, the water was totally stopped. He being dead, on opening the body I observed the following particulars: viz. The internal parts being all carefully examined, we found no defect in them till we came to the bladder, which being taken out, we found it quite full of stones, of which the largest was of the size of a pigeon's egg. Of the larger sort there were 16, yet differing in size. The 22 others were very small.

Not a drop of urine was found in the bladder; but it had already made, on the side of the orifice of the bladder, an opening of a considerable size, which immediately caused death. In the kidneys and ureters there could not be found the least grain or mark of sand.

Of an Uncommon Fœtus. In a Letter from M. Denys, at Paris. Translated from the French. N° 99, p. 6157.

A few days ago, I was called to a sick woman just brought to bed. After I had prescribed the physic I judged necessary, I asked for the child, which died, I heard, as soon as it was born. The body of it appeared outwardly very well formed, and very fat; but the head was so deformed, that it frightened all that were present. It had no front; the two eyes were on the top of the face, very large, and almost without an orbit to lodge them in. The upper and hind part of the head was red like coagulated blood, and resembled the bottom of a calve's head, when cut and severed from the vertebræ of the neck. I had the curiosity to examine this red flesh, and found under it a bone, not a hollow skull, but a solid bone in the form of a small oyster. I had it opened every way; but found no hollowness or brains in it. This bone was only fastened before to the bones of the face, and not behind to the vertebræ of the neck; so that the marrow of the back bone had no communication with the head. I pursued the optic-nerves, and lost them in this bone, which was in lieu of the cranium, and was not at all spongy, but very hard. It seems to me somewhat extraordinary, that a child should be able to live nine months without brains; for I was informed, that it was very lively and brisk in the womb, but died as soon as it came into the air.

Account of some Natural Curiosities presented to the Royal Society, by Sig. Paulo Boccone, of Sicily. N° 99, p. 6158.

The first were uncommon pieces of coral, red and white; some of which were ramified into solid massy bodies; others were coralline buttons or flowers; in some of which the presenter, on squeezing them, found a lacteous juice.

And being present at the coral-fishing in the channel of Messina, which separates Calabria from Sicily, he relates, that before the coral-fishers drew their nets out of the water, he immersed his hand and arm into the sea, to feel whether the coral was soft under the water before it was drawn up into the air, and found it quite hard, except the round end, or button, above-mentioned; which having been bruised with his nails, he found it made up of five or six little cells, full of a white and somewhat mucilaginous liquor, resembling that milky juice, found in summer in the long cods of the herb, called Fluvialis pisana foliis denticulatis, mentioned by Joh. Bauhinus. This coralline juice he calls leven, because of the sharp and astringent taste, in such pieces as came recently out of the sea; those that are dried losing that part of the taste which is acrimonious, and retaining only that which is astringent: which change of taste he affirms to be made in about six hours after the coral has been drawn up; in which time also the leven, that is inclosed in the pores, is dried, and has changed its colour.

As to the question whether coral be a vegetable, M. Guisony is of opinion, that so far from being a plant, it is a mere mineral, composed of much salt and a little earth; and that it is formed into that substance by a precipitation of divers salts, that ensues upon the encounter of the earth with those salts; after the manner of the known metallic tree, which in a very little time is formed and increased by the settling and combination of mercury and silver, dissolved in aqua fortis, and afterwards cast into common water; the parts of this mineral and metal uniting with each other. This sentiment he confirms by alleging, that he can show a salt of coral, which being cast into water, and there dissolved on the evaporation of the water by a gentle heat is presently coagulated, and converted into many small sticks, resembling a little forest.

The second was a certain stony scaly substance, that scented of bitumen, complicated and laid together membrane like, and found in the Hyblean mountains of Sicily, near Millelli, near the town of Augusta, and the ancient Megara. Being burnt in a candle, the bituminous smell will soon be perceived; and it is affirmed, that this stony body, being recently severed from its mine and bed, is flexible like paper; but being long exposed to the air and sun becomes brittle. And the herbs that grow on this stone, insinuate their fibres and roots between its several coats.

Thirdly, an extraordinary sanguisuga or leech, found sticking to the fish called Xiphias or sword-fish, slightly mentioned by Gesner in his book de Aquatilibus, and Johnston in his book de Piscibus.* Our presenter gives it the name of Hirudo or Acus caudâ utrinque pennatâ, because of its working itself into the

* It is the *Pennatula filosa* of Linnæus.

flesh, and sucking the blood of the said fish. It is about 4 inches long; its belly white, cartilaginous, and transparent; without eyes or head, that could be observed, but instead of a head, it had a hollow snout, encompassed with a very hard membrane, differing in colour and substance from the belly; which snout it thrusts into the body of the fish, as strongly as an augur is wound into a piece of wood, and fills it full of blood to the very orifice. It has a tail shaped like a feather, serving for its motion; and under it, two filaments or slender fibres, longer than the whole insect, by which it seems to cling about stones or herbs, and stick the closer in the body of the sword-fish; attacking those parts only, where the fins of the fish cannot touch or trouble it. Within its belly he observed some vessels, like small guts, reaching from one end of it to the other, which by the pressure of his nail he made reach to the orifice of the snout, whence they retired back of themselves to their natural situation. They seem to be the instruments for sucking the blood, because the snout is in itself an empty part, destitute of fibres and valves to draw and suck with; whereas these vessels have a motion resembling that of a pump, in which the snout of this animal serves for a sucker, drawing the blood from one end to the other: and the belly of this insect being framed ring-wise, this structure serves to thrust the said inner vessels into the orifice of the trunk, and to draw them back again. As it torments the swordfish, so is itself infested by another insect, which he calls a louse, of an ash colour, fastened towards the tail of this leech, as firmly as a sea snail is to a rock. It is of the size of a pea, and has an opening, whence come out many small winding and hairy threads.

Fourthly, a parcel of sal ammoniac, brought from Sicily, where it had been gathered in the late fiery eruption of Mount Ætna, found on the surface of that ferruginous matter, which remained of the burnt minerals. Some of this salt was as yellow as saffron; some like citron-colour; some white, and some greenish. This seemed to be a factitious salt, such as is sold in shops, being a concrete of nitre, sulphur and vitriol burnt and sublimed, and not pre-existing in those caverns; adding some of this sal ammoniac to pulverised sulphur and nitre, he found, that it was so far from being kindled by fire, that it manifestly hindered the ascension of the brimstone and salt-petre, which were even extinguished by it, as if water had been poured on them.

Lunæ ad fixas Appulsus, Derbiæ Anno 1674. *Observabiles, ab Ephemeride Doct. D. Heckeri deducti, et brevibus notis descripti, à J. Flamsteed.* N° 99, p. 6162.

Not now sufficiently interesting to be reprinted.

An Account of some Books. N° 99, p. 6166.

I. Pharmaceutice Rationalis, sive Diatriba de Medicamentorum Operationibus in Humano Corpore: Auth. Tho. Willis, M. D. in Univ. Oxon. Prof. Sedleiano, nec non Coll. Med. Lond. et Societ. Reg Socio. E. Theat. Shel. 1673, in 4to.

This book being in the possession of most physicians, an analysis of it is deemed superfluous.

II. Johannis Hevelii Machinæ Celestis Pars prior, Organographiam Astronomicam plurimis Iconibus illustratam et exornatam exhibens, &c. Ged. 1673, in fol.

The famous author of this work, having given us in the preface a learned account of the origin and progress of astronomy, and of the succession of the chief astronomers from the beginning, among whom he accounts the most eminent to have been Hipparchus, Ptolomy, Copernicus and Brahe; and having also taken notice of the advancement made of that science in our age, in England, Germany, France, Italy, &c. and considered that without a due restitution of the fixed stars to their places, nothing considerable and accurate could be performed by astronomers, for establishing the motion of the planets, and for perfecting astronomy; he resolved to apply himself with all care and diligence to that work: for the prosecution of which, a great apparatus of instruments being requisite, he gives in this volume an ample description of them; reserving the observations themselves for another volume, already committed to the press.

In this vol. then, he first treats in general of the instruments used both by the ancients and moderns, and of what is chiefly remarkable in them. Secondly, describes in particular his own instruments, especially those that are made of solid metal, his quadrants, sextants, octants, &c. Thirdly, as astronomy has been greatly improved by telescopes, he takes occasion to mention what glasses have been made, by himself and others, especially one that draws 140 feet, made and presented to him out of Poland, by Signor Burattini. Lastly, he subjoins a discourse on his skill and way of grinding glasses of a hyperbolical figure, and of his actual performances therein.

His thoughts on telescopical sights, and his exceptions against them, delivered page 294 et seqq, we leave to the consideration of those, that prefer them to the common ones.

III. A Treatise of the Bulk and Selvage of the World, &c. By N. Fairfax, M. D. London, 1673.

IV. Apologema pro Urinis Humanis; Auth. Antonio Eygel, M. D. et Practico Amstelodamensi. Amstelod. 1672, in 8vo.

On a Subterraneous Fungus, and a Mineral Juice. By M. Lister. N° 100, p. 6179.

The fungus subterraneus is found in a rocky lime-stone ground, on a common about two miles from Castleton in the Peak of Derbyshire, 15 or 16 yards deep in the Old Man, as they call a mine formerly wrought, and now stopped up, covered with earth, that had either fallen or was thrown in. There is no coal-bed hitherto discovered within five or six miles of the place. This fungus does not seem to me to have any constant shape; the pieces that I received are much like peat or turf, both in the sooty colour and inward substance; only this is more clammy and tough, and does not dry. And some of this fungus substance is very soft, and like jelly. In and about the more solid pieces, are many large lumps of a bituminous substance, which is very inflammable, like rosin. It is light and breaks firm, and shines like good aloes; being also somewhat like it in colour, save that it is darker and purpleish; yet there is much of it of a dark green colour. We distilled a part of it, which yielded an acidulous limpid water; then a white liquor, probably from some of the oily parts precipitated; and in the last place, a copious yellow oil, not unlike that of amber or pitch. Whether this fungus owes its original to a vegetable, or is truly a concrete mineral juice, and a fossil bitumen, I forbear to determine; but the finding of it in an old mine, favours much the opinion of its being a vegetable substance; either the very substance of the props of wood, used in lining and supporting the grooves, are thus altered, or certain funguses grow out of them. That birch, of which there is great plenty, and has been vast woods in all these mountainous parts of England, will yield a bitumen, as limpid as the sap which runs from it by tapping, if we now had the skill to extract it: Pliny is very express, l. 16, c. 18. Bitumen ex Betula Galli excoquunt. And besides it is certain, that much of that wood, if not all, which is dug up in the high moors of Craven, and which the people use there for candles, and call candle-wood, is no other than birch, as it appears from the grain and the bark; though it exudes a rosin, which makes many pronounce it real fir-wood. Whatever this bitumen is, which this fungus subterraneus yields, it differs much from the asphaltum of the shops.

There is another mineral juice in these parts of England, resembling cream both in colour and consistence. It was found in great quantities at the bottom of a coal-pit, 49 yards deep. At Sheriff-Hales, in Shropshire, in the iron mines, especially that which the country people call the white mine, which yields the best iron-stone. On the breaking of a stone, they commonly meet with a great quantity of a whitish milky liquor, inclosed in its centre; sometimes they find the quantity of a hogshead contained in one cavity. Its taste is sweetish, only it has a vitriolic and iron-like twang.

*Of Trochitæ and Entrochi.** *By Mr. Lister.* N° 100, p. 6181.

I send you an account of some of the parts of certain stones figured like plants; which Agricola (5° fossilium) calls trochitæ, and the compound ones entrochi; we in English, St. Cuthert's beads. Agricola thinks them akin, for substance, to the lapides judaici; and indeed these are of an opaque and dark coloured spar; though some are of a white spar or cawke, as the miners call it: they all break like flint, pollished and shining. Put into vinegar, they bubble, and are dissolved. But this is true of all fossils, whatever their figure may be, that vinegar will corrode and dissolve as a menstruum; provided they be broken into small grains, and if the bottom of the vessel hinder not, they will be moved from place to place by it.

The figure of trochitæ is cylindrical; the utmost round or circle is in general smooth; both the flat sides are thick drawn with fine and small rays, from a certain hole in the middle to the circumference. Two, three, or more of these trochitæ joined together, make up that other stone, called entrochos. The trochitæ or single joints are so set together, that the rays of the one enter into the furrows of the other, as in the sutures of the skull. They are found very plentifully in the scars at Braughton and Stock, small villages in Craven.

As to their size, I never yet met with any much above two inches about; others are as small as the smallest pin, and of all magnitudes between these two. They are all broken bodies; some shorter pieces, some longer, and some of them real trochitæ, that is, single joints only. I never found one entire piece much above two inches long, and that very rarely too; in some of which long pieces, I have reckoned about 30 joints. And as they are all broken fragments, so they are found scattered, and lying confusedly in the rock; which in some places, where they are to be had, is as hard as marble; in other places soft and shelly, as they call it, that is, rotten and perished with the wet and air. And though in some places are but sprinkled here and there in the rock, yet there are whole beds of rock of vast extent, which for the most part consist of these, and other figured stones, as of bivalve, serpentine, turbinate, &c. as at Braughton.

The injuries they have received in their removal from the natural position, if not the place of their growth and formation, are manifest. For, besides their being all broken bodies, many of them are depressed and crushed, resembling

* The petrified bodies described in this paper belong to different species of the coral tribe, and principally to those of the Linnæan genus *isis*. Those at fig. 25 and 26 are the knots or thicker joints of some radiated *isis*, and are allied to the species called by Linnæus *isis entrocha*.

the real cracks as of a stone or glass. Again, such stones as consist of many vertebræ or joints, are many of them strangely distorted; sometimes two, three, or more of the joints in a piece are dislocated and out of their places; and sometimes a whole series of them. Others seem twisted like a cord. Lastly, some have their joints regular, but stuffed with a foreign matter, as when bricks are laid in mortar.

There is great variety as to the thickness of the trochitæ or single joints: some are so thin, that they are scarcely the 24th part of an inch; others are a full quarter of an inch thick; of these latter I only found at Stock: there are joints of all measures between these two extremes; but they are mostly of an equal thickness in one and the same piece: and there are slender and small entrochi, which have as thick joints, as the largest and fairest pieces.—There is also some difference in the seams of the joints: some are but apparently jointed; which appears by this, that if they be eaten down a while in distilled vinegar, the seeming sutures will vanish as in some I had out of Staffordshire, from about Beresford on the Dove: others, and all here at Braughton and Stock, are really jointed, and the sutures indented; which indentures being from the terminating of the rays, they are larger, according to the difference of the rays; but even, equal and regular.

Generally the utmost circle of each joint is flat and smooth; yet are there many other differences to be noted as to that part: very probably because they are parts or pieces of different species of rock plants.

The Explanation of the Figures above described. See pl. 5.

1. A trochites or single joint, with very fine and small rays.—2. A trochites or single joint, with the pith bored through like cinquefoil.—3. A trochites or single joint, of an oval figure, the rays scarcely apparent, and a very small point in the place of the pith.—4. A single joint of two of a middle size, with the pith very large.—5. A pack of single joints dislocated, and yet adhering in their natural order.—6. A very long entrochos, or a piece of many smooth joints, with the branches broken of.—7. An entrochos with smooth joints not branched.—8. The largest entrochos I have yet seen, with stumps of branches.—9. A smooth entrochos with very thin and numerous joints.—10. The deepest jointed entrochos, except the oval one in fig. 3.—11. An entrochos with very many disorderly knots in each joint.—12. An entrochos with only one single circle of knots in the middle of each joint.—13. An entrochos with three circles of knots.—14. A smooth entrochos, with a large and much raised edge in the middle of each joint.—15. Alternate joints round or blunt.—16. A double edge in the

middle of each joint.—17. Alternate joints edged.—18, 19, 20. Certain other differences noted in the paper, but not perfectly expressed in the design.—21. An entrochos with a branch of a good length.—22. A branch of an entrochos knocked off.—23. An entrochos fruit-like.—24. A fastigium or summitas.—25. A radix of an entrochos in perspective: where A is a joint or trochites yet remaining, whence an entrochos was broken off. C, E, F, D, are four of the double feet; the 5th being hid.—26. The same radix to be seen at the best advantage: A the trochites or basis; C, B, D, E, F, the five double feet. Note also the sexangular rough plates, which incrustate the stone or cover it all over.—27. A smaller radix with smooth plates and five single feet: H, the top stone; I, one of the five feet; K, one of the five angular plates which incrustate the middle of the stone; G, the basis. Also the same stone in perspective; G, the same with the hollow bottom upwards.

Figures of Plates supposed to incrustate divers Roots.

28. A pentagonal plate knotted.—29. A thin edged smooth pentagonal plate.—30. An indented pentagonal plate.—31. The Northamptonshire pentagonal plate.—32. A large pentagonal smooth plate.—33. An hexagonal plate, imbossed with angles.—34. An hexagonal plate, as deep as broad.—35, 37. Odd figured plates.—36. A quadrangular plate, ribbed and indented.

Mr. Ray relates, that there are found at Malta certain stones, called St. Paul's batoons, which he supposes were originally a sort of rock-plants, like small snagged sticks, but without any joints; the trunks of which diminish according to the proportion of other plants after the putting forth of their branches.

A farther Description and Representation of the Icy Mountain, called the Gletscher, in the Canton of Berne in Helvetia; which was formerly taken notice of in N° 94 *of these Tracts.* N° 100, p. 6191.

This paper is not sufficiently important to require reprinting. For descriptions of this and other glaciers, see Coxe's Travels through Switzerland.

A New Way by an English Manufacture, to preserve the Hulls of Ships from the Worm, &c.; better for sailing, and more cheap and durable than any Sheathing or Graving hitherto used. N° 100, p. 6192.

The method of sheathing here alluded to, is not described in this paper. From some hints thrown out, however, it is guessed to be principally a casing of thin sheet lead. But whatever it was, it seems to have been long since laid aside, in favour of the copperplate sheathing now commonly used.

An Account of two Books. N° 100, p. 6194.

I. Musica Speculativa del Mengoli, Dottor dell' una et l'altra Legge, et P. P. de scienze Mechaniche nello Studio di Bologna. Bolog. 1670, in 4to.

In this work, the author gives a minute anatomical description of the ear, and the mechanism or manner in which he thinks sound is conveyed to the mind, by the pulses of air acting on the ear. He afterwards treats on the theory of music, after his own way.

II. Georgii Wedelii M. D. Specimen Experimenti Novi, de Sale Volatili Plantarum. Franc. 1672, in 12mo.

END OF VOLUME EIGHTH OF THE ORIGINAL.

Description of Nova Zembla; by a Person at Amsterdam. N° 101, p. 3. *Vol. IX.*

I herewith send you what I have received out of Muscovy, which is a new map of Nova Zembla and Weigats, as it has been discovered by the express order of the Czar; and drawn by a painter, called Panelapoetski, who sent it to me from Moscow; by which it appears, that Nova Zembla is not an island, as hitherto it has been believed to be; and that the Mare Glaciale is not a sea, but a sinus or bay, the waters of which are sweet. Which is also affirmed by the Tartars who have tasted those waters in the very middle of the bay. The Samojeds, as well as the Tartars, unanimously affirm, that passing on the back of Nova Zembla, at a considerable distance from the shore, navigators may well pass as far as Japan. And it is a fault in the English and Dutch, that seeking a passage to Japan on the south-side of Nova Zembla, they have almost always passed the Weigats. The Weigats itself is very difficult to pass, because of the great quantity of ice falling into it from the river Oby, by which the strait passage is stopped up. The Samojeds go every year a fishing on the said sweet sea, and that on Nova Zembla side.*

On extracting Volatile Salt and Spirit out of Vegetables. By Dr. Daniel Coxe, F. R. S. N° 101, p. 4.

In this paper, the author states that he has extracted from various vegetables

* Notwithstanding what is said in this paper, concerning Nova Zembla, and the north-eastern passage to Japan and China, modern geography, and probably with reason, represents the first as an island, and the voyage by that route as impracticable at least, if not impossible.

a volatile salt and spirit, by the following process: Take in warm weather, a considerable quantity of the leaves of any vegetable, stripped or pulled from the greater stalks; lay them on a heap, pressing them pretty close together, and they will soon become very hot, especially in the middle; and after a few days resolve into a pappy substance, excepting the outward leaves; then being made into pellets, and put into a glass-retort, and distilled, they will yield, besides a great quantity of liquor, much thick black oil, of the consistence of a balsam. The liquor being separated from the oil, and distilled in a tall glass body, a volatile spirit sublimes, which, after one, two or three rectifications, becomes perfectly urinous, and not to be distinguished, by smell or taste, from well-rectified spirit of hartshorn, blood, urine, or sal ammoniac.

On Stones of a perfect Gold Colour, found in Animals. By Dr. Johnston of Pontefract; and communicated by Mr. Lister. N° 101, p. 9.

In the German Philosophic Ephemerides of the year 1672, I meet with these words of Dr. Wedelius, Obs. 246, page 439, Possideo particulam Calculi vaccini, instar Auri foliorum fulgidi; the subject of that observation being an enumeration and the description of several stones found in divers animals, as in dogs, hogs, stags, and in cows also. I beg Dr. Johnston's pardon for having kept by me two years an observation of this nature, which he was pleased to communicate to me, and which yet was so surprising, that I had not the assurance to offer it to you, being in this, as well as in all other matters, relating to the phænomena of natural history, very diffident. Being a little more confident since I read the words of Dr. Wedelius, that the stones sent me by the learned doctor were such indeed, and not some insect's eggs, as I once really persuaded myself they were.

In the year 1671, one Thomas Capidge, a butcher of Pomfret, killed an ox for the shambles; in which nothing was observed preternatural, till the bladder being blown by his servant, there was something observed sticking to the inside with a duskish froth. Keeping the bladder half blown, the butcher's son, who first discovered it, knocked with his hand on the side and the bottom of the bladder, to make it settle to the neck, and by shaking and squeezing it, got out the froth, and about 200 little globular stones, of several sizes, the largest being about $\frac{1}{10}$ of an inch diameter; others like pin-heads or mustard-seed. They were of a duskish yellow colour, and smooth. Some he broke, and the rest he kept in a paper; which, when dry, were like seed-pearl, but more smooth, and of a perfect gold-colour, and so continued ever after. Viewed in a microscope, they appeared very polished, and without any rugosities: the figure in most was spherical; in some a little compressed; the colour like burnished gold. I broke one or two of them with some difficulty, and I found by the microscope, that it was only a thin shell that was so orient and bright, the inner side of which shell was like unpolished gold: the inmost substance was like brown sugar-candy to the naked eye, but not so transparent: the taste was not discernible. In spirit of vitriol they shrunk much and wasted, but continued their colour. Likewise aqua fortis would corrode and dissolve them tumultuously.

York, March 12, 1673-4.

On the Origin of Pearls. By M. Christ. Sandius. N° 101, p. 11.

The pearl shells in Norway, and elsewhere, breed in fresh water. Their shells resemble those commonly called muscles, but they are larger. The fish in them looks like an oyster, and it produces a great cluster of eggs, like those of crawfish, some white, some black, which latter become white, the outer black coat being taken off. These eggs, when ripe, are cast out, and then grow, becoming like those that cast them. But sometimes it happens, that one or two of those eggs stick fast to the sides of the matrix, and are not voided with the rest. These are fed by the oyster against its will, and they grow, according to the length of time, into pearls of different sizes, and imprint a mark both in the fish and the shell.

This account M. Sand had from a Dane, called Henry Arnoldi, a person, he says, of veracity, and who had made the observation himself, at Christiana, in Normandy. [This Mr. Arnoldi had a very erroneous notion concerning the notion of pearls, which are certainly not the eggs of certain muscles and other bivalves, in which they are found.]

An Account of some Books. N° 101, p. 12.

I. An attempt to prove the Motion of the Earth from Observations, made by Robert Hook, F. R. S. London, in 4to, 1674.

The ingenious author of this attempt, having considered with himself, that the grand controversy about the earth remains yet undetermined; and finding there was no better means left for human industry to decide it but by observing, whether there be any sensible parallax of the earth's orb among the fixed stars; did thereupon resolve to employ himself in making some observations concerning so important a point in astronomy. His method, which he gives an account of, is to observe by the passing of some considerable star near the zenith of some place, whether such a star does not at one time of the year pass nearer to the zenith, and at another farther from it.

Accordingly he affirms to have actually made four observations; by which, he says, it is manifest, that there is a sensible parallax of the earth's orbit to the star in the dragon's head, and consequently a confirmation of the Copernican system against the Ptolemaic and Tychonic. At the end of the explication he mentions some things, which he looks upon as very remarkable, occurring in those observations; one of which was, that in the day time, the sun shining very clear, he observed the bright star in the dragon's head to pass by the zenith as distinctly and clearly as if the sun had been set; which he esteems to have been the first time that the stars were seen when the sun shone very bright; that tradition, of seeing the stars in the day with the naked eye out of a deep well or mine, being by him judged a mere fiction, a thing he had deemed impossible.

Lastly, he promises that he will explain to the curious a system of the world, differing in many particulars from any yet known, but answering in all things to the common rules of mechanical motions; which system he here declares to depend on three suppositions: 1. That all celestial bodies whatsoever have an attraction or gravitating power towards their own centres, whereby they attract, not only their own parts, and keep them from flying from them, as we may observe the earth to do; but also all other celestial bodies that are within the sphere of their activity. 2. That all bodies whatsoever, that are put into a direct and simple motion, will so continue to move forward in a straight line, till they are by some other more effectual power deflected and bent into a motion that describes some curve line. 3. That these attractive powers are so much the more powerful in operating, by how much the nearer the body, acted on, is to their own centres.

II. Medicina Militaris, or Body of Military Medicines experimented, by Raymundus Mindererus, late chief Physician of the Electoral Court of Bavaria, &c. Englished out of High Dutch. London, 8vo. 1674.

In the present improved state of pharmacy, it would be little satisfactory to our readers to detail the contents of this obsolete military pharmacopœia. The author, however, was a very celebrated physician in his days. He was born at Augsburg, where he practised, and died in 1621. Besides this and other works, he wrote a treatise de Pestilentia. That useful diaphoretic medicine, which is formed by the saturation of the vol. alkali with vinegar, has continued, till within these few years, to be named after him. In the Pharmacopœia of the Lond. Coll. it is now called aqua ammoniæ acetatæ.

III. Ephemeridum Medico-Physicarum Germaniæ Annus Tertius, &c. Lips. et Franc. 1673, in 4to.

For the reason mentioned at the conclusion of the 1st volume of this abridgement, we omit an enumeration of the contents of this miscellaneous work.

IV. England's Interest and Improvement, consisting in the Increase of the Store and Trade of this Kingdom, by Samuel Fortrey, Esq. Which Tract was first published An. 1663; and is again newly addressed to his Majesty. In two or three sheets it contains, I think, a great treasury.

V. A Discourse of the Fishery, briefly laying open not only the Advantages and Facility of the undertaking, but likewise the absolute Necessity of it, in order to the well-being both of the King and People; asserted and vindicated from all material objections, by Roger L'Estrange, Esq. In one single sheet he says, in our opinion, more to the purpose than some would have done in a large volume.

Considerations of M. Leewenhoek, touching the Compression of the Air. N° 102, p. 21.

In this paper, M. Leewenhoek describes an experiment he made to compress air, or rather, as he erroneously imagined, to force it through solid glass. He took a long tube of glass, close at one end, and a plug or piston to fit it very tight, that so the air might not escape by the side of it. By pushing this plug into the tube, full of air, with much force, he was able to force it to within the 100th part of the length of the close. Whence, instead of inferring that the air was compressed into the 100th part of its space or volume, he concluded that he had forced nearly all the air, through the pores of the solid glass.

More Microscopical Observations made by the same Leewenhoek. N° 102, p. 23.

I have observed by the microscope, that blood consists of small round globules, driven through a crystalline humidity or water. I have likewise observed some of the sweet milk of cows, and find that also to be made up of small transparent globules, carried in the same manner as in the blood through a clear liquor. I observed the hair of an elk, and found it wholly to consist of conjoined globules, which by my microscope appeared so manifestly to me, as if they could be handled. And therefore having so clearly seen those globules, I assure myself, that the growth and increment of hair is made by the protrusion and driving on of globules. This hair of the elk I find to be within much hollower, than that of men or of other animals. Again, I also observed a nail of my hand, and found it likewise to be made up of globules, not doubting but that it also grows from globules protruded. Having formerly spoken of the louse, her sting, &c. I cannot here omit to say something of what I have seen within that creature. I have several times put a hungry louse upon my hand, to observe her drawing blood from thence, and the subsequent motion of her body, which was thus: the louse having fixed her sting in the skin, and now drawing blood, the blood passes to the fore part of the head in a fine stream, and then it falls into a larger round place, which I take to be filled with air. This large room being, as to its fore part, filled about half full with blood, then propels its blood backward, and the air forward again; and this is continued with great quickness, while the louse is drawing the blood; except that at times she stops a little, as if she were tired, and recollects herself; a motion like that, it seems, which is in the mouth of a sucking infant: from thence the blood passes in a fine stream into the midst of her head, that being also a large round place, where it has the same motion. Hence it passes in a subtile stream to the breast, and thence into a gut, which goes to the hindmost part of the body, and with a curvity bends a little upwards again. In the breast and gut the blood is without intermission moved with great force, and especially in the gut, and that with such strong beatings downwards, and with such a retrocourse and contraction of the gut, that a curious eye cannot but admire that motion. In the upper part of the crooked ascending gut, which is very straight, now and then a little blood crowds through, which returns not back, and here I presume is a little valve: the blood, that is thrust through here, stands still, and soon receives another nature, becoming of a watery colour; and in this watery liquor there appear some blackish sandy particles, having a confused motion, which grow in size, and being grown as large as sand is to our eye, the said particles join themselves close and firm together, as it were in one mass, and then shoot down to the anus, carrying with them, in case the louse have much blood in her body, a little aqueous blood. These excreted particles appear like the excrement of a silk-worm.

Sir Samuel Moreland's Undertaking for raising Water. N° 102, p. 25.

The common and received opinion throughout Europe, has been, that if a given weight will force up water 20 feet high, there must be more than twice that weight to force it up 40 feet, and more than thrice that weight to force it up 60 feet, and so by a geometrical proportion in infinitum. And also, that a barrel of a pump, 6 inches wide, does not require a pipe, through which the water must be drawn up, above $1\frac{1}{2}$ inch, or 2 inches, at the most, in diameter.

Sir Samuel Moreland undertakes to demonstrate, 1. That he will force water 60 feet high with treble the weight that shall raise it 20 feet, and so proportionably in infinitum.—2. That by how much wider the barrel is, in which the forcer works, than the pipe through which the water is forced up, by so much is the engine pressed with unnecessary weight.

Of Parhelias. By M. Hevelius. N° 102, p. 26.

On Feb. 5, 1674, N. S. near Marienburg in Prussia, I saw the sun, in a sky every where serene enough, being yet some degrees above the horizon, and shining very bright, yet lancing out very long and reddish rays, 40 or 50 degrees toward the zenith (as in fig. 1, pl. 6.) Under the sun towards the horizon, there hung a somewhat dilute small cloud, beneath which there appeared a mock sun of the same size, to sense, with the true sun, and under the same vertical, of a somewhat red colour. Soon after, the true sun more and more descending to the horizon, towards the said cloud (as seen in fig 2), the spurious sun beneath it grew clearer and clearer, so as that the reddish colour in that apparent solar disk vanished, and put on the genuine solar light, and that the more, the less the genuine disk of the sun was distant from the false sun: till at length the upper true sun passed into the lower counterfeit one, and so remained alone; as appears in fig. 3.

This appearance being unusual, I took the freedom of imparting it unto you, especially since here the mock sun was not found at the side of the true sun, as it is wont to be in all parhelias seen by me, but perpendicularly under it; not to mention the colour, so different from that which is usual in mock suns; nor the great length of the tail, cast up by the genuine sun, and of a far more vivid and splendid light, than parhelias use to exhibit. Upon this appearance there soon followed here an exceedingly intense and bitter frost, whereby the whole bay was frozen up from this town of Dantzic, as far as Hela in the Baltic sea, which lasted till the 25th of March, and the bay was frozen so hard, that with great safety people run out into it with sleds and horses, for several of our miles.

Concerning the famous Kepler's Manuscripts, with some Considerations about the Use of Telescopic Sights in Astronomical Observations. In a Letter from M. Hevelius. Abridged and Translated from the Latin.* N° 102, p. 27.

—— I perceive that all your countrymen do not agree with me in opinion, on the subject of sights in astronomical instruments, which I have treated of in the

* John Kepler, one of the best astronomers and mathematicians of the 16th century, was born in Wirtemburg, in 1571; and obtaining a good education at Tubingen, he became professor of mathematics at Gratz. In 1600 he joined Tycho Brahe, then in Bohemia, as an assistant, by whom he was introduced to the emperor Rudolphus, who honoured him with the title of his mathematician. Kepler completed the astronomical tables which Tycho had left unfinished, and published them by the name of Rudolphine Tables. In 1613, at the assembly of Ratisbon, he assisted in the reformation of the calendar. After many difficulties and privations, he died in that city in 1630, at 59 years of age.

organographia of my machina cœlestis. But although Mr. Hook, and Mr. Flamsteed, and others, be of a different opinion, yet I have been, and still am taught by daily experience, that the matter is far otherwise in those large instruments, as quadrants, sextants, and octants, especially in azimuth quadrants, and other quadrants constructed of rulers or bars, which cannot so easily, or indeed by any means, be disturbed and inverted (while examining dioptric telescopes) as those instruments of 3 or 4 feet constructed with a perpendicular. The matter chiefly consists in this, that they can undertake no observation with their telescopic sights, till they examine and rectify them anew; in which examination there are various ways, and much room for mistake, however carefully it may be done. And indeed I cannot understand how this examination can be performed, in azimuthal quadrants, octants and sextants, at all times, with convenience, and without much loss of time.

There are some also I see, among whom is Mr. Flamsteed, who have undertaken to give judgment on my observations, whatever they may be, before they have examined or seen them. I am not given to a vain boasting of my own affairs, nor did ever imagine, that in this attempt at the restitution of the fixed stars, knowing my own weakness, that I should be perfect in every thing. But this I am convinced of, that if I had undertaken the business by means of telescopic sights, I must not only have wasted many years in fruitless examinations, but I should doubtless have been disappointed of my hopes, and that on various accounts, not necessary here to mention. Hence I congratulate with myself, that I never could adopt that opinion, but that whatever by God's help I have performed, it has been done after my own manner: but when we shall have observations made in both ways, continued for a series of 20 or 30 years, that is, as well those that may be made with telescopic sights, as those derived from the heavens by our plain sights; the matter will then be brought to a fair trial. In

Kepler was doubtless a genius of the first rank, and possessed a pure and rigid mathematical taste. He made some splendid discoveries; particularly the true laws of the planetary motions; viz. that they move in ellipses about the sun, describing equal areas in equal times; and that their periods and distances are all regulated by one general law, the squares of the former being always proportional to the cubes of the latter: laws however which he discovered, not a priori, by philosophical reasonings, but by tracing out and comparing their motions as recorded in the observations of Tycho: it being reserved for the immortal Newton to demonstrate the truth and indispensable condition of those laws. Kepler had some idea of the sun's restraining influence over the planets, which he judged to be an attraction of a magnetical nature. He was addicted to judicial astrology, and sometimes even fancied that the earth and sun were animated beings. Kepler's writings and works were very voluminous, and some of them very learned. Among them, is one on the gauging of wine casks, said to have an origin whimsical enough, being occasioned by a quarrel he had with an excise officer, whom he accused of an erroneous estimate in gauging the wine vessels used on occasion of his wedding dinner.

the mean time let every one enjoy his own opinion, and proceed in his own way.*

On Kepler's Manuscripts.

As to the manuscripts of the celebrated Kepler, which you enquire after, I purchased them all from his representatives for a certain sum, as well those that have been published, as those that have never yet been published; among which are a great many letters that passed between him and other celebrated persons. Among these MSS. are found several well deserving to be published, not only many letters, but some works also, among which is his Hipparchus, which, though not quite digested, might be easily put in sufficient order for that purpose. I have also his MS. chronology. But I do not find a written account of his life; though there are numerous facts and traits, from which a clear account of it might be given. But what I possess of his is contained in the following catalogue.

Then follows the catalogue, which is curious, but not of any use now, and too long to be copied here; especially as the whole of those papers were destroyed at the time of the dreadful fire, which consumed the books and observatory of Hevelius, in the year 1679.

An Account of some Books. N° 102, p. 31.

I. Johannis Schefferi Lapponia, &c. Franc. 1673, in 4to.

Describing the situation of the country, the climate and inhabitants, their persons, manners, customs, &c.

II. The True English Interest, or an Account of the Chief National Improvements; in some Political Observations, demonstrating an Infallible Advance of this Nation to infinite Wealth and Greatness, Trade and Populacy, with Employment and Preferment to all Persons. By Carew Reynel, Esq. in 8vo, 1674.

III. England's Improvement Revived, in Husbandry and Trade, by Land and Sea, &c. By John Smith Gent. in 4to.

* In the future numbers of these Transactions, we shall have occasion to notice several other papers on this subject, of the difference between plain and telescopic sights, by Hevelius, Hook and several other astronomers: of which also a connected account, pretty much at large, may be seen in the life of Hevelius, in Dr. Hutton's dictionary, vol. I. page 596.

Observations and Experiments on Vitriol, by a Fellow of the Royal Society. N° 103, p. 41.

The observations and experiments set forth in this paper, present nothing in the least degree interesting to modern chemists and mineralogists.

Some Inquiries and Suggestions concerning Salt for domestic Uses; and concerning Sheep; to preserve and improve the Race. By Dr. John Beal. N° 103, p. 48.

A very useless and uninteresting paper.

An Account of some Books. N° 103, p. 53.

I. About the Excellency and Grounds of the Mechanical Hypothesis, some Considerations occasionally proposed to a Friend, by R. B. E. F. R. S. Lon. 1674, in 4to.

A treatise, by Mr. Boyle, on the importance and nature of the mechanical philosophy, or that which consists in the affections of matter and motion.

II. On Mr. John Smith's England's Improvement received, in a treatise of husbandry and trade, by land and sea; plainly discovering the several ways of improving the several sorts of waste and barren grounds, and of enriching all earths, with the natural quality of all lands; and the several seeds and plants, which naturally thrive therein, observed, &c. Together with the manner of planting all sorts of timber trees and underwoods. Experienced in 30 years practice.

III. Davidis von der Beck, Mindani, Experimenta et Meditationes circa Naturalium Rerum Principia, &c. Hamb. 1674, in 8vo. dedicated to the Royal Society.

A Continuation of the Discourse concerning Vitriol, begun in N° 103; *showing, that Vitriol is usually produced by Sulphur, acting on, and concoagulating with, a Metal; and then making out, that Alum is likewise the Result of the said Sulphur; as also evincing, that Vitriol, Sulphur and Alum do agree in the Saline Principle; and lastly declaring the Nature of the Salt in Brimstone, and whence it is derived.* N° 104, p. 66.

The chief object of this paper is to prove what is now well understood, viz. that the saline principle, or more properly the acid obtained from vitriol, sulphur and alum, is one and the same. In the concluding part of these observations, it is very erroneously conjectured that "the saline principle of sulphur is identical with common salt."

An Extract of a Letter from M. Hevelius, lately written to the Editor, with a Copy of a Letter of Dr. Wasmuth, giving a large Account of a New Astronomico-Chronological Work, now by him preparing. N° 104, p. 74.

In this tedious advertisement, concerning a book on chronology, composed by a German author, there is nothing that is worth preserving in this abridgement.

An Account of some Books. N° 104, p. 78.

I. Observations touching the Torricellian Experiment, and the various Solutions of the same, especially touching the Weight and Elasticity of the Air. Lon. 1674, in 8vo.

A strange and futile attempt of one of the philosophers of the old cast, to confirm dame nature's abhorrence of a vacuum; and to arraign the new doctrines of Mr. Boyle and others, concerning the weight and spring of the air, the pressure of fluids on fluids, &c.

II. A Mathematical Compendium, collected out of the Notes and Papers of Sir Jonas Moore, by Nicholas Stevenson. Lon. 1674, in 12mo.

This volume contains abundance of useful practices in arithmetic, geometry, astronomy, geography, navigation, embattling and quartering of armies, fortification, gunnery, gauging and dialling; explaining also the logarithms with new indices; Napier's rods; making of movements, and the application of pendulums; with the projection of the sphere for a universal dial, &c.

III. Icones et Descriptiones Rariorum Plantarum Siciliæ, Melitæ, Galliæ, et Italiæ, Auth. Paulo Boccone, Panormitano Siculo, &c. E Theatro Sheldoniano, 1674.

This industrious and skilful botanist, having spent many years and much labour in the search after rare plants through Sicily, Italy, and other countries, thought fit to publish part of them in England, where he was himself not long since, and very generously left his engraven plates and manuscripts concerning the same, to be printed, as now they are, in the splendid Sheldonian theatre at Oxford.

IV. Navigation and Commerce, their Original and Progress: containing a succinct Account of Traffic in general; its Benefits and Improvements: of Discoveries, Wars and Conflicts at Sea, from the Original of Navigation to this day; with special regard to the English Nation: their several Voyages and Expeditions unto the beginning of our late differences with Holland; in which his Majesty's Title to the Dominion of the Sea is asserted against the novel and late Pretenders. By J. Evelyn, Esq. F.R.S. printed 1674, in 8vo.

Extract of M. Huygens's Letter, touching his Thoughts on Mr. Hook's Observations for proving the Motion of the Earth, noticed in N° 101 *of these Tracts.* N° 105, p. 90.

—— The observations of Mr. Hook for finding out the motion of the earth are very good, and of great consequence; but they must be continued, to see whether in the course of one or more years the parallaxes regularly answer to the annual motion of the earth. To which we also shall contribute our labours; and the vault in our observatory, being 28 fathoms deep, will in time be very useful for that purpose. This, if it succeed, will prove an almost entire conviction of the anti-copernicans, since there will remain for them nothing but this ungrounded subterfuge, to say, that the centre of the sphere of the fixed stars continually changes its place for an annual motion.

Extract of another Letter from Sig. Cassini, relating to the same Subject. N° 105, p. 90. *Translated from the Latin.*

Mr. Hook could not, in my opinion, in a better method investigate the parallaxes of the annual orbit in the fixed stars; nor select for that purpose a fitter star, on account of its lustre, its proximity to the vertex, and not far from the pole of the zodiac. Nor will his endeavour be useless, though it should turn out different from what is required by the parallax.—Indeed our observations could not hitherto show any thing of this kind distinctly, although in the meridian altitudes of the fixed stars, at various times of the year, we should have detected other differences, than those which follow from the precession of the equinoxes for advancing the stars.—We have a pit in the observatory ready prepared for this purpose, through which the heavens may be viewed, and useful observations made, &c.

Observations concerning the Comet seen in Brasil, March 1668, *by P. Valentine Estancel a Jesuit, and printed at Rome, in the* 9*th Italian Giornale de Letterati, Sept.* 31, 1673. N° 105, p. 91.

This is the same Comet as that noticed in N° 35 of these Tracts. There has not been a phenomenon this great while, of which before this, we have had observations from all parts of the world. Those from Europe and Asia may be seen in our 3d and 4th Giornale. Now I have received those of America, made in the city of St. Salvador, in the southern latitude of 12 deg. 47 min. Likewise the jesuit P. Pietro Susarte, rector of Macao, in the East Indies, writes to have seen the same all along the coast of Bona Speranza. As to the

Brasilian observations, on the 5th of March 1668 (N. S.), at 7 o'clock at night, Father Estancel began to see this comet a little above the horizon, from west to east-south-east. At the very beginning it appeared in its greatest bulk, and with extraordinary brightness, whereas usually they at first appear small, and then gradually increase. The beginning of its tail was a little under the two lucid stars, the 15th and 16th, of the Whale's back, over which it then passed, the whole length being about 23 degrees. The globe or head of it was so small and thin, that very few could discern it with the naked eye; and the father needed a telescope to see it well. The 7th of March, the former brightness was somewhat less, and become so thin, that the eye could easily see the stars that were behind it. The 8th, there were discovered more fixed stars behind the Chevelure; from the 8th, 10th and 11th day, until it came to the little Hare and to Eridanus, certain small stars, being in that celestial river, likewise appeared through it. The tail was always directly opposite to the sun. As to the colour of this comet, it was at first very splendid, and its rays were reflected from the sea to the shore, where the observers stood. But this brightness lasted only for three days, after which it considerably decreased. But what seemed remarkable was, that having lost so much of its light, yet its bulk was not diminished, but continued rather increasing till the comet disappeared.

Directions for Tanning Leather according to the New Invention of the Hon. Charles Howard of Norfolk; and a Machine for beating and cutting the Materials.* N° 105, p. 93.

Every part of the oak-tree, of what age or growth it may be, and all oaken copice wood, of any age or size, being cut and procured in barking time, will

* In the tanning of leather, some improvements were suggested by Dr. Macbride several years ago, (Phil. Trans. Vol. 68). This process has been further elucidated by modern chymists, and particularly by Mons. Seguin; and it now appears that the changes produced on hides, in the operation of tanning, are owing to the combination of a peculiar substance, called tannin, (present in oak-bark, galls, &c.) with the glue or gelatine of the skin. It has been supposed by the French chymist abovementioned, that the gallic acid (also present in those astringent vegetable substances), has considerable influence in promoting this process; but from the recent experiments of Mr. Davy, (Phil. Trans. for the present year, 1803), it may be inferred that this is not the case. One object which Mr. Davy proposed to himself in his experiments was to ascertain, what indeed had been before attempted by Mr. Biggin, (Phil. Trans. for 1799), in the case of the several barks used by tanners, the relative quantity of tannin in certain astringent vegetable substances; and the result shows, that catechu or terra japonica, contains this principle in the largest proportion. It would appear, that 1lb of catechu is, for the purpose of tanners, equal to 7 or 8 lbs. of oak-bark. As connected with this subject, we would further refer the reader to Mr. Davy's observations on the preparation of skins for tanning, inserted in the 15th Number of the Journals of the Royal Institution, for the present year 1803.

tan all sorts of leather as well at least as bark alone. This material being gotten in its proper season, it must be very well dried in the sun, and more so than bark; then housed dry, and kept dry for use; and when it is to be used, the greater wood may be shaved small, or cleft fit for the engine described below; and the smaller to be bruised and cut small by the same engine: which done, it must again be dried very well on a kiln, and then ground, as tanners usually do their bark. Such wood as is to be used presently after getting it, will require the more drying on the kiln; otherwise it will blacken and spoil all the leather. Where oak is scarce, thorns may indifferently well supply its place. Birch prepared and used instead of oak, is very fit for soal-leather. As these ingredients will tan better than bark alone, and that with far less charge; so may this invention save the felling of timber when the sap is up; which causes the outside of the trees to rot and grow worm-eaten; whereas, if the trees had been felled in winter, when the sap was down, they would have been almost all heart, as they call it, and not so subject to worms. Besides, this invention will greatly improve the value of underwoods.

The Description of the Engine above-mentioned, for Beating and Cutting the Materials of Tanning.

This machine is represented in pl. 6, fig. 4, 5, 6. It consists of a long square wooden block, and of some pieces of iron, to be fastened on it and used about it, viz. an anvil, a hammer, an iron holding the wood to be bruised and cut, and a knife to cut the same. Oak or elm is accounted best for the said block, the dimensions whereof are these: AB, in fig. 4, the length of the block, about 4 feet. CD, the breadth thereof, 15 or 16 inches. EF, the depth, 8 or 10 inches. The iron pieces are: GHIK, in fig. 4, a square cavity to receive a plate of iron, serving for an anvil, to beat and bruise the tanning materials on; which is to be about 4 inches deep, 9 inches broad, and 12 inches long. LMNO, in fig. 4, the iron for clasping and holding fast the materials, to be bruised and cut; which must lie across the engine, about the middle of the said piece of timber; and it may be about 3 inches broad. P, Q are two hooks at one end of it; which are turned upwards, and must be hooked into the loops of the two hinges that are let in and fastened to the side of the engine RS, in such manner, that this clasping piece may have liberty to be a little raised, for putting the tanning materials under it. At the other side T, in fig. 5, is a single hook, likewise turned upward, to hang a weight upon, whilst the stuff is bruising on the anvil, or cutting by the knife. The button, in fig. 4, serves to take up this piece by: aaaa, on the other side of the block, in fig. 5, are the places for the 4 feet to set this engine upon, which are to be of a convenient height to work on it: b the hammer, in fig. 6, for beating and bruising the stuff; which may be of 6 pounds weight, and have the head about 3 inches square, to work with both hands; but to work with one hand, or for a youth to use, let it be of about 3 pounds weight, and the head about 2 inches square. The surface of one end of these hammers is best to be smooth; but that of the other dented, the better to enter into the stuff for the greater dispatch. They are to be well steeled at both ends. The handles of these hammers may be about a foot long; the larger ought to be somewhat longer. cd is the knife in fig. 4, to cut the bruised stuff; which must be 8 or 9 inches broad, and near as much in depth, made like a tobacco-knife, with a handle to work. This knife must be fastened to the block at the two opposite sides, that are to be hollowed by two grooves, efgh in fig. 5, and iklm in fig. 4; and this fastening is to be performed by two pieces of iron, to be fitted in the said grooves, to hold and guide the knife in working; the one piece nopq, in fig. 4, is to be fastened to the end of the knife c by a pin r, passing through three holes; and this end is to be screwed into the groove efgk in fig. 5, by a couple of screw-pins; the other piece stuxyz in fig. 4, being forked, is to receive the other end of the knife d; and the solid square part thereof, iklm, is to be fastened in the groove under it, by two iron plates αα, εε, under which it must run in the said groove, so as that it may be slipped out from under it, and laid by, when the machine is not used; when also the piece at the other end may be unscrewed and laid up.

The two long squares on one end of the block, in fig. 4, viz. 5, 6, 7, 8, are two iron-plates to be fastened where the knife, moving in a fit cavity, is to cut the bruised stuff between them. And of these plates, that which lies next the end, is to be laid a little lower, the block being there pared accordingly, so that the stuff may fall off from the end of the machine the quicker, as your left hand furnishes the knife with the bruised materials, whilst the right hand is cutting them. Let the hollow place, where the knife cuts, be as near as may be, so large only, that the knife may easily fall and rise; and let the block be hollowed under the cutting hole, and sloped off at that end, for the stuff to fall off as it is cut by the knife.

A further Consideration on Snails. By Mr. Martin Lister. N° 105, p. 96.

I herewith send you the first part of our tables of snails, and some queries upon that subject; also the lively figure of each shell for illustration, done by Mr. Lodge. I reserve by me the sea-shells and rock-stones. Again, in that part of the tables, you have from me, authors are very little concerned; in the other, of sea-shells and stone-like shells, there are many authors, which are to be consulted and taken in, if possibly we can understand them, treating of the same species. As for rock-shells in particular, they come in to me in greater numbers, than I could ever have imagined. And I can assure you, that of near 30 species, I have now by me, found in this county alone, not any one can be sampled by any sea, fresh-water, or land-snail, that I have or ever saw. So that you see I have still good reason to doubt of their original, besides many other arguments that my observations about fossils afford, and which you may possibly one day see. And that there are the elegant representations of even bivalve-shells, which never owed their original to any animal, I can demonstrate; and think none, that have considered the thing with me, yet have denied. But whether all be so or no, I choose this method as the most convincing, viz. to give a comparative view.

Some General Queries concerning Land and Fresh Water Snails.

1. Whether there are other shell-snails at land than turbinate?—2. Whether this kind of insect are truly androgyna, and equally participate of both sexes, as Mr. Ray first observed; and whether both of them which shall be found in the act of venery, do accordingly spawn, or lay those perfectly round and clear eggs, so frequently to be met with in the surface of the earth and in the water too; and the circumstances of those eggs hatching?—3. Whether the way of fattening snails, in use amongst the Romans, that is, to make little paved places encircled with water, be not also very expedient in order to the true noting the manner of their generation?—4. What light the anatomy of this kind of insect may give to the rest?—5. Whether the black spots, observable in the horns of some snails, are eyes as some authors affirm, and not rather parts equivalent to the antennæ of other insects; as the flat and exceeding thin shape, also the branched horns, in other species of snails seem to confirm?—6. Whether the coccinea sanies, which some of our water-snails freely and plentifully yield, be not a saliva, rather than an extravasated blood. The like may be thought of the juice of the purple-fish, now out of use, since the great plenty of cochineal?—7. In what sort of snails are the stones mentioned by the ancients, to be found? And whether they are not to be found (in such as yield them) at certain times of the year? And whether they are a cure for a quartan; or what other real virtues they have?—8. What medicinal virtues snails may

1. Probably not.

2. Each individual appears to be truly androgynous, and it is probable that each deposits the eggs here mentioned.

5. Uncertain.

8. Common snails have long been considered as restorative in hectic cases, and the larger snail or pomatia particularly so.

have, as restorative to hectic persons; and what credit the Romans may deserve, counting them, especially the necks of them highly venereal; Celsus also particularly commending them to be boni succi and stomacho aptas.—9. Also inquire concerning the mechanical uses of the saliva of these animals, as in dying, whitening of wax, hair, &c.

N. B. The figures are numbered and explained by the tables. The figures of the naked snails are omitted in this specimen, being not material to that part of the design, which is, (when the other parts of these tables are finished,) to give the reader an exact view of animal-shells, as well as of fossils figured like shells, whereby he will be best able to judge, what to think of their original.

A Table of English Snails of the Land, River and Sea Species with their Figures. See Plate vi. N° 105, p. 99.

Cochleæ

TERRESTRES—testis intectæ; turbinatæ,

breviore figura, N° V.

1. Cochlea cinerea maxima edulis, cujus os operculo gypseo per hyemem tegitur, agri Hartfordiensis.
2. Cochlea cinerea et levitèr rufescens, striata, operculo testaceo cochleato donata.
3. Cochlea et colore et fasciis multa varietate ludens.
4. Cochlea subflava, maculata, atque unica fascia castanei coloris per medium anfractûs insignita.
5. Cochlea vulgaris major, hortensis, maculata et fasciata.

longiore figurâ

ad sinistram convolutæ, N° IV.

6. Buccinum exiguum subflavum, mucrone obtuso, sive figurâ cylindricâ.
7. Buccinum alterum exiguum in Musco degens, 5 anfractuum, mucrone acuto.
8. Buccinum rupium majusculum, senis orbibus protractum.
9. Buccinum parvum sive trochilus sylvaticus agri Lincolniensis.

ad dextram, N° II.

10. Buccinum pullum, ore compresso, circitèr denis spiris fastigiatum.
11. Buccinum alterum pellucidum, subflavum, intra senos ferè orbes mucronatum.

compressæ, N° II.

12. Cochlea cinerea, fasciata, Ericetorum.
13. Cochlea altera, pulla, sylvatica, spiris in aciem depressis.

TERRESTRES nudæ, limaces dictæ quibusdam, N° III.

14. Limax cinereus maximus, striatus et maculatus, lapillo sive ossiculo insigni, loco Cranii, donatus, locis udis et umbrosis degens.

FLUVIATILES turbinatæ, cochleæ, N° I.

15. Limax cinereus alter, parvus, unicolor, pratensis.
16. Limax ater.
17. Cochlea fasciata, ore ad amussim rotundo.

Buccina, N° V.

18. Buccinum flavum, pellucidum, intra tres spiras terminatum.
19. Buccinum alterum majus, paulo obscurius, pellucidum tamen, 4 Spirarum, mucrone acutissimo.
20. Buccinum fuscum, 5 Spirarum plenarum, mucrone sæpiùs mutilato obtusoque.
21. Buccinum subflavum alterum, 5 Spirarum, atque operculo tenui et pellucido, testaceo tamen cochleatoque donatum.
22. Buccinum longum sex spirarum, in tenue acumen ex amplissmâ basi mucronatum.

Compressâ testâ, coccum fundentes, N° III.

23. Cochlea pulla, ex utraque parte circa umbilicum cava.
24. Cochlea altera parte plena, et limbo donata, 4 circumvolutionum.
25. Cochlea minor, alterâ parte plana, sine limbo, 5 circumvolutionum.

Bivalvæ, N° II.

26. Musculus, parvus, subflavus, testâ pellucidâ, pisi magnitudine, palustris.
27. Musculus alter, fluminum maximus, subviridis.

MARINÆ, &c.

A Relation written to the Editor from a Person of great veracity in Germany, concerning an aged Woman of 60 Years, giving suck to her Grandchild, &c. N° 105, p. 100.

I cannot but impart unto you something which lately happened in my family, which is, that having taken two months ago a nurse for my little girl, since dead; the boy of that nurse having been on that occasion weaned, did by repeated sucking the breasts of his grandmother, a woman of three score years of age, cause such a commotion in her, that abundance of milk ran to her breasts for a sufficient nourishment to the said weaned boy, whom also my nurse, his mother, after she returned home upon the death of my girl, now again gives suck to, though her breasts had been for some weeks dried up.

So far this relation : which as it can be confirmed by many other like histories, given by very credible persons, so I shall here second it but with one only, recorded by the learned Diemerbroeck in the second book of his Anatome corporis humani, cap. 2, page 408; of which book an account is given at the end of this very tract. The relation is as follows: At Viana, a town very near us, some years ago, a poor woman, living before the town-gate, and being brought to bed of a fine boy not long after the death of her husband, and dying presently after her delivery, left her child behind her in good health ; but leaving nothing to keep a nurse to give the child suck, the grandmother of the babe being yet living, a woman of 66 years of age, but very poor also, and not able to pay a nurse, out of great pity to the poor child, attempted though at that age, to give it suck herself; in which undertaking she succeeded so well, that having out of her great commiseration put her crying grandchild several times to her breasts to suck, these breasts did from that old woman's strong imagination and vehement desire to give suck to this child begin to yield milk, and continued so to do with that plenty, that it was sufficient to feed the child, so that it hardly needed any other food; which all that saw it much wondered at, and which can be attested by many credible citizens of the said town.

As this author alleges this example to fortify his opinion concerning the cause that impels the chyle to the breasts, which he takes to be the mother's or nurse's strong imagination and passion to give suck; so he adds another, for the same purpose, known to himself, and happening in his own family, which is; that a little boy of his having been suckled for a while by his own mother, the author's wife; but being fallen very sick, and for great weakness unable to suck any more for 6 or 7 weeks, and consequently given over for dead; the mother, having cast off all hopes of giving it any further suck, let her milk dry up. But the child by great care recovering so far as to be able to suck again, and being put to a hired nurse, after the mother's breasts were dried up, and this nurse not using the child well, the mother out of great compassion to her child, did, about the end of the ninth month from the time of her being brought to bed, take the babe to herself again, and, whilst another nurse was sought after, with a thousand embraces she passionately wished and desired, she might have a full breast to give suck again herself. A nurse being found the same day, and the child put to her breasts, the wife of our author found at night, from her strong imagination and passion (says he), that her breasts, though not stroaked by her, nor sucked by the child, swelled again, after they had for eight whole months been quite dried up, and they yielded so much good milk, that, if the new nurse had not been hired, she could have given plentiful suck to the boy herself.

An Account of Two Books. N° 105, p. 101.

I. Tractatus quinque Physico-Medici, de Sale-Nitro et Spiritu Nitro-Aëreo; de Respiratione; de Respiratione Fœtus in Utero et Ovo; de Motu Musculari et Spiritibus Animalibus; de Rachitide: auth. Joh. Mayow, LL. D. et M.D. &c. Oxon. è Theat. Sheld. 1674, in 8vo.

The first part of this book treats of nitre, and the nitro-aërial spirit, premising a history of nitre, concerning which it teaches, what are its component parts; how it is produced in the earth; what the air contributes to its generation, and what the earth.

Having delivered the constituent principles of nitre in general, the author treats in particular of the acid spirit of nitre; affirming it to be produced partly by the air, and partly by a terrestrial matter. Where he refers the reader to those Boylean experiments,* which make it out, that the air furnishes something that is necessary to produce flame: which done, he teaches, that in nitre there exist igneous particles of the air, which constitute its most active part, and by which the flame of kindled nitre is produced, without any sulphur, which substance he will not at all admit to be found in pure nitre, being of opinion, that the deflagration of nitre is made, not by any sulphureous parts of its own, but by those fiery aërial parts, put into a very quick motion. Concluding upon the whole, that the aërial parts of nitre are nothing else but the igneo aërial particles thereof, requisite to make flame, and that this aërial part of nitre is lodged in the acid spirit of the same, and not in the fixed salt, which acid spirit, in his opinion, is compounded of a terrene matter, that is flexile and humid, and of ethereal corpuscles, that are rigid, dry, active and igneous, proceeding from the air. And the igneous particles, conceived by him to be common to nitre and air, he calls nitro-aërial, from whence the spirit of nitre derives its caustic and corrosive nature, which he calls a potential fire, and from whence he thinks also that the form of fire chiefly, if not wholly, depends. Now, forasmuch as this nitro-aërial igneous spirit resides in the acid spirit of nitre, he thence infers, that that nitro-aërial spirit is of a nitro-saline nature, obtaining rather the nature of an acid than fixed salt.

These things being thus by our author pre-supposed, he descends to the explication of the nature of fire, and makes its form and essence principally to depend on the said nitro-aërial spirit put into motion; rejecting the opinion of those, that will have fire producible by the subtile and briskly moved parts of any matter, and declaring on this occasion his dissent from those philosophers, that deduce all effects of nature from the same uniform matter, and the various modifications thereof; † which he thinks inconsistent with the phænomena of fire, not at all, in his opinion, producible but by a certain determinate kind of particles, such as he calls nitro-aërial. This he endeavours to prove by divers experiments.

Having done with the aërial part of the spirit of nitre, he proceeds to its ter-

* See Mr. Boyle's excellent Tracts, printed anno 1672, in London, the first of which contains new experiments touching the relation between flame und air, and about explosions; where among many other things, is evinced the efficacy of the air in the production of flame, even without any actually flaming or burning body: and where also the curious reader will meet with a full and very instructive account of those experiments, which this author here glances at.—Note by Mr. Oldenburg.

† Compare herewith the considerations of the Hon. Mr. R. Boyle, about the excellency and grounds of the mechanical hypothesis.—Note by Mr. Oldenburg.

restrial and acid part, and labours to show, how the spirit of nitre is produced in the earth. For the better understanding whereof, he premises something concerning the spirit of sulphur and other acid liquors; teaching, that that spirit does not exist in sulphur before deflagration or the operation of the fire; and affirming, that, as the nitro-aërial spirit of the fire, by a very brisk motion and effervescence, contending with and acting upon the salino-sulphureous particles, does in a very short time comminute and render fluid the saline parts included in the sulphureous; so the same spirit, boiling up by a more remiss motion with the same salino-sulphureous parts, does in a longer time turn the saline parts into an acid liquor. Where he takes occasion to discourse, how liquors in general become acid by the operation of the nitro-aërial spirit; as also, wherein fermentation consists, viz. in the effervescence of the nitro-aërial particles with the salino-sulphureous ones of the liquor.

According to this author, sal-nitre is made up of a threefold salt; whereof one, the most active, deduces its origin from the air, and is of an ethereal and igneous nature; and this by its architectonical power forms to itself out of a terrestrial matter a saline vehicle, which, together with the igneous salt residing in it, constitutes the spirit of nitre; which as soon as it is generated, falls to working upon the fixed salts of the earth brought to due maturity, and together with them makes up the common nitre. From this nitro-aërial spirit he derives all fermentations tending both to the production and dissolution of things. From the same he deduces rigidity, and particularly congelation, and the expansion made therein; where he examines the explication given by Descartes of the rarefaction of congealed water. And as he makes rigidity the effect of that spirit, so he would have the restitution of rigid and infected bodies, in which consists springiness, to result from the same. And acknowledging, from the many Boylean experiments,* that the air is endowed with a considerable spring, he attempts to give an account, whence that elastic power arises; taking it for granted, that the air contains store of those nitro-aërial particles, that to him are absolutely necessary to make fire, of which the air being exhausted by deflagration, the fire must needs be extinguished; and assuming thereupon, that the elasticity of the air proceeds from such aërial particles as maintain flame; having found by experiments, that air deprived of those nitro-aërial parts loses its springy virtue; which virtue he also affirms to be lessened by the respiration of animals, who, in his opinion, do exhaust out of the air certain vital, and those

* To be met with in Mr. Boyle's physico-mechanical experiments of the year 1660, at Oxford, and the continuation of the year 1669, at Oxford, where the reader may find a full information of what our author here declares.—Note by Mr. Oldenburg.

elastic particles, insomuch that he doubts not but that something aërial absolutely necessary to life, passes into the blood of animals by means of their respiration; whose necessity therefore he cannot acknowledge to arise from thence only, that thereby and by the motion of the lungs the mass of blood may be comminuted, as some have asserted: concluding at last, that fire and life are maintained by the same aërial parts; and giving a reason, why an animal is able to live in a receiver a while after a candle is extinguished, which is, that, in his opinion, there is required a greater quantity of aërial particles to the burning of a candle, than the maintaining of life. Whence yet he would by no means have it inferred, that though flame and life are maintained by the same particles, therefore the mass of blood is kindled, as some teach.

He imagines that the nitro-aërial particles, from which the spring of the air proceeds, are lodged in the very parts of the air, and carried away from it by the burning of a candle, or the respiration of animals; so that those nitro-aërial and elastic parts that come to fail are not the air itself, but the subtilest and the most active part thereof, which being expelled by burning or respiration, the air becomes effete, and destitute of its spring. Moreover he considers, how the air, when deprived of its nitro-aërial parts, is again supplied with them?*

He afterwards proceeds to explain in what manner the nitro-aërial spirit is breathed in by animals; and how it comes to lose in them its elastic power; and of what use it is being inspired; where he teaches that those nitro-aërial parts are in animals as well as in vegetables the principal instrument of life and motion; and that the fermentation both of the blood and the vegetable juice depends on the same.

He then inquires, whether air may be generated de novo, and, on this occasion, recites an experiment, which he says is like to one formerly made by Mr. Boyle, tending to prove the affirmative of the question. Concerning which, this author is of opinion, that though the aura, produced by that experiment, and by others here recited, be endowed with a no less spring than the air we breathe in; yet it is no true air, such as contains vital and igneous parts; for as much as that aura, wherein he found an animal and a lighted candle to expire, was, he says, endowed with a spring as well as an unviolated air, but destitute of nitro-aërial and vital parts. Mean time, he suggests an experiment to discover, whether the pretended new generated air be true air indeed and fit to maintain

* In the writings of this ingenious philosopher, we may easily trace, (as we have already hinted at p. 295, of the 1st vol. of our abridgement,) the outlines of the modern chemical theories concerning acidification, combustion, and respiration. Unfortunately for science, Mayow died at a very early age. By nitro-aërial spirit, he meant the same principle which the chemists of these days have denominated oxygen.

any life: and by that experiment determines this matter in the negative, though he denies not that there is a great affinity betwixt them.

After this, he spends a chapter in discussing how fire is kindled and propagated, and in what manner all fermentations are made, namely, by the pulsation of the Cartesian materia subtilis, whereby as by a substance that constantly moves, he says, the ignited parts are put into a vehement motion. And here he declares, that fire seems to him to be nothing else than a very great fermentation of nitro-aërial and sulphureous parts; and concludes, that as the most vehement motion of the igneous particles proceeds from thence, that the sulphureous ones pass into the particles of the nitre or air, and there hitting upon the briskly agitated materia subtilis, are by the impulse of the same, together with the nitro-aërial parts, found in the said substances, by an elastic impetus struck out; so all the more remiss fermentations of natural things are caused hence, that the said nitro-aërial particles, penetrating the salino-sulphureous mass, do enter into the body of the subtile matter, by which, being put in a great agitation, the said nitro-aërial parts are protruded together with the sulphureous ones; so that the effervescence of fire seems to him to differ from the more remiss intestine motions, by which vegetables are generated or dissolved, only in this; that in fire the nitro-aërial particles being closely joined with the fixed salt or parts of the air, are by the impulse of the sulphureous parts and the subtile matter carried away with violence, and put into a very swift agitation; whereas in other fermentations, the sulphureous parts not being lodged so fast in the embraces of the fixed salt, are by the pulsation of the nitro-aërial parts and of the materia subtilis moved with more remissness.

Next he discourses of light and colours, embracing the doctrine of Descartes, making light to consist in a pulse, which by reason of the continuity of the luminous medium is suddenly transmitted to the greater distance; but this impulse our author would have made by his nitro-aërial particles; as he is of opinion, that colours are produced not from a light reflected, but from a peculiar impulse of the medium, altogether different from that of light; concerning which and the manner of which, the discourse itself may likewise be perused.

To this he subjoins a chapter about lightning, which he denies to proceed from kindled exhalations; and having espoused the Cartesian opinion of the production of thunder from the impetuous fall of the upper condensed and conglaciated clouds upon the lower; he thence infers, that lightning is made by his nitro-aërial particles struck out of the air, and by their vehement igneous motion causing a slight and momentaneous flash spreading itself over the whole hemisphere. Where he adds his thoughts about the force of lightning, and its wonderful effects in melting swords without hurting their scabbards, in killing animals, in occasioning very tempestuous winds, &c.

So much concerning the first treatise. As to the other four, we shall not say much of the two former, viz. of respiration and the rickets, but hasten to make some mention of the two remaining parts, treating of the respiration of a fœtus in the womb and the egg, and of muscular motion and the animal spirits.

Touching the former of these, our author considering with himself, how a fœtus can live in the womb without the access of air, and finding the offices hitherto assigned to the umbilical arteries to be ill grounded, scruples not to affirm with the learned Everard, that the said arteries are formed chiefly, if not only, for the use of respiration, declaring, that the blood of the embryo, being conveyed through the umbilical arteries to the placenta uteri, carries to the fœtus not only the nutritious juice, but also with it a quantity of the nitro-aërial particles, whereby the blood of the fœtus, by its circulation through the umbilical vessels, is impregnated just as it is in the vessels of the lungs: whence he would not have that placenta called any more the liver, but the lungs of the womb. And this supplement for respiration he extends to the chicken in an egg, asserting, that the same does, no otherwise than a child in the womb, breathe by the said arteries; esteeming, that the primogenial liquors of the egg, furnished with a pure aërial substance, being incessantly conveyed through the umbilical vessels to the chick, perform to the same the office not only of nutrition, but of respiration also. To this he adds, that even that gentle warmth, excited in the egg by incubation, may also contribute something, there to supply the defect of respiration; forasmuch as he supposes to have proved in his treatise of respiration in general, that the nitro-aërial particles, by the blood's fermentation struck out of the parts of the air, serve animals for respiration; and that, as all heat proceeds, in his opinion, from such nitro-aërial particles put into motion; so in this case, the heat given by the incubating bird, and received and detained in the albumen, is thence collected by the many small suckers of the umbilical vessels, and so conveyed to the chicken. Upon which ground he undertakes to solve that difficult query, viz. why a fœtus after it is born, and yet closed up in its membranes, may yet live for some hours; whereas if, being divested of those skins, it have once taken air into its lungs, it cannot live a moment after without it? which he answers thus; that a fœtus born, and yet wrapped close within the membranes, is in a like state, and breathes much after the same manner, as a chick included in an egg. But if, those membranes being pulled away from the fœtus, it do, for breathing, with labour contract the muscles of the chest and the midriff, it spends in that muscular labour much more of those nitro-aërial parts than before; whence there is a greater necessity for the fœtus to breathe in the open air, there being now nothing to compensate the defect of that respiration.

Let us pass to the last, which is of muscular motion and animal spirits. It is undoubted, that the motion of animals is made by the contraction of the muscles; but it is controverted in what manner that contraction is made. The most received opinion is, that the fibres of the muscles are inflated by some elastic matter, swelling them as to their breadth, but contracting them as to their length; though the learned Steno in his Myology thinks it needless to take in a springy matter for the contraction of the muscles; forasmuch as he judges it may be effected by the sole change of their figure. Concerning which our author considers that it appears not; 1. How that motion, requisite to make a change in the figure, can be produced without the accession of some new matter, 2. How it comes to pass, if no new matter enters the muscle, that in its contraction it is so sensibly hard and tense. And whereas anatomists have hitherto taught, that the carneous fibres chiefly make the contraction in muscles, our author thinks it more probable, that the fibrillæ, transversely inserted into the greater fibres, perform the chief part in that contraction, by reason as well of their position as their size and number. And as to the cause of this contraction in these fibrillæ, he thinks, that besides the animal spirits, there are also required to this motion some of the salino-sulphureous parts of the blood; and that those animal spirits, that contribute to the animal motion, consist of those nitro-aërial parts, which he asserts to be transmitted into the blood by inspiration. And both these parts he judges necessary to this muscular motion, because he understands not how that animal motion can be performed without different particles mixed together and briskly moved; in regard that, in his opinion, it cannot be effected by springiness and weight, which do the work in automatons, since their impetus will soon cease: whence he concludes, that the muscular contraction is performed by the effervescence of the salino-sulphureous and nitro-aërial particles.

II. Anatome Corporis Humani, conscripta ab Isbrando de Diemerbroeck,* Med. et Anatomes Professore, Ultrajecti, 1671, in 4to.

* Isbrand van Diemerbroeck was a celebrated Dutch physician of the 17th century. He practised for several years at Nimeguen, and wrote an account of the dreadful plague, which raged there in 1635, 1636, and 1637. He afterwards removed to Utrecht, where he was appointed to the professorship of physic. He died in 1674, aged 65. Besides the abovementioned anatomical work, several other treatises were written by this author; viz. De Variolis et Morbillis; Observat. et Curationes Medicæ; Disputationes Practicæ, &c.; but his principal work is that De Peste, first published in 1646, 4to. and reprinted, some years after his death, with his other writings, under the title of Opera Omnia, Utrecht, 1685 folio. During the raging of the abovementioned pestilence, Diemerbroeck attended

There are now extant systematic treatises of anatomy, so much more complete than this, (which however possesses considerable merit, and was among the best at the time of its publication,) that we deem an account of it altogether unnecessary.

Microscopical Observations concerning Blood, Milk, Bones, the Brain, Spittle, and Cuticula, &c. By M. Leuenhoek. N° 106, p. 121.

The small red globules in the blood, formerly spoken of in N° 1024, are heavier than the crystalline liquor in which they are carried; because, soon after the blood is let out of the veins, those globules gradually subside towards the bottom; and consisting of soft fluid corpuscles, many of which lie on one another, they unite close together, by which conjunction the blood under its surface alters its colour, and becomes dark, red, or blackish.

The red globules of the blood I reckon to be 25,000 times smaller than a grain of sand; which perhaps will to many seem incredible: but the matter being about figured bodies, it is known that two globes being given, the axis of one whereof is 1, and that of the other 20, the proportion between their magnitudes is as 1 to 8000; spheres being in a triplicate proportion to their diameters. The same red globules, when they are single, and stick within to the sides of very slender glass tubes, will appear white and colourless.

I have several times endeavoured to observe the parts of a bone; and at first I imagined I saw on the surface of the shinbone of a cow, several small veins, but I have not found it since in any other bone. I thought likewise, I saw then also, that the bone consisted of united globules. Afterwards I viewed the shinbone of a calf, in which I found several little holes, passing from without inwards; and I then imagined that this bone had divers small pipes going longwise; but I have since observed the tooth of a cow, and found it made up of

vast numbers of the infected, many of whose cases he has related. Among other prophylactic measures to which he resorted, he particularly mentions the fumigation of the bed-chambers and apartments with the *acid vapours* produced by the burning of brimstone; directing those employed in this business, to withdraw from such apartments for an hour or two, until the fumigating process should be finished; lest their lungs should be injured by the irritating action of the sulphureous vapour. As he escaped infection himself, although he was constantly exposed to it in visiting the sick, all persons were eager to know what precautions he observed on this occasion. Accordingly, at the end of the 2d book, he gives a circumstantial account of his mode of living, while this contagion lasted; whence it appears that he used a generous, but not intemperate diet; that he banished all fear, and resisted as much as possible all depression of spirits; that he occasionally held in his mouth, and chewed cardamon seeds, elecampane-root, &c.; that he fortified his stomach with bitters and aromatics; and lastly, that he indulged freely in the smoking of tobacco; placing upon this measure more reliance as a preservative, than is warranted by the experience of succeeding observers.

transparent globules, which I can see very perfectly. The same I have observed in ivory or elephants' teeth. And I have no doubt but that all white bones do consist of transparent globules. I am also of opinion, that all things which appear white to our eyes, are made up of nothing but transparent particles lying one upon another, such as snow, white paper, linen, white stones, white wood, scum, beaten glass, beaten rosin, sugar, salt, &c.

The brains of a cow being viewed, I found the white substance of it to be made up also of very fine globules. As to the marrow of the back bone, I found that also to consist of very subtile globules. Having divers times observed the flesh of a cow, I found it consist of very slender filaments, lying by each other, as if woven into a film. I have also viewed several filaments which were beset with globules. These I judged to be blood, and that, pricking our body with a pin without hitting a vein, the bloody globules issued from between these filaments; but this I leave to further consideration. Mean time I have with a pin's point severed these filaments from one another, and found the single ones so fine, that any of them seemed to me 25 times thinner and finer than a hair. Having exposed them to my microscope, I saw to my wonder, that they were made up of very small conjoined globules, which in smallness seemed to surpass all the rest.

The cuticula, or uppermost skin of our body, consists of round parts or small scales. And I fancy that the continual growth of this cuticula is made in this manner; that the humidity issues forth from between all those round particles or scales lying close upon each another, and not through pores as many have taught. Like a close and well twisted cable, upon which pouring continually some water, this water will pass through the whole cable, and oose out at the end; not passing through any pores, but making its way about and between the filaments of the cable, and so getting out beneath. And the coarser or more consistent matter cleaves to the body, and so makes the uppermost skin; which thus grows on from beneath, and is worn off from above: and the more transparent these particles are, the whiter is our skin; which yet are but our conjectures and suspicions. And the like manner of growing I have formerly said to take place in plants, only with this difference, that when the superficies of a moist globule, which is driven out of the plant, is become somewhat stiff, the moisture is then propelled out of the upper end of the plant, and that by a continual succession. Which kind of progress of growing I apprehend may in some manner be seen in the pith of wood, in cork, in the pith of membranes, as also in the white of a quill.

Other Observations made by Mr. Leuenhoek, about Sweat, Fat, and Tears. N° 110, p. 128.

I have often viewed the sweat of men and horses, and found it consisted of a crystalline moisture, in which I saw many transparent globules moving with some odd larger parts, which I judged to be scalings off from the cuticula.

I formerly acquainted you, that I imagined I had seen hair as made up of united globules, and to have also observed elephants' hair consist of the like. I cannot omit now to communicate, that since then I have seen such globules, not only in human hair and horse hair, but also frequently in the wool of sheep; and further, that the root of the hair pulled out of the eye-brows, consists altogether of the like globules. Having pulled out of an elephant's tail a black hair, and cut transversely from it a thin scale, I exposed it to my microscope, which represented in the thick of that hair about a hundred little specks somewhat whitish, and in each speck a black point, and in some few of those black points a little hole; and this hair consisted of united globules, which yet I thought I should have found larger in this thick hair of so bulky a beast than indeed they were.

I lately viewed some blood in which there was much of the crystalline liquor; and going into the open air in high wind, I saw to my great delight, continually, and without any other motion but that of the wind, the red globules blown about, and as if each globule had yet a second motion, and that about its axis.

I have heretofore viewed the fat of sheep and cows, and showed to several of the curious, that it is made up of globules joined together, which appeared to my eye as large as ordinary hail-stones. And I have lately observed, that each globule of fat consists of more than a thousand small globules. Yet I am apt to believe, that those that have not seen the globules in blood, hair, bone, &c. will not satisfy themselves about seeing them in fat, because of their extraordinary minuteness.

Having viewed the tears of two infants, I found therein very few round globules, but other odd and misshapen particles of divers forms; some of which seemed to consist of united globules.

An Account of a remarkable Case of a Dropsy mistaken for Pregnancy in a young Woman; in which the Observations made on the same Case by Dr. Tulpius are considerably enlarged.* N° 110, p. 131.

Some years since, there came to Dr. D. in Holland, a young woman of about 17 years of age, unmarried, and reputed a maid, of a florid countenance, and strong body, having a good stomach, periodicè menstruata, and wanting none of other due evacuations; not troubled with head-ach nor sleepiness, nor difficulty of breathing, nor drought, nor any of the symptoms incident to hydropical persons. This young woman having her belly swollen to excess in three months time, was much suspected by him of incontinence; which yet with many imprecations she denied, though in vain: the physician disbelieving her assertions, and particularly that she had her periodical discharges, because she looked so well, nor had any signs of an hydropical distemper upon her, except the tumor of her belly, which being felt, afforded some considerable signs to dispossess the physician of the opinion he had of this person; seeing it was not a prominent nor roundish tumor, nor any such as is usual in women with child; besides her urine was not such as is usual to childbearing women: yet there appearing no symptoms of a dropsy, the physician sent her away, without giving or prescribing her any physic. Yet she returned soon after, importuning him to give her his advice against the dropsy. He consulting with other physicians about it, found them as distracted between both, as he himself was. Yet some of them, inclining rather to the opinion of a dropsy, with appropriate medicines made her void abundance of water, yet without reducing her belly. In 6 months, after consulting other doctors without effect, her body was dried and bloodless, her breath short, her temples fallen in, her nose sharp, her eyes hollow, her

* Nicolas Tulpius was born at Amsterdam in 1593, and was educated at Leyden. After taking his degree of M. D. in that university, he returned to his native city, and soon got into first-rate practice there. Being a man of great integrity, sound judgment, and extensive information, not only in matters appertaining to his profession, but in others also not immediately connected therewith, he was elected a member of the senate and burgomaster. In this capacity he rendered the most essential services to his country; for, in 1672, when the victorious army of Lewis XIV. had penetrated within 9 miles of Amsterdam, and the inhabitants, seized with a panic, were preparing to evacuate the place, he boldly stepped forth, although at the advanced age of 79, and in a firm and energetic harangue exhorted his countrymen to stand fast and defend their capital to the utmost extremity. This address had the desired effect. The soldiers and citizens returned to their posts; the garrison was well served; the guardships which lay in the harbour were quickly manned; the sluices were opened; Amsterdam was saved—This venerable magistrate and excellent physician died in 1674, aged 81. His Observationes Medicæ rank among the best practical works on physic, of the 17th century.

skin wan and ill-favoured, her pulse creeping, her appetite prostrate, her tongue dry, her voice weak, her evacuations sparing, &c.; but refusing to undergo the operation of tapping, she was left to herself, and died three months after. Her body being opened, there soon appeared a great lake of water; whence at first it seemed to be a common ascites, a tumor of waters stagnating in the abdomen. Then the liver being looked after, it was no where seen. Next the other viscera being sought for, viz. the mesentery, pancreas, spleen, and kidneys, none of them appeared, to the astonishment of all that were present, who searching further, and meeting with the peritonæum, found it to be turned into a bag, by a separation made of its interior membrane from its exterior, and so inclosing within it the whole bulk of that restagnant water, that not a drop of it could pass into the abdomen. And the compass of this bag formed by the two membranes, reached from the pubes to the diaphragm, and from the left region of the loins to the right; so that the nervous body of the peritonæum, was by little and little expanded, as the capacity of the womb in gravidation is still more and more enlarged, which yet had become very thick, being thicker and closer than any ox's hide, whereas naturally it is as thin as a silken web. This bag of the peritonæum being removed, the viscera came to view, which were not gravelly, nor tartareous, nor chalky, as they often are in hydropical bodies, but only decayed and colourless: which decay, by the timely use of an incision, might have been prevented.

Account of Three Books, N° 106, p. 134.

I. De Secretione Animali Cogitata, Auth. Guil. Cole, M. D. Oxon. 1674, in 12mo.

In this treatise a principal share in the business of secretion is erroneously attributed to fermentation.

II. Erasmi Bartholini Selecta Geometrica, Hauniæ, An. 1674, in 4to.

These Selecta Geometrica are:—First, a general method to come to an equation, by reducing all sorts of questions to general heads: where the learned author reduces to one head, for instance, all problems of proportionals, both arithmetical, geometrical, and harmonical; but treating here only of the two latter, and more largely of the last of all.—The second is his dioristice; in which, having observed how far short the ancient geometricians came of a general art of determining problems, he says he has endeavoured to supply that defect, by delivering here two general rules for determining the limits of equations, so as to know how many roots are possible: in the first of which he follows the rule of that famous geometrician Fermat: in the second he agrees with the method of the learned Hudden, contained in the annexa to the first part of Descartes's geometry; which seems indeed to be a corollary of Slusius's general method of tangents, published in N° 90 of these tracts.—The third is an Auctarium Trigonometriæ, to solve and demonstrate triangles, both rectilinear and spherical.

III. Logica, sive Ars Cogitandi, è Tertia apud Gallos Editione recognita et aucta, in Latinum versa. Lond. 1674, in 8vo.

This logic being now turned into good Latin, seems worthy to be recommended to all young students; as, omitting what is useless and pedantic, and comprehending what is indeed sober, and necessary to direct our reason in all sorts of ingenious and useful sciences.

An Observation, made by the Learned Dr. Sampson, of a Man Anatomized, whose Bowels were found inverted. N° 107, p. 146.

Mr. J. D. formerly a minister in Yorkshire, was troubled with a cough, and other complaints; for relief against which he took a journey to London, for the most part on foot: he lived not above a fortnight after he came up. In his sickness he was much addicted to drink brandy, which hastened his death. I opened his body with the assistance of two other physicians.

We observed his limbs to be much macerated; his belly was swelled with some inequality, especially in the tract of the right muscles: a considerable quantity of water was taken out of it; his guts inflamed, and extended with wind; his gall very viscid; his lungs inflamed, and beset with divers glandules. But that which most of all surprised us, was the inverted order of his bowels: his liver, which was very large, lay in the left hypochondre, and his spleen in the right; the cone of his heart was on the right side, and accordingly, the larger and thinner ventricle was found on the left; and the thick one, which in others is on the left side, was in him on the right. The great artery descended on the right side, and the vena cava ascended by his liver on the left. The œsophagus descended to the first orifice of the stomach on the right side, which made the pylorus and entrance of the pancreas be on the left, and the first flexure of the small guts to be towards the right: so that the beginning of the colon with its appendicula, lay at the left os iliôn, and the flexura sigmoïdæa towards the right. Other things, that necessarily followed this site, need not be mentioned.

It was forgot to inquire, on which side the lacteous thoracic duct ascended, or where it ended; or on which side the recurrent nerves took their places of returning about the trunks of the great artery and the axillary; nor had we time to do it.—This person was never observed in his life time to have any distemper, or usage, which might discover this inverted situation of his bowels, nor had this contraposition any evident influence upon his diseases and death. He was about thirty years of age, a married man, had several children, was of a middle stature, healthful till toward the latter end of his time: had no prominency on his left side more than the other; was not left-handed, nor had any weakness on his left side. The observer adds, that some such, though none so complete, instances of inverted bowels, are mentioned by Bartholin, in his second century of observations, n. 29; and that Schenke mentions two others, l. 3, obs. 9, de jecore; one found by Gemma, another by Aquapendente.

Account of the two Sorts of the Helmontian Laudanum, communicated to the Editor by the Hon. Robert Boyle; with the Way of Baron F. M. Van Helmont, of preparing his Laudanum.* N° 107, p. 147.

As for the Helmontian laudanum, you may use your own liberty in suspecting the receipts that go about of it. For the name itself seems ambiguous to

* John Baptist van Helmont was born of a good family, at Brussels, in 1577. After finishing his education at Louvain, he travelled into various parts of the Continent, and on his return, settled at Vilvorde, where he dedicated a great portion of his time to the prosecution of chemical experiments, and to the preparation of elixirs and other chemical remedies, by means of which he is said to have performed the most astonishing cures, and to have attained to a degree of celebrity, as a practical physician, almost unexampled. He attacked and refuted the doctrines of Aristotle and Galen, at that time taught and revered in most of the universities on the Continent; and maintained, in their stead, the hypotheses of the ancient pneumatics, with which he intermixed his own paradoxical opinions expressed in a barbarous jargon, invented by himself. He died in 1644, aged 67; although he had asserted in several of his writings, that he had discovered the means of prolonging life, far beyond what is generally believed to be its natural term. In the true spirit of empiricism, he extolled his *Alkahest* as a remedy for all diseases; the origin of which he referred to the exasperation and disturbance of his supposed *Archæus*, an essence or being which he imagined to be distinct from body and mind, &c. &c.

In these days, when a clearer insight into the constitution of living bodies, enables us to form a better estimate of the powers of those agencies which go under the name of remedies, Van Helmont claims our attention rather as a chemist than as a physician. Paracelsus had taken some notice of that aëriform fluid, which is extricated from fermenting liquors, and which he called spiritus sylvestris. He thought, however, that it was similar to atmospheric air. Van Helmont examined it more closely, and found that it was very different from common air, possessing deleterious qualities. To this and other aëriform fluids he gave the name of *gas*, a name adopted by modern chemists. In his complexionum atque mixtionum elementalium Figmentum, and in his Tractatus de Flatibus, he has enumerated various species of gases, insomuch that one of the greatest chemists of the last century confesses, that in reading the works of this author, he has found, with astonishment, an infinite number of facts, which philosophers are accustomed to consider as more modern; and that on the subject of permanently elastic fluids, Van Helmont has mentioned almost every thing with which we are now more thoroughly acquainted. The Elzevir edition of the collected works of this author, is in one vol. 4to. The editions printed at Venice, Lyons, &c. are in folio. A catalogue of his me, who am well informed that there are two sorts of the Helmontian laudanum; the one used by the elder Helmont, the other by his son. The former was, as a great secret, communicated to me by an expert chemist, sent by a German Prince to compliment Johannes Baptista Van Helmont, some of whose manuscripts (one of which perished in the fire of London,) he procured, together with a way of making his laudanum; which, having received from him 14 or 15 years ago, I carefully prepared, and thought my labour so well recompensed by the extraordinary operations it had, not so much in my hands, as those of learned physicians and others, to whom I presented portions of it, that I should have thought the chemist a benefactor to physic, if he would have made public, or permitted me to publish the way of making so successful a medicine. And though the access to my laboratory was so free to ingenious men, who knew such a medicine to be preparing there, that some of them might easily suppose themselves masters of the secret; yet my justice to the communicator, who made a great and deserved benefit of the laudanum, made me take that care to conceal some circumstances, that men may easily be much more confident than sure that they have the right way of making the medicine. Which, because I durst not communicate, meeting two years ago with Baron Van Helmont, son to the famous Johannes Baptista, I obtained from him, by word of mouth, some directions about the laudanum he uses; which, though he confessed, and I soon perceived, to be different from his father's, yet he seemed to think it not inferior, and more easily made. But he having, for a certain reason, imparted to me his process only by word of mouth; lest it should slip out of my memory, I soon after committed it to writing, as the particulars I gathered from him occurred to me; and at the next season caused the medicine to be prepared in my laboratory, where the progress was often watched in my absence by a very learned and industrious London doctor; who having at my request made many trials with it, and some in cases where other laudanums had been found unavailable, both uses it, and commends it more than I could expect from so wary and judicious a man. This medicine being somewhat more cheap and easy to be made than the elder Helmont's, the experience of its efficacy made me desire of the younger a permission to communicate it for the public good, and to prevent those spurious receipts that go about of the Hel-

writings, too numerous for insertion here, is given in the 2d vol. of Haller's Bib. Med. Practica.— His son Francis Mercurius Van Helmont, was not less remarkable for eccentricity than his father. Besides several medical Tracts, he wrote a curious book entitled Alphabeti vere naturalis hebraici delineatio, wherein he attempts to prove that the shape of the organs concerned in speech is correspondent to the figure of the Hebrew letters.

montian laudanum, which request of mine being almost as soon granted as made, I think myself bound both to own his readiness to oblige the public, and to acquaint them with his way of making so considerable a medicine, as I practised it; though if I had received his directions in writing, they might have been more full and methodical. But though I perceived that he sometimes varies his preparations a little, yet that laudanum proving very successful, that was made according to the annexed paper, I think it will not be amiss to keep to that; which I wish could have been published before the season of the quinces were so far advanced.

Laudanum Helmontii Junioris.

Take of opium a quarter of a pound, and of the juice of quinces four pounds at the least;* the opium being cut into very thin slices, and then as it were minced, to reduce it into smaller parts, is to be put into, and well mixed with the liquor, first made luke-warm, and fermented with a moderate heat for eight or ten days, rather more than less; then filter† it, and having infused in it of cinnamon, nutmeg, and cloves, of each an ounce,‡ let them stand three or four days more; if it be a full week, it may be so much the better; then filter§ the liquor once more, having let it boil a walme or two after the spices have been put in; this being done, evaporate away the superfluous water to the consistence of an extract, or to what other consistence you please. Lastly, incorporate very well with it two ounces of the best saffron reduced to fine powder.||

According to the consistence you desire to have your medicine of, you may order it so, as either to make it up into a mass of pills, in which form I have caused it to be given, or keep it in a liquid form; but in this latter case the evaporation must have been made more sparingly, that after the putting in of the saffron,¶ it may not grow too thick. In this form the dose may be from five or six drops to ten, or fewer, according to circumstances; and of the pills a somewhat less quantity is required.

* For near five pounds would perhaps do better.—Original.

† Which circumstance the author often omits, though I do not.—Original.

‡ The author sometimes uses half an ounce more of each spice.—Original.

§ Or strain it well through a canvas bag.—Original.

|| Sometimes the author instead of the powder, makes use of as much extract as can be obtained from that quantity of saffron.—Original.

¶ Or its extract.—Original.

A Discourse denying the Pre-existence of Alcalizate or Fixed Salt in any Subject, before being exposed to the Action of the Fire. To which is added a Confirmation of an Assertion, delivered in Numb. 101, of these Tracts, viz. That Alcalizate, or Fixed Salts, extracted out of the ashes of Vegetables, do not differ from each other: the same likewise affirmed of Volatile Salts and Vinous Spirits. By Dr. Daniel Coxe. N° 107, p. 150.

The alcalizate or fixed salts of plants, extracted out of their ashes after incineration, or out of tartar calcined, do, in my apprehension, neither pre-exist in the vegetables that afforded them, before they were exposed to the action of the fire; nor do they differ considerably, I am certain not sensibly, from each other. The former part of this position may be thus made out.

1. I never yet found, that any vegetable, or indeed animal or mineral substance, did in the least manifest to the taste or by its effects, that it contained any such salt. Many plants and roots lightly bruised, affect the eyes and nose after the manner of volatile salts, and several bite the tongue and strike upon the palate. Some herbs yield a copious volatile salt, immediately after they are pressed by a considerable degree of heat; and many sorts of earths abound with it, so that it is highly probable, they often actually exist in vegetables, in the very same form, wherein they appear to us on distillation from the herbs themselves, or from soot. And that acid salts do really exist in many plants, is displayed by their taste and effects. They may also be obtained without fire or any artificial analysis; as is evident in tartar, and the reputed essential salts of many plants; in verjuice, vinegar, and verdegris, whose acidities may be concreted, and made to appear in a dry form. Now did alcalies exist in the plants before the analysis, especially so copiously as they sometimes appear afterwards, they would certainly betray themselves by some visible sensible property, or other symptom of their presence.

2. Did alcalies pre-exist in plants, probably animals, whose sole food they are, would also abound therewith; whereas, on the contrary, we do not find the least traces of them, either in blood, urine, bones, horns, &c. which all abound with volatile salts; nor in some other parts, excrements and juices, that afford much acidity, which may frequently by coagulation be brought to a saline form or consistence. Nor can it reasonably be pretended, that the ferment of the stomach and other parts, several digestions and repeated circulations, have altered its property, and at length rendered it volatile. For first, alcalies seem to be of a very fixed nature, and are not easily volatilized: and daily experience will evince, that the chyle does not in the least participate, either in taste or any other property, with alcalisate salts. Besides, herbs

taken out of the omasus of ruminating animals, without any further digestion or preparation, yield a volatile salt, when fermented or putrified in the open air, without additament.

3. Most vegetables, whether woods or herbs, if burnt whilst they are green, and with a smothering fire, yield salts which are far enough from alcalisate; being either neutral or acid, or to speak more properly, tartareous; for they almost exactly resemble purified tartar; and by distillation yield the very same substances. Indeed some few herbs, such as satureja, rosemary, &c. which abound with a sprightly volatile oil, if they are well dried, on simple incineration yield an alcalisate salt; so do some dry woods. But that they are produced by the fire and not separated, I shall prove from experiments which I think unquestionable.

4. In the most natural method of analysing plants, which is by fermentation or putrifaction without additaments, or the intervening of a suspicious analyser, we receive oil, acid spirit, and volatile salt copiously; all which did evidently pre-exist. But if the herbs are perfectly or entirely putrified, little or no alcali can be extracted from them; as neither from rotten nor putrified wood; the active salts, by whose combination the alcali is produced, being either expired or evaporated.

Next I am to inquire, how the fire produces this alcali: whether by the changing of one single pre-existing principle; or by enabling any among them to make so notable an alteration on or in the other; or lastly, whether it is effected by the union of two or more active principles, which thereby become different from what they were before the said combination?

I shall not at present trouble you with the reasons, experiments, and observations, which have induced me to reject the former; but briefly suggest those which encourage and dispose me to believe and assent to the latter: so that this is my position: that alcali salts do result from the combination or union of the saline and sulphureous principle. But whether it is the volatile or acid salt, which combines with the oil or sulphur, is now the subject of our inquiry. The ensuing considerations seem to determine in favour of the acids.

First, tartar, which is sensibly acid, and from which volatile salt cannot be separated by any commonly known method, by bare calcination, becomes a strong and perfect alcali. Secondly, nitre, an undoubted acid, with a small proportion of mineral or vegetable sulphur, is converted into a genuine fiery alcali. Thirdly, nitre which is made by the affusion of an acid spirit on an alcali, may be almost totally distilled into an acid spirit, there appearing not the least traces of a volatile salt, and scarcely any of the alcali, out of which it was chiefly produced.

But these are very weak and inconsiderable, compared with arguments, which necessitate me to believe, that it emerges from the union of the volatile salt with the oleaginous or sulphureous principle. For,

1. There seems to be a great contrariety between acids and alcalies. Being mixed, they heat, contend, and mortify each other; whatsoever one dissolves the other precipitates: whereas, were the salt of alcalies of a nature approaching to acids, they would more plainly unite without that violent contention which usually ensues.

2. Alcalies and volatile salts agree in most properties, excepting their different degrees of gravitation. They are both diuretical and de-obstruent; they both dissolve sulphureous bodies; agree in their contrariety to acids, but mix together quietly without noise, heat, ebullition, or impairing each other's virtues, and are easily separable; the same in quantity and quality they were before mixture.

3. Tartareous or essential salts of vegetables cannot become alcalies, until their acidity be driven away; during which operation, the volatile salts and oil uniting, become more ponderous than the acid, which before gravitated more than either of them in their separate state; so that such a degree of fire, as will wholly dissipate the acid spirit, cannot elevate the more ponderous alcali. Not but that, contrary to what is commonly asserted, the most fixed alcali may be sublimed to great height without additaments, by any intense degree of heat: for I have frequently reduced a pound of it to 3 or 4 ounces, and recovered a considerable proportion, which was caught in well contrived vessels, some yards above the crucible, little if at all altered from what it was immediately before it suffered this violence. On this account chiefly it is, that soot yields some small quantity of an alcali, especially that nearest the focus.

4. Alcalies may be divided into oil and volatile salt, by easy and natural methods of procedure. I myself have many ways effected this in part; and a very worthy person, in whom I can perfectly confide, assured me, he has frequently resolved the whole body of alcalies into the two distinct substances of volatile salt and oil, receiving of the latter a small proportion; which is also confirmed by those trials I have made on the same subject.

I could suggest many more arguments and experiments; but these being sufficient, and I think indissoluble, I proceed to confute the pretensions of acid salts to an interest in this new production. First, what concerns tartar, its acidity is driven away in great quantity before it can become alcalisate; and a volatile salt may, to my knowledge, be by divers methods separated from it. Secondly, as to nitre, though that in distillations yields an acid spirit, yet it abounds also in volatile salt, as I could demonstrate from the manner of its generation, and from irrefragable experiments. And besides, perhaps in the operation of the sulphur on the acid salt, supposing it such, there is a comminution of its parts, and thereby that made a volatile salt which was before acid, only magnitude discriminating between them: and that they are often thus produced by each other, I could fully and at large evince.

Having dispatched this, I cannot but take notice, that I am credibly informed, that many persons, of no ordinary repute for their skill in chemistry, and other arts subservient to experimental philosophy, have been pleased to censure, in an unusual measure of severity, an assertion, accidentally dropped from my pen, in a discourse concerning the volatile salts of vegetables, in N° 101 of the Philos. Trans.; which, although circumscribed by a parenthesis, and an alien to the main design and scope of my undertaking, yet was so far from being thereby protected, that it has sustained the brunt of many unkind reproaches, and been represented as a position without foundation in reason or experience. I shall not endeavour, by an elaborate apology, to vindicate myself from that disgrace, whereunto a charge of being inconsiderate, injudicious, or (which is still worse) insincere, must necessarily expose me; but shall nakedly and simply rehearse, without flourishes, digressions or circumlocution, the reasons, observations, and experiments, which induced me to embrace and publish an opinion, so contrary to what has been hitherto generally received: and I shall then appeal unto all unprejudiced, impartial, and intelligent persons, whether the arraigned position be ungrounded and temerarious; or rather, whether the arguments I have produced in its favour, and for its confirmation, do not render it highly probable, and excuse any, who shall give it entertainment, from suspicion of levity or too prompt credulity.

My assertion was, that alcalisate or fixed salts, extracted out of the ashes of vegetables, do not differ from each other; as neither do their vinous spirits; yet with this restriction, if they were highly rectified or purified. And that I may further manifest, I do not distrust my cause, I shall add, nor volatile salts, not only of vegetables, which I did heretofore faintly affirm, but even those yielded by animals or minerals, with the before-mentioned limitation of due purification.

First then, I say, that salts perfectly alcalised, differ not from each other in sensible, nor, so far as I have had opportunity to enquire, in hidden properties. It has been a constant and general persuasion, that many fixed salts do retain some, at least, the specifical properties of those vegetables, out of whose ashes they were extracted. The salt of wormwood and mint are said to be stomachical; that of the greater celandine proper for icterics; those of broom, ashkeys, elder, bean-stalks, &c. diuretical; of rosemary, sage, &c. cephalic; and others, too many now to enumerate, which are thought to be endowed with very different medicinal properties. I am not very forward to question and quarrel with opinions and maxims established by universal consent, and confirmed by the experience of many ages, unless I have sufficient reason to distrust their veracity and validity. In the present case, the persuasion of the ancients, and the position, which I shall endeavour to illustrate, though at the first appearance they seem diametrically opposite, may be easily reconciled. I formerly declared, that most vegetables burned whilst green or moist, and with a smothering fire, yield a kind of neutral salt, which may be called tartareous, and sometimes not improperly essential, many of them retaining the vomitive, purging, sweating, diuretical, opiate, or other general, and perhaps some specifical, properties, wherewith the plants were ennobled which produced them. Now, whether it is some small quantity of the essential oil, which mixed with the saline principle renders it so variously medicinal, the essential oils of plants being manifestly as it were a compendium of the plant, which they do equally exactly resemble in smell, taste, and other qualities: or, whether those virtues are the result of the crisis and mixture of the several principles; certain I am, that, after the oil is evaporated by an intense heat, or the crasis disturbed by avolation of some parts, and new combinations of what remains, farewell all specifical qualities, and consequently all other differences, than what purity and impurity, and several degrees of heat may occasion; some being more white and fiery than others. Now some salts are much more easily deprived of their acid and oily parts, than others and in some, on the contrary, the oil is of so fixed a nature, or rather so closely combined with the other principles, that it must be a very intense heat which can disjoin them, and thereby reduce the salt to the common standard or aggregate of qualities, wherein all alcalies agree.

The industrious Tachenius somewhere pretends to demonstrate, that there is a real difference between the alcalies of different plants; which he would prove by the various effects they have upon a sublimate dissolved in common water. But this is easily resolved by what I before suggested; as also by an easy obvious experiment, which may at any season in any plant be readily proved. Take what wood or plant you please, burn it green; the salt, being extracted out of the ashes, will, according to the different degrees of fire to which it shall successively be exposed, variously influence the mercurial solution; the several præcipitates differing no less from each other, than when made with the salts of different plants.

This is also most evident in tartar, which, the less and more gently it is calcined, the more salt it yields; and on the contrary, a much smaller proportion, if suddenly and with the highest degrees of heat. That which is prepared by the former method, is mild and gentle, its taste approaching somewhat towards that of acids; whereas the other, which has passed through the violence of fire, has not the least affinity therewith, and can almost as little be endured by the tongue as a live coal of actual fire. And there being very many degrees of heat, whereunto the tartar may be successively exposed, according to the said degrees, the manner of applying it, the space of time, and the substances employed in its calcination; the result will be different, and produce different effects: and the very same sort of tartar will oftentimes become sensibly different upon these methods of procedure, and produce most of the appearances mentioned by Tachenius. And sometimes several parcels of tartar, which seem to our taste and eye calcined to the same degree; yet the operations in nice experiments are frequently various. And to me it does not seem so very wonderful, that many concretes do really differ, which to the senses appear simple and uniform; of which many causes may be assigned. A great number and variety of instances might be here introduced, to clear this truth, if it were not already sufficiently known and believed.

But to proceed where I digressed; what I have asserted, is confirmed by the great variety, which is most visible in pot-ashes: some, being highly alcalizate, are fiery hot; others cold, watery, nitrous to the palate, and no less weak in effects than taste; whereof soap-boilers, dyers, and other mechanics are very sensible. All which proceeds from the woods, being, when they are burnt, green or dry; from their abounding with oily, aqueous, or acetous parts; as also from the several degrees of heat employed in their production. Those who make glass, and especially the finer sorts, complain, that they cannot, with the same quantities and proportions of ingredients, always produce the same sort of glass: which they, not without reason, ascribe to the differences in their ashes. This must necessarily often happen according to the above mentioned hypothesis.

An Account of some Books. N° 107, p. 159.

I. Erasmi Bartholini de Naturæ Mirabilibus Questiones Academicæ. Haf. 1674, in 4to.

The subjects of these disquisitions are twelve, viz.

1. The figures of bodies in general.—2. The figure of snow.—3. The pores of bodies.—4. Attraction.—5. The Cartesian physiology.—6. Experiments.—7. Physiological hypotheses.—8. Custom.—9. Nature.—10. The study of the Danish tongue.—11. Judgment and memory.—12. The secrecy of sciences.

II. Thomæ Bartholini de Anatome Practica ex Cadaveribus morbosis adornandâ Consilium. Haf. 1674, in 4to.

Taking it for granted, that it is the least part of a physician's and surgeon's skill, to know the constitution of the parts according to the usual course of nature in sound bodies, and that the main consists in the inspection and consideration of particular subjects dead of remarkable diseases, and thence composing an anatomy useful in practice; the author declares that he used in his work that perished by the fire, the same method in the anatomy of men and women deceased by considerable sicknesses, that he did in his Anatome Reformata, made up of his observations taken from sound corpses. As for example, what is the situation, shape, colour, connection, substance, &c. of the brain, lungs, liver, intestines, and the like, in such as died of the apoplexy, epilepsy, consumption, fevers, dropsy, jaundice, small-pox, cough, &c.; solicitously examining the preternatural constitution of every part, in those and other diseases, in order to the better understanding of the places affected, and the conjunct causes.*

III. La Lettre de Charles Drelincourt à M. Porrée, sur la Methode, pretendue Nouvelle, de tailler la pierre: avec trois autres à Monsieur Vallot, Premier Medicin de sa Majesté. A Leide, 1674, in 12mo.

These letters were written by the learned author, on the occasion of a new lithotomist in France, pretending to cut all sorts and sexes of mankind for the stone in the bladder, how large soever, without any considerable medical preparatives; which, as to men, he would perform by introducing into the anus some fingers of his right hand, well oiled, and thereby finding the stone immediately, and thrusting it into the neck of the bladder, where it is to be held fast by an assistant; and thereupon having withdrawn his right-hand fingers, he would place his patient in a due posture, and then oil some fingers of his left hand, and slide them into the same place, turning the neck of the bladder, together with the stone, towards the small left trochanter, till with his right hand he could draw the skin of the perinæum towards the right femur, where it is to be held with his left thumb, and then with a fit single instrument he would make a semi-lunar incision, and so without any other mystery, as he speaks, draw out the stone, and then apply healing medicines.

As to women, he would perform the operation dirigendo digitos in sinum pudoris, &c.

Of this operation Monsieur Drelincourt observes; first, that it is an invention as old as Celsus himself, 1600 years ago. Secondly, that it cannot be used

* Since Bartholine's time, morbid anatomy has been prosecuted with great diligence by De Haen, Ludwig, Stoll and Walter, in Germany; by Sandifort, in Holland; by Morgagni, in Italy; by Lieutaud, Portal and Pinel, in France; and by the Monroes, the Hunters, Baillie and others, in Great Britain.

upon adult persons, especially when they are very corpulent and fat, and the stone large and closely adhering; since it appears not, how in such subjects the operator can reach the bottom of the bladder. Thirdly, that it is very doubtful, whether the bladder can be thus thrust and turned at pleasure, as he pretends; and that it cannot but exceedingly torture the patient, to make such compressions, as must needs be made, both to thrust down the stone, and to force the whole bladder to descend to the perinæum. And fourthly, it seems to our author very suspicious, that this operator puts his fingers into the fundament, before he places his patient in a due posture to cut him. Fifthly, notice is here taken, that this operator dispatches some in two or three minutes, but others he holds above thirty minutes; and our author can give himself no other reason for it, but that he deceives those, and cuts these. Sixthly, he notes that this pretended artist makes in some but very slight and superficial compressions, and that very few of his patients make water at the wound, even not at the moment of the operation. What other cause can there be, than that those who urine are really cut, and those that do not urine, receive but a mere incision? Seventhly, he observes, that those that are cured of their incision in five or six days, whom he proves to have been deceived, are free from all the ordinary symptoms of this cutting, but remain subject to the same dysuria, and make as thick and fetid urine as before: whereas in others, whom he cuts indeed, the cicatrice is long in forming, and is preceded by divers accidents; but then, indolence, and the exemption from fits of the stone, and the clearness of the urine, do presently follow after the operation. To all which our author adds, that his proofs are more than convincing, when at the end of three days, upon sounding, a stone is found in some, and none in others.

In the second and third letters, our author relates, that a certain person of Normandy, whom the new pretender affirmed upon sounding to have no stone, was cut of a stone of three ounces weight; and that he dying some days after it, and being opened, his bladder was found not only full of very hard callosities, like ganglions about the nerves, but also lumps of a white, grumous, solid and friable matter, like white tartar, which was as it were cemented upon the scirrhous substance of the bladder: besides, there were fastened to the same bladder certain caruncles, resembling the heart of a pullet, large at their basis, and by little and little growing narrower, and ending in a point, of a fresh vermillion colour, which end was loose, whereas their basis stuck close to the bladder, by very many filaments, which as so many roots nourished them, and made them look so fresh in this their soil.

Lastly, He observed store of little strange bodies, that were so interposed between the woof of the fibres of the bladder, that it was thereby exceedingly tumified and scirrhous. He says, he cut it athwart, and found the inner substance of it very full of vessels, of which he sounded many, and found, that though it was hard, yet it was very full of pipes running through it, which made him at first imagine, that as the spermatic and hypogastric channels of a breeding woman grow large in proportion as the embryo is nourished; so these hypogastric veins and arteries of the bladder were all dilated, and widened, to feed these caruncles, which from thence, as their placenta, drew all their nourishment: but when he saw that these tubes did far exceed their ordinary number, he believed that this was from thence, because that each capillary branch, whose smallness commonly hides them from us, was much stretched in this case, to furnish this matrix, (if it may be so called) with more blood than ordinary.

Continuation of Dr. Daniel Coxe's Discourse, begun in N° 107, *touching the Identity of all Volatile Salts, and Vinous Spirits; with two surprizing Experiments concerning Vegetable Salts, perfectly resembling the Shape of the Plants, whence they had been obtained.*

Concerning volatile salts and vinous spirits, there is nothing worthy of notice. One of the two surprising experiments concerning vegetable salts, relates to a lixivium of fern-ashes of a very red colour, the other to a sublimation of volatile alkali from a mixture of equal parts of sal ammoniac and potash. The sublimed salt adhering to the inside of the glass head or receiver, exhibited a pleasing representation of firs, pines and other trees.

More Observations from Mr. Leuenhoeck. N° 108, p. 178.

I took the eye of a cow, and having with a great pin pierced the cornea, found in the aqueous humour some few crystalline globules swimming. The dark-brown colour which I saw in this eye, consisted of dark gray globules.—The crystalline humour, which in hardness almost resembles a nutmeg preserved, I have with a rasor cut asunder, and found it made up of orbicular scaly parts, lying upon one another, which had their beginning out of the centre, and all consisted of crystalline globules.

Having viewed the vitreous humour lying deeper in the eye, I saw many more globules than in the aqueous, which I took out from the top of the eye. The transparent cornea, after I had let it dry for several days, I viewed also, and found it likewise to consist of crystalline globules. In the second tunicle of the eye, there appeared divers very fine glittering colours. It was black, and consisted also of globules, and viewing the single globules, I found them dark.

The third tunicle was exceedingly thin and tender, and having viewed it, I found it also to consist of globules united. I viewed three optic nerves of cows; but I could find no hollowness in them; I only took notice that they were made up of many filamentous particles, of a very soft substance, as if they only consisted of the corpuscles of the brain joined together, the threads were so very soft and loose: they were composed of conjoined globules, and wound about again with particles consisting of other transparent globules. I viewed the sixth pair of nerves, called par vagum, cutting it off about the pipe in the lungs of a cow, and finding it to consist of a very few filamentous particles, composed of globules joined together, which thready parts are very strong, and they lay as wound about with a matter made up of pellucid globules, of which the small threads were composed. I found in them not only one hollowness, but as often as I cut the nerve asunder, the hollowness still continued therein, and I found in some places not only one cavity, but two or three cavities at once; and where the cavity of the nerve was any thing large, it was lined about with films, as if they had been purposely contrived there to keep open those cavities, and to keep them from being compressed by the surrounding parts.

About two leagues from this town is an inland-sea, called Berkelse Sea, whose bottom in many places is very moorish. This water is in winter very clear, but about the beginning or in the midst of summer it grows whitish, and there are then small green clouds permeating it. Passing lately over this sea, at a time when it blew a fresh gale of wind, and observing the water as above described, I took up some of it in a glass vessel, which having viewed the next day, I found moving in it several earthy particles, and some green streaks, spirally ranged; and the whole compass of each of these streaks was about the thickness of a hair: other particles had but the beginning of the said streak; all consisting of small green globules interspersed; among all which there crawled abundance of little animals, some of which were roundish; others were of an oval figure: on these latter I saw two legs near the head, and two little fins on the other end of their body: others were somewhat larger than an oval, and these were very slow in their motion, and few in number. These animalcula had divers colours, some being whitish, others pellucid; others had green and very shining little scales; others again were green in the middle, and before and behind white, others grayish. And the motion of most of them in the water was so swift and so various, upwards, downwards, and round about, that I confess I could not but wonder at it. I judge that some of these little creatures were above a thousand times smaller than the smallest ones, which I have hitherto seen in cheese, wheaten flower, mould, and the like.

An Account of some Books. N° 108, p. 182.

I. Die Africanische Landschaft Fetu beschrieben durch Wilhelm Johan Muller von Harburgh; Gedruckt zu Hamburgh, 1673, in 12mo.

This piece composed and printed in High Dutch, contains divers not inconsiderable observations, made by an author that lived eight years in Fetu; a country situated about the middle of the gold-coast in Guinea, in 5½ deg. northern latitude. Observations, not of sufficient importance however to be reprinted here.

II. The first Book of the Art of Metals, written in Spanish by Alonso Barba, &c. and Englished by the R. H. Edward Earl of Sandwich. London, 1674, in 8vo.

It is here observed—1. That the provinces of the West Indies abound as much in salt as in metals: that the sulphurous liquor naphtha and petroleum will take fire at a great distance from the flame; here confirmed by a sad instance, in which it came to pass, that a labourer being let down into the bottom of a well with a candle (in a lantern) to repair it, the petroleum immediately through the holes of the lantern sucked, (as they speak) the flame to itself, and set fire on the whole well; which instantly discharged itself like a piece of cannon, and blew the poor man into pieces, and took off an arm of a tree that hung over the well.—That though Albertus M. and others think that marcasites contain no metal in them; yet experience has taught the contrary.—That where orpiment is found, it is a certain sign of a mine of gold, whereof also it always contains some little particle.—That there is a water in Peru, near Guancavelica, of which all the cattle that drink it die; and which when put into moulds of any size, and being for a few days exposed to the sun, it becomes a perfect stone, with which they build their houses: and that in the mountain Pacocava, a league from the mines of Verenguela de Pacagues, there are springs, that are whitish, inclining to a yellow, of so petrifying a nature, that as the water runs along, it concretes into hard and weighty stones of different shapes.—That abundance of brimstone in mines is a good sign of their richness: a considerable instance of which, is the rose coloured ore of the famous mountain of St. Isabella of new Potosi, in the rich province of the Lipes, which is almost all plate, and bred among such abundance of brimstone, that the cavities in the rocks are presently all on fire, if a lighted candle touch them.—That the opinion confining the number of metals to seven is very uncertain, since it is very probable, that in the bowels of the earth there be more sorts than we yet know; and that in the mountains of Sudnos in Bohemia there was some years ago found a metal, by them called Bismuto, which is a metal between tin and lead, and yet distinct from them both.

That Carabaya in Peru contains plenty of the finest gold, as fine as the finest gold of Arabia, it being of the fineness of 23 carats and 3 grains; which though formerly it had only been gathered up in fragments, washed off by rains, yet it is now wrought by following the veins of it under ground. That in the circuit of the Charcas there is such abundance of silver-mines, that they alone, if there were no other in the world, were sufficient to fill it with riches.

That there are mines of other metals in the same provinces; as of copper, tin, lead, iron, quicksilver, &c.

There is a second book annexed to the first; on the refining of silver by quicksilver.

III. The Royal Almanack, &c. by N. Stevenson, one of his Majesty's Gunners, London, for 1675, in 12mo.

This full and ingeniously contrived diary, contains first the true places of the sun, moon, and other planets, with their rising and setting. Secondly, the high water at London-bridge, with rules to serve other places after the new theory of tides. Thirdly, the eclipses; as also the tables of the sun's rising, moon's southing, moon's rising and setting. Fourthly, the moon's and the other planets appulses to the fixed stars for the meridian of London, by Mr. John Flamsteed.

A Strange Kind of Bleeding in a Little Child. By Mr. Sam. du Gard. N° 109, p. 193.

A child, about a quarter of a year old, at Littleshal, in Shropshire, was taken with a bleeding at the nose and ears, and in the hinder part of the head, which lasted for three days; at the end of which the nose and ears ceased bleeding; but still blood came, like sweat, from the head. Three days before the death of the child, which happened the sixth day after she began to bleed, the blood came more violently from her head, and streamed out to some distance. She bled also on her shoulders, and at the waist in such quantities, that the linen next her might be wrung it was so wet; and every day required clean linen. She for three days bled also at the toes, at the bend of her arms, at the joints of her fingers of each hand, and at the fingers' ends; and in such measure, that in a quarter of an hour the mother has caught from the droppings of the fingers, near as much as the hollow of her hand could hold. All the time of this bleeding the child never cried very much, but only groaned; though about three weeks before, it had such a violent fit of crying as the mother said she never heard. After the child was dead, there appeared in those places where the blood came, little holes like the prickings of a needle.

A Further Account of the Zirchnitzer Sea. By Dr. Edw. Brown. N° 109, p. 194.

This lake is encompassed with high hills at some little distances; but I saw no snow on them; but on other mountains in the country, as I travelled to and from this lake, I observed snow in June. Upon hills on the side of great lakes, the snow lies not so long as on hills more distant. The holes for the water are generally stony, and not in soft or loose earth; yet in one or two places the earth has been known to sink, and fall in, particularly near a village called sea-dorf. The great holes are the same every year; but possibly part of the water may sometimes find or make new passages. When the water begins to retire, it is seen in these holes for a while, but afterwards it descends lower out of sight. There remains in June no water, at least not any that is considerable for any time, in places more elevated than those holes, most of it draining away towards the holes in the valleys; the rest is either imbibed by the earth, or if any remain in the hilly or rocky part, it is evaporated. The fish are taken at these holes when the water descends. For besides that the water spreads speedily, the Prince of Eckenberg, who is lord of this lake, and the parts about it, will not permit them to be taken at any other time. The holes are of different sizes and shapes. Some perpendicular at the beginning and then oblique; others oblique at first; scarcely two exactly alike. The water ascends so plentifully, that it fills the lake in a short time, especially the valleys; but I cannot determine it to a day: for some years the water rises so plentifully, that it fills all about Niderdorf, and almost to Zirchnitz. The water that spouts seems somewhat clear in the air; but being spread about, it looks as formerly in the lake. The water is not always at the same height, but somewhat differing according to rains, snows, or drought; and they are sensible of its height by the tops of the hills in it, and its spreading towards Zirchnitz; but it alters not very much till it begins to go away. No river enters the latter, but only inconsiderable rivulets on the south and east side; nor has it any known outlet but by the holes. The country is high about the lake. Probably this lake may hold dependence of, and communication with some subterraneous great lake, or magazine of water, belonging to these hilly regions, which when full, and running over, may vent itself with force and plenty into this field, and when scant of water, absorb and drink in the same again; the water of the lake, returning but from whence it came, having no river running out of it whereby to be discharged. It freezes in winter like other lakes; so that the fishes of this lake have a closer habitation than those in others; for they are under the ice a part of the winter, and under the earth a part of the summer. What they call the fisher-stone, is a large stone upon one of the hills, or elevated parts of the field, which whenever it appears above water, the fishermen, being upon the lake, take notice of it, and know thereby, that in a few days the water will retire under ground. For after the filling of the lake in September, the water never decreases so low again, as to let the fisher-stone appear, till it begins to retire under ground.

Observations made upon several Voyages, undertaken to find a way for Sailing about the North to the East-Indies, and for returning the same way from thence hither: together with Instructions given by the Dutch East-India Company for the Discovery of the famous Land of Jesso near Japan. To which is added a Relation of Sailing through the Northern America to the East-Indies. Englished by the Editor out of Dutch, which had been composed by Dirick Rembrantz van Nierop, and printed at Amsterdam 1674, *in 4to.* N° 109, p. 197.

The attempts at a north-east passage to India, have always failed of the object. And as to the remarks and observations contained in this paper, which are chiefly about the land of Jesso, and the adjacent parts, they are rendered totally useless at this time, by the more accurate relations of modern important voyages.

Some Observations concerning a possible Passage to the East-Indies by the Northern America Westward. N° 109, p. 207.

Of this passage, much was at first discoursed, merely by hearsay; as may be seen in the hydrographical discourse of W. Bourne, printed anno 1594. Next one Hessel Gerritse, anno 1612, wrote of this passage thus: since the English, upon the several voyages of W. Barentz, made some attempts for the north-east; the directors of the Dutch East-India Company, for some years last past sent thither one Hudson, who not being able to find a passage to the east, sailed to the west, whence, without effecting any thing, he came into England. Afterwards being sent out again by the English, anno 1610, and having sailed 300 Dutch miles and come to the west of Bakalaos, and wintered there at about 52 deg. north latitude, and desirous to search further, he was, together with the rest of the governors of the ship, set on shore by the seamen, who refused to go further, but came home, having been at sea 10 months, whereas they had been victualled but for eight.

All hopes of the north-west passage have since been completely disappointed, from the researches of the modern voyages.

An Account of some Books. N° 109, p. 209.

I. A Discourse made before the Royal Society, concerning the Use of Duplicate Proportion, in sundry important Particulars; with a New Hypothesis of Elastic or Springy Bodies. By Sir Wm. Petty, Knight,* &c.

* Sir Wm. Petty was a very remarkable instance of a universal genius, being highly eminent as a philosopher, a physician, a linguist, mathematician, mechanist, politician, &c. He was born at Rumsey, in Hampshire, 1623. While a boy, he spent much of his time among the artificers there, by which he acquired a knowledge of almost every trade that he inspected; handling the several tools with a skill like an expert workman. With equal facility he obtained the Latin, Greek, and French languages, as well as a good knowledge in mathematics. He went to study at the university of Caen, in Normandy; and after some stay there, returned to England, where he was preferred in the king's navy. In 1643, when the civil war raged, he retired to the Continent, where he passed some years in France, Flanders, and Holland, prosecuting his studies, especially physic. In 1647, he obtained a patent to teach the art of double writing for 17 years. Adhering to the prevailing party of the nation, he went to Oxford, where he taught anatomy and chemistry, and was created a doctor of physic. He may be accounted, as it were, the father of the R. S. since here he grew into such repute, that at his house were held those philosophical meetings, which preceded and laid the foundation of that learned society. In 1650 he was made professor of anatomy there; and soon after, a member of the college of physicians in London, as also professor of music at Gresham college, London. In 1652 he was appointed physician to the army in Ireland; as also to three lord-lieutenants successively, Lambert, Fleetwood, and Henry Cromwell; by the last of whom he was appointed his secretary, and clerk of the council, and by his interest also, a burgess for Westloo in Cornwall, in Richard Cromwell's parliament, which met in January 1658. After the rebellion was over in Ireland, he was appointed one of the commissioners for dividing the forfeited lands among the army who suppressed it. In Ireland he acquired a large fortune, but not without suspicions and charges of unfair practices in his offices. So that he was impeached in parliament of high crimes and misdemeanors, which in 1659 procured his dismission from his employments. He then retired to Ireland, till the restoration of King Charles the 2d; when he returned to England, where he resigned his professorship at Gresham college, was graciously received by the king, and was appointed one of the commissioners of the court of claims. Also, in 1661, he received the honour of knighthood, with the grant of a new patent, constituting him surveyor-general of Ireland, where he was also chosen a member of the parliament.

On the incorporating of the R. S. Sir Wm. was one of the first members, and of its first council. About this time he invented his double-bottomed ship, to sail against wind and tide, a project which, not completely answering in all respects, was afterwards given up. In 1666, he suffered great losses by the fire of London; but afterwards, by projects of various works and manufactures, he augmented his fortune. On the first meeting of the Phil. Society at Dublin, on the plan of that at London, every thing was submitted to Sir Wm. P.'s direction; and when it was formed into a regular society, in 1684, he was chosen the first president: so that he was in a manner the father both of the society of London and of Dublin. But, a few years after, all his great projects, as well philosophical as political and commercial, were determined by the effects of a gangrene in his foot, occasioned by the swelling of the gout, which put a period to his life, at his house in Piccadilly, Westminster, Dec. 16, 1687, in the 65th year of his age; leaving behind him an immense fortune, amounting to about 15,000l. per annum.

The author of this discourse, much addicted to apply speculations to use and practice, has given in this exercise several instances, wherein the consideration of duplicate and sub-duplicate proportion, or the consideration of sides and their squares, is of use in human affairs. Those instances are—1. In the drawing or driving powers, which force ships or other bodies through the water, with reference to the respective velocities caused thereby.—2. In the shapes or sharpness of bodies, cutting or dividing the water, through which they are driven or drawn, and in the different velocities arising from thence, where the bodies and forces are equal.—3. In the strength of timbers, or other homogeneous materials applied to buildings, to carts, or any other machinery intended for strength: and how by a model to judge the sufficiency of such engine as is represented by it.—4. In the effect of oars upon equal and like vessels, according to their numbers, length, blades, and motions, with or against the stream of smooth or uneven waters.—5. In the motion or travelling of horses, on their several paces, and with different burthens on them.—6. In the strength and velocity of mills and their wheels.—7. In the effects of gunpowder.—8. In the distance at which sounds may be heard.—9. In the distances at which odoriferous matters may be smelt.—10. In the distance at which the objects of sight may be seen.—11. In the time of the returns made by vibrating pendulums.—12. In the lives of men and their duration.—13. In musical and sounding bodies, such as strings and bells.—14. In the effects and motions of fire and burning spirits.—15. In the rising and falling of bodies, but especially of water in pumps, over-shot mills, leaks in ships, the heights of rivers at their head above their fall into the sea.—16. In bellows.—17. In the prices of several commodities, as masts, diamonds, large timber, amber, loadstones, &c.—18. In mill-dams, sea banks, and the bulwarks or walls of fortresses.—19. In the compression of wool and other elastic bodies, and of the air within diving vessels, as also in the effects of screw-presses upon several materials.

Having thus enumerated his several instances, wheren duplicate and sub-duplicate proportion is of great importance, he explains what he understands by every material word he uses in this discourse; such as matter, body, figure, place, motion, quantity, quality, habit, time, proportion, weight, swiftness, force, and elasticity. This done, he makes the applications of the said proportions to each of the respective matters just mentioned. To which he subjoins an appendix on his new theory of elasticity mechanically explained.

The various pursuits in which Sir Wm. Petty was engaged, show that his genius was capable of any thing to which he chose to apply it: and it is very extraordinary, that a man of so active and busy a temper, could find time to write so many things as it appears he did; which are even too numerous to be more particularly noticed in this place.

II. The Second Book of the Art of Metals, wherein is taught the common way of Refining Silver by Quicksilver, with some New Rules added for the better performance of the same: written in Spanish by Alonso Barba, and Englished by the Right Honourable Edward Earl of Sandwich, London, 1674, in 8vo.

The first book of this subject was noticed in the last foregoing Number. This second consists of 24 chapters, most of them of a practical nature; describing the methods employed by the workmen in extracting and refining the metal.

III. Animadversions on the First Part of the Machina Cœlestis, of the deservedly famous astronomer Johannes Hevelius, &c. together with an Explication of some Instruments, made by Robert Hook, P. of Geom. in Gresh. Col. and F. R. S. London, 1674.

This piece consists, as the title intimates, of two main parts. The one represents the author's thoughts of the astronomical organography of the excellent Hevelius, both examining his, and Tycho Brahe's instruments, and undertaking to evince, that if they had made use of telescopical sights, their observations might have been much more exact than they are; forasmuch as, in the author's opinion, an instrument of 3 feet radius with telescopes, will do more than one of 60 feet radius with common sights. The other describes an instrument for taking all sorts of angles and distances in the heavens, which, if increased in bulk, is capable, Mr. Hook says, of as great accuracy, as the atmosphere will ever permit celestial observations to be made. Its perfection he places in these seven particulars: 1. In the sights; which are such, as may be made to discover the minutest part discoverable in an object, not at all straining the eye, and fit for all eyes. 2. In the divisions; which are such, as will distinguish the angle as minutely as the sights will distinguish the parts or objects, and that even to seconds. 3. In such a contrivance of the sights, that with one glance of the eye, both the objects, though a semi-circle distant, are at once distinguished and seen together. 4. In the method of setting it exactly perpendicular to a second if need be. 5. In its fixation and motion; it being so fixed and moved, that if once set to the objects, it continues to move along with them, as long as it is necessary to continue, or be very certain of any observation. 6. In its being to be made and adjusted without difficulty, and not to be put out of order without design; as also in its great easiness of being rectified and again adjusted. 7. In its being not very chargeable.

All these perfections the author explains, and endeavours to make good, by describing and delineating this instrument and all the parts thereof, and endeavouring to obviate such exceptions as he foresaw might be made against it.

To all which he annexes occasionally something that relates to the priority of the invention of the circular pendulum; and likewise a description of a wheel-work, which, he says, may be called the perfection of such work, having the perfectest idea, he thinks, that toothed wheel-work is capable of performing the same effect, as if the wheel and pinion had an indefinite number of teeth. Which done, he describes the frame for keeping the instrument, which is the main argument of this book, in its perpendicularity, and yet always in the azimuth of the celestial object; with a digression of the great use of this principle in dialing, equaling time, clock-work, &c.

He mentions also a mechanical way he has, of calculating and performing arithmetical operations, much quicker and more certainly, than can be done by the help of logarithms.

He concludes the whole by showing many of the particular uses of this new quadrant, as—1. For measuring the refraction of the air. 2. For regulating the places of the fixed stars and of the planets. 3. For stating the latitude of places. 4. For examining the influences of the planets on the earth. 5. For measuring the quantity of a degree upon the earth. 6. For measuring seen distances. 7. For levelling. 8. For taking the diameters of the sun, moon, and other planets. Where, by the by, are mentioned two other instruments, one for taking diameters to seconds; the other for looking on the body of the sun without injury to the eyes.

A Letter of the Learned Franc. Linus, to a Friend in London, animadverting upon Mr. Isaac Newton's Theory of Light and Colours. N° 110, p. 217.

Honoured Sir.—Understanding that things of the nature I now write are always welcome to you, from what hand soever they come, I thought good to mention, that perusing lately the Phil. Trans. to see what I could find therein, in order to a little treatise of optics I have in hand; I lighted, in p. 3075, upon a letter of Mr. Isaac Newton, wherein he speaks of an experiment he tried, by letting the sun beams through a little hole into a dark chamber; which passing through a glass prism to the opposite wall, exhibited there a spectrum of divers colours, but in a form much more long than broad; whereas according to the received laws of refraction, it should rather have appeared in a circular form. Whereupon conceiving a defect in those usual laws of refraction, he frames his new theory of light, giving to several rays several refrangibilities, without respect to their angles of incidence, &c.

Truly Sir, I doubt not of what this learned author here affirms; and have myself sometimes in like circumstances observed a similar difference between the length and breadth of this coloured spectrum; but never found it so when the sky was clear and free from clouds near the sun; but then only appeared this difference of length and breadth, when the sun either shined through a white cloud, or enlightened some such clouds near unto it. And then indeed it was no marvel the said spectrum should be longer than broad; since the cloud or clouds so enlightened, were in order to those colours like to a great sun, making a far greater angle of intersection in the said hole, than the true rays of the sun make; and therefore are able to enlighten the whole length of the prism, and not some small part of it only, as we see enlightened by the true sun beams coming through the same little hole. And this we behold also in the true sun beams when they enlighten the whole prism: for although in a clear heaven, the rays of the sun passing through the said hole, never make a spectrum longer than broad, because they then occupy but a small part of the prism; yet if the hole be so much larger as to enlighten the whole prism, you shall presently see the length of the spectrum much exceed its breadth; which excess will be always so much the greater, as the length of the prism exceeds its breadth. From whence I conclude, that the spectrum, this learned author saw much longer than broad, was not effected by the true sun beams, but by rays proceeding from some bright cloud as is said; and by consequence, that the theory of light grounded on that experiment cannot subsist.

What I have here said needs no other confirmation than mere experience, which any one may quickly try; neither have I only tried the same on this occasion, but near 30 years ago showed the same, together with divers other experiments of light, to that worthy promoter of experimental philosophy, Sir Kenelm Digby, who coming into these parts to take the spa waters, resorted oftentimes to my darkened chamber, to see those various phænomena of light made by divers refractions and reflexions, and took notes upon them; which industry if they also had used, who endeavour to explicate the aforesaid difference between the length and breadth of this coloured spectrum, by the received laws of refraction, would never have taken so impossible a task in hand.

The rest is, honoured sir, that it is far from my intent, that the mistake here mentioned do any way derogate from that learned person: which truly might have happened to myself, if at my first trial thereof, the sun had been in a white cloud, as it seems, it happened to him. Wherefore ceasing further to trouble you, I rest, yours to command, FRANCIS LINUS.

October 6, 1674.

An Answer to this Letter.

SIR,—The letter you thought fit to write by way of animadversion upon Mr.

Newton's new theory of light and colours, grounded upon an experiment of letting the sun-beams through a little hole into a dark chamber, seems to need no other answer but this, that you would be pleased to look upon and consider the scheme in Mr. Newton's 2d answer to P. Pardies in Numb. 85 of the Phil. Trans. and rest assured, that the experiment, as it is represented, was tried in clear days, and the prism placed close to the hole in the window, so that the light had no room to diverge, and the coloured image made not parallel, as in that conjecture, but transverse to the axis of the prism.

Extracts of Two Letters, writtten by Mr. Flamsteed to Mr. Collins; the one of Nov. 25, 1674, *concerning an Instrument to show the moon's true place to a minute or two; as also the Writer's design of correcting the hitherto assigned Motions of the Sun. The other of Dec.* 14, 1674, *on the necessity of making New Solar Numbers, together with an Expedient for making trial, whether the Refractions in Signor Cassini's Tables are just. London, Dec.* 17, 1674. N° 110, p. 219.

The Extract of the first Letter.

After my return to Derby, I perused Mr. Street's discourse, and considered the contrivance of his moon-wiser; which, though he affirms in the conclusion of his little tract, to be different from Mr. Horrox's; yet I can assure him, that for the motion of longitude it is the very same, and no other than what is taken from my explication; save that, where I thought the manner of librating the apogæum was obvious from the calculation, and needed not to be explained, he has shown how to take it in the libratory circle. As for his motion of latitude, indeed that is a little different; but I can again assure him, not much better than Mr. Horrox's, to whom we are indebted for this contrivance. Mean-time, when he has done what he can, it will not show the true place to half a degree.

I have therefore thought of another contrivance, not so large nor trouble-some as his, that shall show the moon's true place to a minute or two, without error, and with an instrument no larger than his, to less than a single minute.

And because I find, by correcting the sun's motions by Signor Cassini's obser-vations, that his inequalities are 5 minutes less in his mean distance; and that the motion of latitude needs correction; I shall amend both, as I think and find fit, in this instrument; which with the place and latitudes will give the moon's semi-diameters and parallaxes to a second.

The Extract of the other Letter.

I think I have been since my last a little better employed than before. I found it necessary to make new solar numbers, because in my old I had neglected to apply refractions in all the altitudes above 30 degrees; wherein yet reason and some little experience has showed me they are not insensible. I found Signor Cassini's observations, which I took from Ricciolus's Astronomia Reformata, much more accurate than Tycho's, and therefore sought out numbers that might answer them. The apogæum I found it necessary to promote 44 minutes; so that anno ineunte 1655, it might be in ♋ 7° 30′ 0″; and to make the greatest equation only 1° 54′ 13″; whereby I found, the phænomena would be answered much more accurately than I expected, and as near, all things considered, as I could desire.

But still I was uncertain whether the refractions in the said Cassini's Tables were just measures or not; and I had no conveniences for making trial. At last I thought on this expedient, which fully satisfied me; viz.

I considered that if some of those observations of the distances of ♀ from the ☉ by day, the stars in the night preceding or following, were skilfully examined, they might show me the true quantity of the equations of the sun's orb, or rather the difference of his mean and equal motion. I turned over his progymnasmata, and pitched on two. The first made anno 1582, the 5th of March, hor. 4. 42′, and hor. 7. 12′ p. m.; whereby I found the ☉ at hor. 4. 42′ was 94° 47′ in antecedence of the lucida calcis ♊. The second made anno 1585, the 15th Sept. hor. 5. 15′, and hor. 6. 55′ mané. Where from applying and considering the refractions in both, I found the sun at hor. 6. 55′ to be 74° 30′ in consequence of the lower head of ♊. The difference of longitude between these two stars is 17° 59′: and therefore now the sun in consequence of the lucida calcis ♊ 92° 29′. So that the sun's apparent motion between the year 1582, the 5th of March at hor. 4. 42′, and the year 1585, the 15th of Sept. at hor. 6. 55′ mané (besides the whole revolutions) was 187° 16′: but the mean motion is 191° 2′; greater than the apparent by 3° 46′: which parted in proportion to the equation of the earth's motion, collected for those times from my new tables, gives the greatest equation of the orb, 1° 54′ 15″; consenting, to my wonder, without any wresting of the observations, with that which I deduced from Cassini's correct meridional altitudes.

I had not had time to examine any of those others he has related; nor indeed are they any ways convenient: but by this that I have done you may see, that if once we get instruments to our purpose, that then it will not be difficult to correct the sun's motions, without the consideration of the meridional altitudes, in which 24 seconds error gives the place above one minute amiss.

At present I use tables for the sun's motion, grounded on this equation, which is less than Tycho's by no less than 9 minutes; which must needs cause great alterations in our numbers for all the other planets; in correcting of which, I shall employ some of those minutes I can spare from my more necessary studies, and have hopes of good success.

Tycho's great equation made him commit no small errors, and put him upon strange shifts to hide and salve them. All his observations of the planets in their oppositions to the sun, are to be corrected, before we may attempt to represent them by numbers: for his errors in the sun's place made him err sometimes 5 or 6 hours in the time of the opposition, which must be reformed.

And that I may perform my discourse of the parallax of Mars observed, I shall fall upon it at my spare hours after Christmas.

Some Observations and Experiments by Mr. Martin Lister. N° 110, p. 221.

I. *Of the Efflorescence of certain Mineral Glebes.*

I keep by me certain large pieces of crude alum-mines, such as they were taken out of the rock. I had also in the same cabinet like pieces of the ordinary fire stone or marcasite of the coal-pits, which here we call brass lumps. In process of time both these glebes shot forth tufts of long and slender fibres or threads; some of them half an inch long, bended and curled like hairs. In both these glebes these tufts were in some measure transparent and crystalline. These tufts did as often repullulate as they were struck and wiped clean off.—Herein these fibres differed in taste; the aluminous very alumy and pleasantly pungent; the vitriolic styptic and odious: again, the alum ones being dissolved in fair water, raised a small ebullition; whereas the vitriolic fibres dissolved quietly. The alum fibres were generally smaller, and more opake, snow-like; the vitriolic larger, many fibres equalling a horse-hair in thickness, and more crystalline. The water wherein the alum-fibres were dissolved, gave no red tincture with gall; not by all the means I could devise to assist them; whatever has, and that with great confidence, been said to the contrary, by some of the writers of our Yorkshire spaws; the vitriolic immediately gave a purple tincture with gall.

Having laid pieces of the same marcasite in a cellar, they were in a few months covered over with green copperas, which was these fibres shot and again dissolved by the moist air, cloddered and run together. Exposing other pieces of the same vitriolic glebe in my window, where the sun came, they were covered over with a white farinaceous matter, that is, with these fibres calcined by the rays of the sun and warm air beating upon them. Of what figure these fibres were, whether round or angular, I could not well discern. But I take these fibrous and thread-like shootings of alum and vitriol to be most genuine and natural; and their angular shootings, after solution, into cubes and rhomboides, to be forced and accidental; salts of very different natures, as well vegetable as fossil, by a like process in crystallizing of them, being observed to shoot into like figures. But this is not my purpose at this time.

II. *Of an Odd Figured Iris. See fig.* 3 *and* 4, *pl.* 7.

I have not observed any rock or sort of stone, whether metalline or more common, which has not its different sort of spar, shot in some part or other of its bed or seams. And these spars differ not only in their colours and other accidents, but eminently too in their figure. To pass by divers which I have collected, I shall describe one of a very curious figure, and which, though very common in our blue lime-stone rocks, out of which plenty of lead ore is gotten, yet is not, that I know of, mentioned by any author. These crystals are mostly of a black water, like the black flint in chalk-hills; but there are of them, which have a purplish or amethystine colour; and some there are as clear as crystal. They adhere to the seams of the rock, whether between bed and bed, or wherever there are cross and oblique veins through the very substance of the bed. The smaller the veins the less the Iris. Some are as small as wheat corns, and others a hundred times larger. They shoot from both sides of the seam, and mutually receive one another. They are figured thus, viz. a column of six planes very unequal as to breadth; the end adhering to the rock is always rugged, as a thing broken off; the other end of the column consists of three quinangular planes, very little raised in the middle: these planes too are very unequal. However they may be straightened and compressed in their shooting, yet the number of planes mentioned both of the column and top is most certain. The places where infinite of them may be had, are Rainsborough Scar on the Ribble; also in a stone-quarry near Eshton Tarne, in Craven.

III. *Glossopetra tricuspis non-serrata. Fig.* 1 *and* 2.

Mr. Ray, in his travels, has these words concerning the Glossopetræ, p. 115. "Of the Glossopetræ, I have not yet heard that there have been any found in England; which I do not a little wonder at, there being sharks frequently taken upon our coasts." I have had out of the isle of Sheppey, in the river of Thames, very sharks teeth dug up there; which could not be said to be petrified; though as to the colour, they were somewhat gilded with a vitriolic tarnish at first receiving them; but they were white, and in a short time came to their natural colour.

In the stone-quarries in Hinderskelf-park, near Malton, I had this stone, (fig. 1) the greatest rarity of this kind I ever met with, and which I took out of the rock there myself. It is a fair glossopetra with 3 points, of a black liver colour and smooth; its edges are not serrated; its basis is, like the true teeth, of a rugged substance; it is carved round the basis with embossed work; it has certain eminent ridges or lines like rays drawn from the basis to each point.

IV. *Of certain Dactyli Idæi, or the true Lapides Judaici, found in England. Fig.* 5.

The stones called Dactyli Idæi, and Lapides Judaici, are brought over to us from beyond seas in divers shapes, and some of them are described in authors. We have plenty of them for kind in these parts, as in the stone-quarries at Newton, near Hemsley, and at Hellingly, by Malton. There is some variety in the figure of them here also; but the most common one in these rocks is after the fashion of a date-stone, round and long, about an inch, and sometimes longer. They are a little swelled in the middle, and narrower towards each end: they are channelled the length way, and on the ridges knotted or purled all over with small knots, set in a quincunx-order. The inward substance is a white opaque spar, and breaks smooth like a flint; not at all hollow in the middle, as are the Belemnites.

V. *Of the Electrical Power of Stones in relation to a Vegetable Rosin.*

Having occasion, in July, to view certain fossils, which I had disposed of into divers drawers in a cabinet, made of Barbadoes cedar, I observed many of the stones to be thickly covered over with a liquid rosin, like Venice turpentine. Examining further, there was not a drawer, wherein there was not some more some fewer stones thus drenched. That this could be no mistake, as from dropping, the bottoms of the drawers are of oak. Again, many stones which were wrapped up in papers, were also wholly infected and covered with this rosin. Besides, after diligent search, there appeared no manner of exudation in any part of the cabinet. Two things I thought very remarkable: 1. That of the many sorts of stones I had there, divers escaped, but not any of the hæmatites kind; having therein manganese, shistus, botryades, &c. which were all deeply concerned. 2. That among perhaps 500 pieces of the astroites, here and there one or two in an apartment, and sometimes more, were seised, and the rest dry;

as it fares with people in the time of the plague in one and the same house. I further observed, that stones of a soft and open grain, as well as those of a hard and polished superficies, were affected alike. It is certain, that the whole body of the turpentine of the cedar-wood was carried forth into the air, and floating therein was again condensed into its own proper form upon these stones. This makes it more than probable, that odoriferous bodies emit and spend their very substance. Thus camphire is said, if not well secured, totally to fly away. Again, it is hence evident, that there is great difference between the distillation of vegetable juices, and the emission of effluviums, or this natural distillation; that really separating and dividing the substance into different parts; but this carrying out the whole intirely and unaltered in its nature.

VI. *Of the Flower and Seed of Mushrooms.*

The general and received opinion of botanists, concerning mushrooms, is that which Caspar Bauhinus, in his Pinax, expresses in these few words, viz. "Fungi neque plantæ, neque radices, neque flores; neque semina sunt; sed nihil aliud quàm terræ, arborum, lignorum putridorum, aliarumque, putrilaginum humiditates superfluæ." I am of opinion, that they are plants of their own kind, and have more than a chance original. We will instance in that species, called Fungus porosus crassus magnus I. B. The texture of the gills is like a paper pricked full of pin-holes. In August this is very frequent under hedges, and in the middle of the moors in many places of this country. It seems to me, and no doubt it will to any person that shall well examine it, that the gills of this mushroom are the very flower and seed of this plant.* When it is ripe, the gills here are easily separable from the rest of the head: each seed is distinct from other, and has its impression in the head of the mushroom, just as the seeds of an artichoke has in the bottom of it. The larger end of the seed is full and round, and they are disposed in a spiral order, just as those of the artichoke. The like we think of all other mushrooms, however differently figured.

And if it shall happen, when sown, that these will not produce their kind, but be steril; it is no strange thing among plants, there being whole genuses of plants that come up, and flower, and seed, and yet their seed was never known to produce plants of their kind, being naturally steril, and a volatile dust, as all the orchides or bee-flowers. It may be further observed concerning this mushroom, that when fresh gathered, it is of a buff colour on both sides; and yet,

* This conjecture respecting the existence of the seeds of the fungi or mushrooms in their gills, has been verified by the late beautiful discoveries of Hedwig.

cut through the middle, it will in a moment change from a pale yellow to a deep purple or blue, and stain linen accordingly. A drop of the juice, leisurely squeezed out, holding it between your eye and the light, will change through all the colours of the rainbow, in the very time of its falling, and fix in a purple, as it does in the springing out of its veins.

VII. *Of the speedy vitrifying of the whole Body of Antimony by Cauk.*

The several vitrifications of antimony are either opake or transparent. To the first kind I shall add one, which is in itself very curious, and has these advantages above the rest, that it is done with great ease and speed; and by it I have performed some things on minerals and metals, which with crude antimony alone I could not effect. Take of antimony one pound; flux it clear: have an ounce or two of the cauk-stone, described below, in a lump red-hot in readiness. Put it into the crucible to the antimony; continue the flux a few minutes: cast it into a clean and not greased mortar, decanting the melted liquor from the cauk. This process gives us above 15 ounces of vitrum of antimony, like polished steel, and as bright as the most refined quicksilver. The cauk seems not to be diminished in its weight, but rather increased; nor will be brought to incorporate with the antimony, though fluxed in a strong blast.

This cauk-stone is a very odd mineral, and I always looked upon it to be much akin to the white milky mineral juices, I formerly sent you a specimen of: and this experiment is demonstrative that I was not mistaken; for the milky juice of the lead mines vitrifies the whole body of antimony in like manner. That this vitrification is from the proper nature of cauk I little doubt; for I could never light upon any one mineral substance, which had any such effect upon antimony; and I have tried very many, as lapis calaminaris, stone sulphur, or sulphur vivum, galactites, sulphur marcasite, alum-glebe, divers spars, &c. Cauk is a ponderous white stone, found in the lead-mines; it will draw a white line like chalk or the galactites: and though it be so free, it is more firm, and has a smooth and shining grain, spar-like, yet not at all transparent.—[Cauk, or cawk, is a sulphate of barytes.]

An Account of some Books. N° 110, p. 226.

I. Tracts containing—1. Suspicions about some Hidden Qualities of the Air, with an Appendix touching Celestial Magnets, and some other Particulars.—2. Animadversions upon Mr. Hobbes's Problemata de Vacuo.—3. A Discourse of the Cause of Attraction by Suction. By the Hon. Robert Boyle, Esq. F. R. S. London, 1674, in 8vo.

In the first of these tracts, the author, passing by those obvious qualities of the air, heat, cold, dryness and moisture, and such others, as are now also commonly known, I mean gravity, elasticity, refractiveness, &c. inquires into, and delivers his conjectures about, some yet more latent ones. And the chief account on which the air may be thought endowed with hidden qualities, he esteems to be those exotic effluvia, that probably proceed partly from beneath the surface of the earth, partly from the celestial bodies. Towards the end of the first part, occasion is taken, in a peculiar discourse, to inquire, whether, as it is thought no impossible thing that nature should make, so it may not be an unpracticable or hopeless thing that men should find, or art should prepare, useful magnets of the exotic effluvia of the lower region of the earth, or the upper of the world?—To this part are subjoined some observations on the growth of metals; also some new experiments about the preservation of bodies in vacuo Boyliano, or with exclusion of the air; tried on bread, milk, cream, cheese, roasted meat, blood, violets, July flowers, roses, strawberries, blackberries, beer. All which, except the milk, cream, and blood, remained good, and without any notable alteration, after a considerable time.—The second tract of this book being written dialogue-wise, examines Mr. Hobbes's arguments for the absolute plenitude of the world, and shows them to be far short of cogency.—The third examines the cause of suction; and having rejected fuga vacui to be the cause of the raising of liquors in suction, and declared also, that he cannot acquiesce in their theory, who refer it to the action of the sucker's thorax: which done, he makes out, by experiments, his thoughts concerning that cause; which thoughts amount to this, that liquors are upon suction raised into pipes or other hollow bodies, when and so far as there is a less pressure on the surface of the liquor in the cavity, than on the surface of the external liquor that surrounds the hollow body; whether that pressure on those parts of the external liquor, that are from time to time impelled up into the orifice of the pipe, proceed from the weight of the atmosphere, or the propagated compression or impulse of some part of the air, or the spring of the air, or some other cause, as the pressure of some other body quite distinct from air.

II. R. P. Claudii Franc. Millet de Chales* è S. J. Cursus seu Mundus Mathematicus, universam Mathesin tribus Tomis complectens. Lug. 1674, in folio.

The first volume of this body of mathematics, comprehends eight books of

* This learned mathematician was born of a noble family at Chambery, 1621. He was of the Society of the Jesuits, and professed mathematics with reputation at Marseilles, Lyons, Paris, and Turin, where he died 1678. The best edition of his course of mathematics, is that of 1690, in 4 volumes. The treatise of navigation is a part highly esteemed.

Euclid; arithmetic; Theodosius's spherics; trigonometry; practical geometry; mechanics; statics; universal geography; a treatise on the magnet; architecture, and carpentry.—In the second are contained these treatises, viz. of the section of stones, of military architecture, of hydrostatics, fountains and rivers, hydraulics, navigation, optics, perspective, catoptrics, and dioptrics.—The third contains his treatises about music, pyrotechny, astrolabes, dialling, astronomy, calendars, astrology, algebra, the method of indivisibles, and conic sections.

III. The Sphere of M. Manilius made an English Poem, with Annotations, and an Astronomical Appendix. By Edward Sherburne,* Esq. Lond. 1675, in folio.

In this ancient and poetical treatise on astronomy, many particulars occur, touching the nature of the heavens and the celestial bodies, that agree with the assertions of some of the most eminent modern astronomers, viz. the fluidity of the heavens, against the Aristotelean solidity of the orbs; the position of the fixed stars, not in the same concave superficies of the heavens, equally distant from the centre of the universe, but at unequal distances in the ethereal region, some higher, some lower, whence the difference of their apparent magnitudes and splendour; the fiery nature and substance of the fixed stars, and in consequence their being endowed with native lustre, and making so many suns like to our sun; and the Galaxy's being an aggregate of numberless small stars.

Of the parts of this poem, their distribution and order, and of the interpreter's endeavours in explaining it, both in his learned notes and considerable appendix, he observes, that the poem begins with a succinct indication of the origin and progress of arts and sciences, particularly of astronomy; of which last, besides what the translator has noted in his marginal illustrations, he has added for the satisfaction of the more curious, a compendious history, continued down to the age of Manilius; with a very instructive catalogue of the most eminent astronomers, from the first parent of all arts, and mankind itself, to this present time. That it is continued on with a description of the mundane system, and of the celestial signs and constellations; the former of which our interpreter has explained according to the various hypotheses, both ancient and modern; the latter he has described by the number of the stars that compose them; their several denominations in most of the learned languages, and as they are distinguished into profane and sacred figures, according to the different uranography of the ancient Ethnics, and some late Christian astronomers.

* This learned writer was born in London, in 1616, and became commissary general of the artillery, in the reign of king Charles the 1st. Continuing true to the royal cause, at the restoration, he received a pension and the honour of knighthood: and he died in 1702. He published translations of Seneca's tragedies, and a collection of poems, besides the Sphere of Manilius, above noticed.

That the third part of this poem contains a description of the celestial circles; for the better understanding of which, besides what is explained in the marginal notes, our author has added a cosmographical astronomical synopsis, for the most part according to Mersenne, and thereto annexed the 12 propositions of Theodosius de Habitationibus, in English. And, as Manilius has touched on the fiery nature of the fixed stars, his interpreter has here made some further and more curious inquiries touching their substance, light, colour, scintillation, number, figure, magnitude, place, and distance from the earth, or rather the sun. In the next place, the planets are enumerated; whose several denominations, by which they were known and distinguished by the ancients, the interpreter has given in his notes; further enlarging about the nature and substance of the sun, his maculæ and faculæ; also of his vertiginous motion, magnitude, and distance; as also of the moon and her spots, adding the selenographic schemes of Hevelius and Grimaldi, with their respective nomenclatures; and exhibiting a brief account of the nature, substance, structure, figure, magnitude and distance of the other planets. And, because this poem concludes with a corollary on fiery meteors and comets, our author has in part explained them also in his notes; more fully discoursing, in the appendix, of their names, kinds, and several species, their matter, place, and efficient causes; and adding in the close a chronological historical table of the most notable comets, that have appeared since the flood to this present.

Nor has our interpreter omitted to inform his reader touching Manilius's life, country, quality, studies, writings, &c. in the doing of which, as well as in composing this whole work, he has given sufficient proof of his more than ordinary acquaintance both with ancient and modern writings.

IV. Avona, or a Transient View of the Benefit of making the Rivers of this Kingdom Navigable; communicated by Letter to a Friend at London; by R. S. London, 1675, in 8vo.

This letter, it seems, was occasioned by observing the situation of the city of Salisbury on the Avon, and the consequence of opening that river to the said city. The author shows the manifold benefits which will redound to the rich and poor, by making our rivers navigable, to promote the wealth, navigation, commerce, and strength of the island; and to advance ingenious arts and useful knowledge, inventions, accommodations, and discoveries.

V. An Essay to facilitate the Education of Youth, by bringing down the Rudiments of Grammar to the Sense of Seeing; which ought to be improved by Syncrisis, fitted to Children's Capacities, for the learning, especially of the English, Latin and Greek Tongues: in Three Parts; an Accidence, a Middle

Grammar, and a Critical or Idiomatical Grammar. By Mr. Lewis of Tottenham, in 8vo. London.

An Account of the Observations in London and Derby, made by Mr. Hook, Mr. Flamsteed, and others, concerning the late Eclipse of the Moon, of Jan. 1, 167$\frac{4}{5}$. N° 111, p. 237.

	Observed at London.		By Flamsteed's Tables.		
	h.	m.	h.	m.	s.
Beginning of the true shadow..	5	22	5	32	58
The immersion..............	6	19	6	32	10
Emersion..................	7	58	8	7	50
End of the true shadow.......	8	58	9	7	2

At Derby, which differs from London in longitude 5 min. Mr. Flamsteed observed the beginning of the entrance of the true shadow 5h. 19m.—The penubra was seen in London to continue near half an hour, before it wholly quitted the body of the moon.

Observed by M. Bulliald at Paris.

						By a Pendulum Clock.			By the Philolaic Tables.		
	°	′	h.	m.	s.	h.	m.	s.	h.	m.	s.
Beginning; the altitude of Capella being	52	26	5	32	29	5	32	50	5	44	27
Immersion; altitude of Capella........	62	8	6	33	3	6	35	46	6	46	24
Emersion; altitude of Capella of Pollux	43	46	8	9	30	8	8	0	8	24	24
End; altitude of Sirius..............	20	47	9	10	0	9	9	40	9	26	21

Account of Iceland, by B. D. Paul Biornon. Translated from the Latin by the Editor. N° 111, p. 238.

The air of Iceland is very healthy all the year round. The diseases the inhabitants are most subject to, are the colic and leprosy. They have no physicians; only two or three surgeons that furnish plaisters for the dressing of wounds. In the air iron rusts very soon.—The changes of the weather are uncertain, nor do they fall out according to the four seasons of the year. Sometimes it snows as well as hails in the middle of summer; and the winds blow now and then very furiously at the same season.—The frost penetrates at most four feet into the earth. Spirit of wine and oil escape freezing; much more quicksilver. Fish are preserved from putrefaction by burying them in the snow. Frozen bodies swell, and are changed both in taste and colour. The figure of the snow is various, as is also its size. Hail is roundish; the largest is only of the size of hail-shot, that we kill fowl with.

Of meteors, I have observed the ignis lambens, the draco volans, and frequently two mock suns, with three rainbows passing through them and the true sun. There are no stated winds.—The depth of our sea is various: the greatest about the coast is 80 fathoms. The sea water in clear nights, being struck with oars, shines like fire bursting out of a furnace. The tides observe the motion of the moon. The sea swells about the moon's rising and setting; and it falls when she is to the south and north. The ordinary highest tides are not above 16 feet, except in autumn, when it is very tempestuous, and then they rise sometimes to 20 feet. About the full and new moon are the highest spring tides, and the lowest neap tides.—There are many lakes, and most of them on high mountains, which are stored with salmon. Innumerable springs gush out of rocks. Also many hot springs; of which, some are so hot, that in a quarter of an hour they will sufficiently boil great pieces of beef; which is done in this manner: they hang the kettles with cold water over them, in which they put the meat to be boiled; for fear of either burning or throwing up the meat by the fervent and vehement ebullition of the hot waters. These waters harden and petrify about the brims of the springs. The highest hills are not above a quarter of a German mile high. There is a whole ridge of mountains through all the island. The people live only in the valleys, and towards the sea shore. There are other volcanoes besides Hecla; and all covered with snow. —The declination of the load-stone is here to the north-west; (but how much he notes not.)—The soil is clayey for the most part; in some places sandy; no where chalky. No tillage is used; all their commodities being imported; of which the chief are, barley, wheat, linen, iron.—They have great numbers of various birds in summer. In winter, ravens, eagles, wild ducks, swans. They are pretty well stored with horses, oxen, cows, sheep, dogs, and in some places with hens. There are foxes in the mountains; and the Greenland ice brings with it those terrible guests, the bears. The oxen and cows live in winter on hay; but the horses and sheep make a shift to live on the grass under the snow, and coralline moss called muscus marinus.—There are no minerals that are known; only store of brimstone, of which they export every year two ships lading.—In the year 1642, on the 13th of May, all the sea which beats on the promontories was for two days so pellucid and shining, that shells and the smallest stones could be seen at the bottom, where the sea was 40 fathoms deep.*

* For more particulars concerning this northern part of Europe the reader is referred to Von Troil's Letters on Iceland, published in 1780. The water of the hot-springs was analysed by the

Divers Rural and Economical Inquiries, recommended to Observation and Trial. N° 111, p. 240.

Of no use at the present time.

Extract of a Letter of Dr. J. Wallis, to M. Hevelius, from Oxford, December 31, 1673; *gratulatory for his Organographia; and particularly concerning Divisions by Diagonals, lately inserted in Mr. Hook's Animadversions on the first Part of the Machina Cœlestis of Hevelius; but so faultily there printed, that it was thought fit, at the Author's desire, to be here done more correctly. Translated from the Latin.* N° 111, p. 243.

After returning thanks to M. Hevelius for the present of his book Organographia, or description of his astronomical apparatus, and some handsome compliments in commendation of the book, the instruments, and of the ingenious astronomical labours of the author, Dr. Wallis then adverts to some certain parts of it, particularly the divisions on instruments by diagonals. Without dwelling on particulars, says Dr. Wallis, I may yet briefly mention one thing, viz. the divisions by diagonal lines, intersecting the concentric circles on the limb of the instrument. You retain that method of dividing, long since used, and very deservedly, and also those concentric circles disposed at equal distances from each other. But although this might occasion certain small, and even larger errors on broad limbs of instruments; yet in your instruments, which are so large, and the limbs so narrow, as you justly observe, this cannot cause any difference at all that can be sensible.

On this occasion I shall here subjoin what formerly, about the year 1650 or 1651, occurred to me on this matter; and which I now find among my memorandums. Namely, if it be required thus to divide the broad limb of a smaller instrument; to determine by trigonometrical calculation, at what distances the concentric circles ought to be drawn, so that the angles may be equal among themselves, which are determined by the transverse intersections with those circles.

The Division of the Arc on the Limb of a Quadrant, or other such like Instrument, by Concentric Circles, and a right Diagonal Line.

Let the breadth of the limb $RL = l$ (fig. 6, pl. 7); the radius of the inner circle $AR = r$; that of the exterior $AZ = AL = l + r = z$; containing the angle $RAZ = A$, to be divided into any number of equal parts, the number of

late Dr. Black of Edinburgh, and among other ingredients was found to hold in solution siliceous earth.

which is n, by the right lines a, b, c, &c. (whose lengths are required) making with the rectilineal diagonal RZ the angles α, β, γ, &c. Then R A a = $\frac{1}{n}$ A, RAb = $\frac{2}{n}$ A, RAC = $\frac{3}{n}$ A, &c. Let ARZ = p, and AZR = q. Having given therefore the legs r, z, with the contained angle A, and thence the sum of the remaining angles $p + q$, the rest will be found, p obtuse, and q acute: for $z + r : z - r ::$ tang. $\frac{p+q}{2}$: tang. $\frac{p-q}{2}$; and $\frac{p+q}{2} + \frac{p-q}{2} = p$. Then, knowing the angles p and $\frac{1}{n}$ A, and thence the remaining angle α, with the included side r; the side a will thence be found; viz. sin. $\alpha : r ::$ sin. $p : a$. And, in like manner, having known p and $\frac{2}{n}$ A; p and $\frac{3}{n}$ A; p and $\frac{4}{n}$ A; &c.,
there will be had b; c; d; &c.

For example—Let $r = 1$; $l = 0.2$; $z = 1.2$; A = 10′. Therefore $p + q$ = 179° 50′; and $\frac{p+q}{2}$ = 89° 55′. Then, as $z + r = 2.2 : z - r = 0.2 ::$ tang. $\frac{p+q}{2}$ = 687.5488693 : 62.5044427 = tang. $\frac{p-q}{2}$, to which answers the angle 89° 5′ 0″ 17‴ nearly. Therefore $\frac{p+q}{2} + \frac{p-q}{2} = p$ = 179° 0′ 0″ 17‴ nearly; the sine of which, or of 0° 59′ 59″ 43‴, is 0.0174511. Then, cutting the angle A into 10 parts, each of them of 1′, there will then be found a, b, c, d, e, f, g, h, i; thus, as

					Diffs.
				1.00000 = r	
Sin. α (0° 58′ 59″ 43‴)	0.0171603 : r = 1 ::	Sin. p =	0.0174511 :	1.01694 = a	1694
Sin. β (0 57 59 43)	0.0168694 : r = 1 ::	Sin. p =	0.0174511 :	1.03448 = b	1754
Sin. γ (0 56 59 43)	0.0165780 : r = 1 ::	Sin. p =	0.0174511 :	1.05264 = c	1816
Sin. δ (0 55 59 43)	0.0162877 : &c.		&c.	1.07144 = d	1880
Sin. ϵ (0 54 59 43)	0.0159969 :			1.09091 = e	1947
Sin. ζ (0 53 59 43)	0.0157060 :			1.11110 = f	2019
Sin. η (0 52 59 43)	0.0154152 :			1.13206 = g	2096
Sin. θ (0 51 59 43)	0.0151243 :			1.15383 = h	1177
Sin. ι (0 50 59 43)	0.0148335 :			1.17647 = i	2264
				1.20000 = z	2353

The Calculation otherwise.

Let $r = 1$; $l = 0.1$; $z = 1.1$; A = 10′. Therefore $p + q$ = 179° 50′, and $\frac{p+q}{2}$ = 89° 55′, whose tangent is 687.5488693. Then, as 2.1 : 0.1 :: 687.5488698 : 32.7404223½ = tang. 88° 15′ 1″ 57‴¼ = tang. $\frac{p-q}{2}$. Therefore $\frac{p+q}{2} + \frac{p-q}{2} = p$ = 178° 10′ 1″ 57‴¼; the supplement of which is 1° 49′ 58″ 2‴¾, the sine of which is 0.0319827. Therefore,

					Diffs.	
				1.00000 = r		
Sin. α	(1° 48′ 58″ 2‴$\frac{3}{4}$ = 316920)	319827 (		1.00918 = a	918	
Sin. β	(1 47 58 2 $\frac{3}{4}$ = 314013)	319827 (		1.01852 = b	934	16
Sin. γ	(1 46 58 2 $\frac{3}{4}$ = 311103)	319827 (		1.02803 = c	951	17
Sin. δ	(1 45 58 2 $\frac{3}{4}$ = 308198)	319827 (		1.03773 = d	970	19
Sin. ϵ	(1 44 58 2 $\frac{3}{4}$ = 305290)	319827 (		1.04762 = e	989	19
&c.	302343)	&c. (		1.05769 = f	1007	18
	299475)	(		1.06796 = g	1027	20
	296567)	(		1.07843 = h	1047	20
	293660)	(		1.08911 = i	1068	21
	290752)	(		1.10000 = k	1089	21

So far my memorandum. Where we may observe two cases; viz. when the breadth of the limb is made the 5th part, and the 10th part, of the shorter radius; and the angle to be divided 10′; so far accurate according as the common trigonometrical canon is so; and indeed the last figure ambiguous, sometimes in excess, sometimes in defect. As to the radius, I make it 1, as commonly I do; not 10000000, as most others do; by which all the multiplications and divisions by radius are saved. Therefore the sines will be in decimal parts; to which therefore, when necessary, I prefix ciphers, which also occupy the unit's place of integers. In like manner are we to proceed, mutatis mutandis, when the limb of the instrument is taken in any other proportion to the length of the radius.

But it will be more convenient, in order to avoid so often finding the proportional part, that the angle p be of some convenient magnitude, to be determined by exact minutes, without the annexed seconds or thirds; and so finding the length of the greatest radius z, in the same manner as the others a, b, c, &c.

Suppose that, as before, there be taken $r = 1$, and the angle A = 10′, and the angle p be taken, not 178° 10′ 1″ 57‴$\frac{1}{4}$ as before found, but rather 178° 10′, whose supplement is 1° 50′, the tabular sine of which is 0.0319922; and the same of the α, β, γ, δ, &c. The sines will be had like as before, but by one division only; and the length of that radius z being, not precisely 3.1 as before, but nearly so, as 1.09996. Then,

				Diffs.	
			1.00000 = r		
Sin. α = 1° 49′	= 317015)	319922 (	1.00917 = a	917	
Sin. β = 1 48	= 314108)	319922 (	1.01851 = b	934	17
Sin. γ = 1 47	= 311200)	319922 (	1.02803 = c	952	18
&c.	308293)	319922 (	1.03772 = d	969	17
	305385)	&c. (	1.04760 = e	988	19
	302478)	(	1.05767 = f	1007	19
	299570)	(	1.06794 = g	1027	20
	296662)	(	1.07841 = h	1047	20
	293755)	(	1.08908 = i	1067	20
	290847)	(	1.09996 = k = z.	1088	21

In like manner every thing else succeeds, if, assuming the radii r, l, with the angle A, we compute q and the intermediate radii; or, assuming the radius l, with the angles A, q, and compute r and the intermediate radii. But, if the breadth of the limb be only the 30th, or 40th, or smaller part of the radius; and the angle to be divided be not 10′, but only 10″, or even less; the differences will be smaller than can be shown by the common trigonometrical canon, and become quite insensible; and the concentric circles will be equidistant from one another, as far as can be distinguished by the senses: because the 1000dth part of an inch, and still more the 10 thousandth or the 100 thousandth, is a difference less than can be distinguished by sense.

An Account of some Books. N° 111, p. 246.

I. Some Physico-Theological Considerations about the Possibility of the Resurrection; by the Hon. Robert Boyle, F.R.S. London, 167$\frac{4}{5}$, in 8vo.

The noble author's design in this discourse is to show, that the philosophical difficulties, urged against the possibility of the resurrection, are not so insuperable, as they are by some pretended, and by others granted to be; and hopes he has handled this subject in such a manner, as to make it appear, that sound philosophy may furnish us with good weapons for the defence of our faith, and that corpuscularian principles may not only be admitted without epicurean errors, but be employed against them.

II. Waare Oeffening der Planten, door Abraham Munting,* M.D. and Prof. Botanices Groningen. Printed at Amsterd. 1672, in 4to.

In this piece, the author makes it his business to describe, from his own observation and search, the nature, culture, preservation and propagation of trees, shrubs, herbs, and flowers. Of trees thus described, there are 65; of shrubs 64; of herbs and flowers 449: in all 578. Of which there being many that are exotic to Europe, the art and way of ordering them in these parts, is, among the rest, here delivered.

Speaking of the setting of kernels and sowing of seeds, he gives this advertisement, that the kernels and seeds of such trees and plants as bear their fruit above ground, must be set or sown in the decrease of the moon; but of such as bear their fruit under ground, as turnips, parsnips, carrots, &c. must be committed to the ground in the increase of the moon: to which he adds, if the contrary be practised, it will be found, that those trees and plants will indeed bear many branches and large leaves, but little, and that very small fruit.

To obtain extraordinary good, large, and beautiful apple fruit, he advises, by

* Abraham Munting, a learned botanist, was the author of various works, of which the chief is entitled *Phytographia Curiosa.* He died in the year 1683.

all means, to graft good grafts upon such apple stocks as are produced from the seed, and have been deprived of their heart-root, which shoots downwards.

III. The Prevention of Poverty. Showing the Causes of the Decay of Trade, Fall of Lands, and Want of Money: with Expedients for remedying the same, and bringing the Kingdom to an eminent degree of Riches and Prosperity. By Rich. Haynes, London, 1674, in 8vo.

A more particular Account of the last Eclipse of the Moon, as promised in our last Number. From the French Journal des Sçavans. N° 112, p. 257.

January 11, N. S. 1675, about 5 o'clock, 12 min. in the evening, in the Royal Observatory, M. Cassini, M. Picard, and M. Roemer, began to perceive that the eastern part of the moon gradually lost its light; so that at 5h. 25m. they saw a manifest penumbra; then at 5h. 32m. 50s. the limb over against the spot called Hevelius grew so dark, that they all agreed that this was the true beginning of the eclipse. They saw yet the little spot Riccioli, which disappeared not till 15 min. after; and so the shadow advanced from spot to spot unto the other opposite limb of the moon, according to the regular order; the most remarkable times and circumstances being as below; viz. Beginning of the eclipse at 5 h. 32m. 50s.; the total immersion 6h. 35m. 46s; beginning of the emersion 8h. 8m. 0s.; end of the eclipse 9h. 9m. 40s. The diameter of the moon, measured just before the eclipse, was 33′ 15″. The times were by large pendulum watches, which were adjusted by the sun the same day, and verified the day after; besides, before the eclipse began, at 4h. 45m. 1s. by the watches, the star Capella was 45° high towards the east.

Account of the Measure of the Earth's Meridian. By M. Picard. N° 111, p. 261.

The sum of the whole of this account amounts to this: that M. Picard has found 57060 toises for one degree, that is, 28½ leagues and 60 toises; which being multiplied by 360, the number of the degrees, makes 10270 leagues and 1600 toises, reckoning 2000 toises to a league, or 2400 paces, 5 feet to a pace. The method employed by him was, to measure on a plain and straight ground, a space of 5663 toises or fathoms, to serve for the first basis to divers triangles, by which he has concluded the length of a meridian line to be equivalent to a degree. What is remarkable in this is, that no one ever measured so long a basis; the greatest of the former observations having been but of 1000 toises; and that here have been employed, for taking the angles of position, very accurate instruments, and telescopical sights, instead of common ones.

M. Picard observes, that this problem, concerning the just dimensions of the circumference of the earth, is no new thing; but has been the inquiry of several ages, in which princes have been curious, and learned men encouraged to the search and clearing of this difficulty. That for this purpose, Almamon, an Arabian prince, ordered experiments to be made in the plains of Sanjar; where a station being chosen, thence two troops of horsemen set out, and proceeded opposite ways, in a straight line, till one of them had raised a degree of latitude, and the other had depressed it; at the end of their marches, they who raised it, counted 56$\frac{2}{3}$ miles, and they who depressed it reckoned just 56 miles. From this observation can be known but very little, because we know not of what length these miles were. He further observes, that the ancient computations of miles for a degree ran always on the decrease; thus, Aristotle counting to a degree 1111 stadia: after him Eratosthenes counted but 700; Possidonius only 666; Ptolemy but 500. Besides, the precise length of these stadia is unknown, and that they were also different among themselves; the stadia of Alexandria differing from those of Greece. At last Fernelius brought it to 56,746 toises or fathoms of Paris, each of which is equal to 6 Parisian feet; Snellius to 55,021. The method of this last M. Picard judges to be the most ingenious; which was by a scale or series of triangles. But in one thing he esteems it deficient, which is, that Snellius took his objects only by common sights, which do not so distinctly point them out.

M. Picard, in his measuring of a degree, chose a meridian, out of which he intended to take his measure, between Sourdon in Picardy, and Malvoisin, on the confines of Gastinois and Hurepois; and he actually measured a way that lay very straight, between Villejuifve and Ivoisy, viz. the line AB, (fig. 7, pl. 7); and he found the distance between these two terms, in going forward, to be 5662 toises and 5 feet, and in coming back 5663 toises and 1 foot; which being measured with great exactness, he stated the distance between these two places, in round numbers at 5663 toises. The intruments he measured with, were pikes joined together at their ends by a screw, which measure was 4 toises long: This he applied along a cord stretched horizontally, and at the end of every such pike placed a stake, of which stakes he had 10 in all. This distance of 5663 toises was the base of the first triangle, on which the measure of all the depending series was formed. The instrument for taking the angles was a quadrant of 38 inches radius, furnished with telescopic sights. He takes occasion, by the bye, to speak of measures in general; and says, that a pendulum vibrating a second of time, computed according to the mean motion of the sun, is 36 inches and 8$\frac{1}{2}$ lines of the Paris measure. And he thinks that this measure may probably serve in all countries, because the same length of a pendulum served for a second both at the Hague and at Paris; whence he conjectures, the same may serve also in other latitudes. Hence he infers, that if it were desired to constitute a universal measure, which might be common to all countries, it might be thus made, viz. Call this pendulum for seconds, of 36 inches and 8 $\frac{1}{2}$ lines, the astronomical radius; the $\frac{1}{3}$ of this radius the universal foot; the double of which radius might be called the universal toise or fathom, which would be to the Parisian toise as 881 to 864; the quadruple might be called the universal perch, which is equal to the length of a pendulum for two seconds. And the universal mile might contain 1000 of these perches.

M. Picard describes the manner of taking the distance between Sourdon and Malvoisin, with the triangles, and the stations from whence he observed his angles. This distance is 68,343 toises and 2 feet; and the measures of the triangles, 13 in number, were as follows.

In the first triangle ABC, (fig. 7), to find the sides AC, BC.

	Angle			Toises	Feet
Angles	CAB = 54° 4′ 35″	Measured side	AB	5663	0
	ABC = 95 6 55	Hence is found	AC	11012	5
	ACB = 30 48 30	And	BC	8954	0

In the 2d triangle, ADC, to find DC and AD.

Angles	DAC = 77° 25′ 50″	Given	AC	11012	5
	ADC = 55 0 10	Hence	DC	13121	3
	ACD = 47 34 0	And	AD	9922	2

In the 3d triangle, DEC, to find DE, CE.

Angles	DEC = 74° 9′ 30″	The side	DC	13121	3
	DCE = 40 34 0	Hence	DE	8870	3
	CDE = 65 16 30	And	CE	12389	3

In the 4th triangle, DCF, to find DF.

Angles	DCF = 113° 47′ 40″	The side	DC	13121	3
	DFC = 33 40 0	Hence	DF	21658	0
	FDC = 32 32 20				

In the 5th triangle, DFG, to find DG, FG.

Angles	DFG = 92° 5′ 20″	The side	DF	21658	0
	DGF = 57 34 0	Hence	DG	25643	0
	GDF = 30 20 40	And	FG	12963	3

In the 6th triangle, GDE, to find GE.

The Angle GDE = 128° 9′ 30″	The sides	DG	25643	0
	And	DE	8870	3
	Hence	GE	318970	0

So then, the line of distance between Malvoisin and Sourdon being divided into three parts, viz. EG, GI, IN, the part EG is already found, as above.

In the 7th triangle FGH, to find GH.

Angles	FGH = 39° 51 0″	The side	FG	12963	3
	FHG = 91 46 20	Hence	GH	9695	0
	HFG = 48 22 30				

In the 8th triangle GHI, to find GI, IH.

Angles					Toises	Feet
GHI =	55° 58′ 0″			The side GH	9695	0
GIH =	27 14 0			Hence GI	17597	0
IGH =	96 48 0			And HI*	21037	0

Thus the 2d part of the three, viz. GI, is found.

In the 9th triangle HIK, to find IK.

Angles				
HIK =	65° 46′ 0″	The side HI*	21043	0
HKI =	80 59 40	Hence IK*	11678	0
KHI =	33 14 20			

In the 10th triangle IKL, to find KL, IL.

Angles				
LIK =	58° 31′ 50″	The side IK*	11683	0
IKL =	58 31 0	Hence KL	11188	2
		And IL	11186	4

In the 11th triangle KLM, to find LM.

Angles				
LKM =	28° 52′ 30″	The side KL	11188	2
KML =	63 31 0	Hence LM	6036	2

In the 12th triangle LMN, to find LN.

Angles				
LMN =	60° 38′ 0″	The side LM	6036	2
MNL =	29 28 20	Hence LN	10691	0

In the 13th triangle ILN, to find NI.

The sum of the angles ILK, KLM, MLN,			
taken from 360, there remains	The sides LN	10691	0
Angle ILN = 119° 32′ 40″	And IL	11186	4
	Hence IN	18905	0

Thus, the line of distance, EN being as before said, divided into three unequal parts, EG, GI, IN, the measures of all three are found by this series of triangles; viz. EG = 31897, GI = 17557, IN = 18905. These added together, make the length of EN, which is the line of distance between Malvoisin and Sourdon, viz. 68359 toises.

Now to continue this measure from Sourdon to Amiens, in order to verify Fernelius's account; in fig. 8, R is the steeple of St. Peter's, in Montdidier; T, a tree on the hill of Mareuil; V, the lantern of Notre Dame, of Amiens. To find the distance NV, observe NLM, the last triangle of fig. 7, and see how it is disposed in fig. 9; where, in the triangle LMR,

Angles				
LMR =	58° 21′ 50″	The side LM	6037	0
MRL =	68 52 30	Hence LR	5510	3

In the triangle NRL;

Angles				
NRL =	115° 1′ 30″	The side LR	5510	3
RNL =	27 50 30	Hence NR	7122	2

* These two lines, HI and IK, are corrected from another calculation of them, in Picard's book.

Then in the triangle NRT, fig. 8;

		Toises	Feet
Angles { NTR = 72° 25′ 40″	The side NR	7122	2
TNR = 67 21 40	Hence NT	4822	4

Finally, in the triangle NTV;

Angles { NTV = 83° 58′ 40″	The side NT	4822	4
TNV = 70 34 30	Hence NV	11161	4

Now adding the distance between Malvoisin and Sourdon, viz.	68359	0
To the distance between Sourdon and Amiens	11161	4
The whole will be the distance between Malvoisin and Amiens	79520	4

Having thus measured the particular distances between Malvoisin, Mareuil, Sourdon, and Amiens, he proceeds to examine the position of each of these lines of distance in respect of the meridian, or to deduce the length of the meridian intercepted between the parallels of Malvoisin and Amiens, which was thus done:—In Sept. 1669, from the top of the hill Mareuil, marked with G in fig. 7, from whence may be seen Clermont on one side, at I, and Malvoisin on the other side, at E, he took the meridian, and with a quadrant the angles of declination from this meridian; and he found—

The angle EGε in fig. 7, the dec. of EG from the merid. westw.	0°	26′	0″
The angle GIθ, the declin. of GI from the meridian eastward	1	9	0
The angle INV, the declin. of IN from the meridian eastward	2	9	10
The angle VNβ, in fig. 8, the dec. of NV from the merid. westw.	18	55	0

So that in all these 4 triangles, EGε, GIθ, INV, VNβ, there are two angles known, (for the angles at ε, at θ, at V, at β, are right,) and a side, viz. EG, GI, IN, NV; whence he concludes,

The length of the meridian Gε to be	31894 to	1.0ft.
of the meridian Iθ	17560	3
of the meridian NV	18893	3
of the meridian Nβ	10559	3
And hence the length of the whole meridian α β between the parallels of Malvoisin and Amiens to be	78907	3

Though these lines, which make up the meridian, are not in strictness a curve, but in reality the side of a polygon circumscribed about the circumference of the earth; yet the difference between those lines and a true curve is only 3 feet for a degree, which he observes is scarce worthy of notice, as he afterwards proves.

The length of the meridian between Malvoisin and Amiens being thus stated, his next business is, to enquire what answers to it in the heavens, comparing those meridian distances already measured, with minutes and seconds there. These were taken by an instrument, whose limb was an arch of $\frac{1}{20}$ of a circle, of 10 feet radius. The star in the knee of Cassiopeia was that he pitched on, from whence to measure the minutes and seconds of a degree in the heavens.

He then assigns how many toises or fathoms, Parisian measure, answer to a degree of the circumference of the earth; as for instance, the difference of latitude between Malvoisin and Sourdon is found, by observations made in the heavens, to be .... 1° 11′ 57″
And between Malvoisin and Amiens 1 22 55

Now the meridian distance between Malvoisin and Sourdon, calculated from measures taken on the earth, as found above, was 60430½ toises; so hence it is concluded, that 57064½ toises, or, in a round number, 57060 toises, are equal to a degree. Hence the whole circumference of the earth, or 360 degrees, comes out 20541600 toises, and its diameter 6538594 toises.*

* Since this measurement of meridional degrees, by Picard, many others have been made, in different latitudes and countries, and with continual improvements and accuracy, both in the instruments and the observers; as in America, north and south, Lapland, Germany, Italy, France, and lastly in England, by Major Mudge, of the royal artillery, with an accuracy, both of instruments and labour, perhaps never before attempted: Of which we may hereafter have occasion to give a detailed historical account in another part of these abridgements. At present we shall only advert to the probable cause of a newly observed irregularity in the measures of the degrees.

In all the measures of degrees, in different latitudes, when compared with each other, irregularities have occurred, the lengths of any of them appearing to be either too great or too little, in respect of the others, and that by differences which have no uniformity or harmony among themselves. But in the last measurement abovementioned, I am told there is an aberration in the conclusions which runs in a pretty regular and uniform series, which will probably appear in a paper of Major Mudge, which I have not yet seen, to be in the next volume of the Philosophical Transactions, for this year 1803. Now those irregularities have usually and chiefly been ascribed to errors in the terrestrial measures. But it is our opinion that the deviations principally arise from the celestial observations, viz. the observed latitudes, resulting from the observed zenith distances of certain stars. These zenith distances are probably all or most of them erroneous, in consequence of the deviations of the plumb line of the zenith sector, produced by the unequal attractions, on the plummet, of the inequalities in the adjacent parts of the earth's surface, sometimes in excess from hills, and sometimes in defect from valleys and seas. And this cause will very well account, not only for the usual irregularities, but particularly for that uniform deviation in Major Mudge's degrees, which are of this nature, that in going from south to north the terrestrial lengths of those degrees become successively shorter and shorter, from beginning to end, instead of measuring longer and longer, as they ought to do, from the oblate spheroidal figure of the earth. Now this aberration appears to be exactly what might be expected from the position of the part of the meridian here measured, which consists of almost 3 degrees, extending from Dunnose at the Isle of Wight, to the north-east corner of Yorkshire, near the mouth of the river Tees. Now by casting an eye on the map of England, or of Europe, we perceive the English channel on the south end of the line, and the still farther extended northern sea at the north end. And these hollows will naturally occasion a defect of attraction on the plummet, the one on the south and the other on the north, according as it is near the one or the other of these depressions. Hence then, at Dunnose, or the south end of that meridian line, the plummet, or the lower end of the apparent vertical line, will be drawn toward the north, while at the north end of the meridian line it will have a deviation to the south, and that in a more considerable degree than the former, on account of the more extensive depression of the northern ocean. In consequence the zenith points of the plumb line will deviate the opposite way, viz. at the southern station the apparent zenith will be too far to the south, while at the northern station it must be directed too far to the north. Hence it must happen that the celestial difference of latitude between these two stations, being the distance between those two apparent zeniths, will be greater than the true or terrestrial difference, by the sum of the said two deviations. From which it follows that, between those two stations, the celestial arcs appearing to be too large, the observed or celestial degrees will change faster than the terrestrial or measured degrees, or will have measures less than the truth, and that always more and more in defect, in receding from the south, and approaching to the north, on account of the greater defect of matter at this latter. Thus then we have a pro-

Of very exact Portable Watches; from the Journal des Sçavans. By M. Huygens. N° 112, p. 272.

The watches of this invention being made small, will serve for very exact pocket-watches; and when made larger will be useful every where else, and particularly to find the longitudes both by sea and land, as their movement is regulated by a principle of equality, as that of pendulums is in a cycloid, and that no kind of carriage shall be able to stop them. The secret of the invention consists in a spiral spring, fastened by its innermost end to the axis or arbor of a poised balance, larger and heavier than is usual, which turns on its pivots; and by its other end to a piece that is fastened to the watch-plate; which spring, when the balance-wheel is once set agoing, alternately shuts and opens its spires, and with the small assistance it has from the watch-wheels, keeps up the motion of the balance-wheel; so that, though it turn more or less, the times of its reciprocations are always equal to one another.

In fig. 1, pl. 8, the upper plate of the watch is AB; the circular balance-wheel CD, of which the arbor is EF; the spring turned spirally, GHM, fastened to the arbor of the balance-wheel at M, and to the piece that is fixed to the watch-plate at G; all the spires or windings of the spring being free, without touching any thing; NOPQ is the cock, in which one of the pivots of the balance-wheel turns; RS is one of the indented wheels of the watch, having a balancing motion, which the balance-wheel of rencontre gives to it. And this wheel RS catches in the pinion T, which holds on the arbor of the balance, whose motion by this means is preserved as much as is necessary.

Extract of a Letter dated Amsterdam, Oct. 9, 1674, to the Editor, by Dr. Swammerdam, of an unusual Rupture of the Mesentery. N° 112, p. 273.

The accompanying figure (fig. 20, pl. 8) represents a fatal volvulus or iliac passion, caused by a rupture of the mesentery, and its tight convolution round the intestines.—A. A. The intestinum ileum distended in a surprising manner by chyle, air, and fæces, and in an inflamed state.—B. B. The ruptured mesentery, forming a sort of ligature, twisted round the gut so tightly as to occasion death. —C. C. The ligature just mentioned, formed of the ruptured mesentery, and twisted round the gut like the tendril of a vine.—D. D. The aforesaid ligature separately represented, together with its capreolus or tendril, composed of two

bable cause of the inverted order in the measures of the degrees. Hence also most other measured meridians will be erroneous, especially in the parts near seas, or near large mountains. And that insular situations must be worst of any, having the plumb line deviating to the north at the south end of the land, to the south at the north end, to the east at the west side, and to the west at the east side; thus producing errors in all observed latitudes and longitudes. But suffice it, at present, just to give the hint of a probable cause of such errors and aberrations.

circumvolutions.—E. E. The volvulus itself, being a part of the intestinum ileum, tied tightly by the ligature, and approaching to a state of sphacelation; whence a complete obstruction of the passage by stool was produced, so that the contents of the small intestines were voided upwards by an almost incessant vomiting.—F. Part of the intestinum ileum so constricted by the aforesaid preternatural ligature, as to have the appearance of a blind gut.—G. The extremity of the ileum where it becomes colon.—H. The colon a little contracted, but [in other respects] of a natural appearance.—I. The cœcum.

On Astroites or Star-stones. By Mr. Martin Lister. N° 112, p. 274.

These astroites or star-stones, were brought me from the Yorkshire Wolds. I have seen them dug out of a certain blue clay, on the banks of a small rivulet, at the foot of the Wolds. There are plenty of them washed into the brook; but the most fair and solid are those that are got out of the clay.

The matter and substance of these stones, if broken, is flint-like, of a dark shining polish; but much softer, and easily corroded by an acid menstruum. Vinegar indeed makes them creep; but a stronger spirit, as of nitre, moves them violently. I doubt not but they will readily calcine, as the belemnites, to a very strong and white lime. These stones are all fragments, as we have observed of the entrochi; either one single joint, or two, three, or more joints set together, forming a five-sided column. I have not yet had any piece much above 1 inch long, which consisted of 18 joints; but I have seen one piece, somewhat shorter than the former, which had 25 joints. Every joint consists of 5 angles, which are either jutting out and sharp, and consequently the sides of pieces, made up of such joints, are deep channelled; or the angles are blunt and round, and the sides plain or very little hollowed. Where the joints are thin or deep, they are so equally throughout the whole piece; yet there are some, though very few pieces, which consist of joints of unequal thickness. Many of the thick-jointed pieces have certain joints somewhat broader, or a very little standing out at the angles, and thereby the joints are distinguished into conjunctions of two, three, or more joints: and these conjunctions are very observable in the thin-jointed stones, and are marked out with a set of wires.

The thickest piece yet come to my hands, is not above an inch and a half about, and those very rare too: from which size to that of a small pin, I have all the intermediate proportions; and these so exceedingly small pieces are as exactly shaped as the largest. Most pieces, if not all, of any considerable length, are not straight, but visibly bent and inclining. All the pieces of any sort are much of an equal thickness, or but little tapering; yet one of the ends, by reason of a top joint, is visibly the thickest. This top joint has five blunt angles, and is not etched or engraven, or but very faintly, on the outside.

Every joint else of a piece, save the top joint, is an intaglio, and deeply engraven on both sides alike, and will accordingly serve for a seal. The middle of each angle is hollow, and the edges of the angles are thick furrowed: the termination of these etchings are the indented sutures, by which the joints are set together; the ridges of one joint being alternately let into the furrows of the other next it. The etchings of the flat-sided pieces are in circular lines; but of the other two species they are straight lines or nearly so. In the very centre of the five angles is a small hole, conspicuous in most joints; and in the middle of each joint, between angle and angle, in the very suture, is another such like small pin hole very apparent, if the stones be first well scoured. In the deep-jointed pieces, just under the top-joint, above described, may be observed the traces of certain wires, rather than branches; and sometimes two, three, or more of the joints of the wires yet adhering. These wires are always five in number, viz. one in the middle or hollow part between angle and angle.

It is no wonder that these wires are knocked off, and but very rarely found adhering to the stones they belong to, being very small and slender, of a round figure, and smooth-jointed, set together per harmoniam, and not indented suture. Nothing is so like these wires as the antennæ of lobsters. Lastly, some of these wires are knotted, and others of them fairly subdivided or branched.

Many of these star-stones are represented in pl. 8, in the figures from 1 to 17; the explanations of which are as below:—

1. The top-joint of an astroites, figured on both sides; on the one it is deeply engraven, on the other the etches are scarcely visible. Also the ends of the five angles are very blunt. 2. A second or sharp-angled joint, with fair etchings on both sides. 3. A piece with very narrow and sharp angles. Also the top-joint designed, as it naturally appears smooth, and without etchings. 4. A round-angled joint. 5. A flat-sided piece; where the etchings are somewhat circular. 6. A thin jointed piece: where note also, that the angles are much narrower, and of a protracted oval figure. 7. The largest piece I have yet seen, a little bending. 8. The smallest piece I have yet met with. 9. The longest piece; where every 4th joint is somewhat larger or more prominent than the rest; as in the 7th fig. also is well designed. 10. A large and round-angled or flat-sided piece; to which belongs that single joint noted fig. 4. 11. A flat or not hollow-sided piece; of which sort also is the 5th figure; the 10th and 4th not much differing. 12. A thin-jointed piece; where the conjunctions are marked out by the traces of the several sets of wires or branches. 13. A piece where the joints are unequal in thickness. 14. A piece with some part of the wires yet adhering in their natural order at the largest end of the piece. 15. A thin-

jointed piece; where on the left side is a single wire accidentally preserved in its natural place, though snapped asunder. 16. A thick-jointed piece with a set of wires in the middle of it. 17. A good long piece of a wire, and a single joint thereof.

An Account of Two Books. N° 112, p. 279.

I. Les dix Livres d'Architecture de Vitruve, corrigés, et traduits nouvellement en Francois, avec des Notes et des Figures; par Claude Perrault* de l'Academie Royale des Sciences, et Medecin de la Faculté de Paris. Imprimé à Par. 1673, in fol.

The author of this version of Vitruvius, and of the notes upon him, considering that one of the obstacles to the advancement of architecture, was the difficulty of drawing the precepts of the art from its true and genuine source, by reason of the great obscurity of Vitruvius, who is the only writer of the ancients that we have on this subject, undertook therefore, by a translation into the French tongue, and by notes on the difficult places, and also by illustrating all with figures, to render this author more clear and useful to those who embrace the profession and practice of that noble art. These figures and explanations having been rendered the more necessary, by the obscurity in many passages in Vitruvius, owing, it is supposed, partly to the mis-copyings, and partly to the omission of the original figures, by the transcribers.

The figures which serve for illustration, are done with no ordinary care and elegance: amongst which there are, the representation of the Parisian observatory, for making celestial and other natural observations: models of two new engines for raising heavy burthens; so contrived as to avoid rubbing; invented by the interpreter himself, the one by a roller, the other by a lever. An engine for raising water very high and incessantly, and that in great quantity, without employing any external force; a scheme of the organ of the ancients, as also of their catapultæ, and balistæ, the former casting javelins, the latter stones. The models of these engines, and many more, both ancient and modern, are to be

* This author was born at Paris, 1613, his father being an advocate to the Parliament, originally of Tours. He studied philosophy and natural history, as well as medicine, which was his original profession, but neglected it for the fine arts, and became a celebrated architect. In this way he erected some grand works, and was author of some useful books, besides the translation in this article above noticed; viz. 1. An Abridgement of Vitruvius, in 12mo. 2. Ordonnances des cinq especes de Colonnes selon la methode des Anciens, 1683, fol. 3. A Collection of Machines of his own invention. 4. Essais de Physique, 4 vols. 12mo, or 2 vols. 4to. 5. Memoires pour servir à l'Histoire naturelle des Animaux, 3 vols. 4to. Perrault died at Paris, Oct. 9, 1688, at 75 years of age.

found in the royal library at Paris, where the French Academy hold their ordinary meetings.

II. Authonii le Grand Dissertatio de Carentia Sensûs et Cognitionis in Brutis: Lond. 1671.

The author of this tract, consonantly to the Cartesian principle, places the life of animals in the continued motion of the blood. And then declares his opinion that matter is incapable of perception; as also that cogitation cannot be truly affirmed of extension, neither as an essential part, nor as a property, nor as a mode thereof: against Mr. Hobbes, who undertakes to maintain cogitation to be a corporeal motion; and likewise showing, against Gassendi, that it is improbable that sense should arise from insensible things.

M. Leibnitz on his Portable Watches. N° 113, p. 285.

The principle I thought on some years ago, for making exact portable watches, is altogether different from that which consists in an equal duration of unequal vibrations of pendulums or springs, applied to watches by M. Huygens, with such general applause; this depending upon a physical observation; but mine being grounded on a mere mechanical reflexion, which is easy enough, and whereof the reason and demonstration itself are manifest to our senses, which have not been noticed, for want of the art of combination, the use of which is far more general than that of algebra. For having considered with myself, that a spring being bent to the same degree, will always unbend itself in the same time, provided it find the same freedom of unbending itself suddenly: I inferred from thence, that there might be employed two such, one of which should play, while the first mover of the watch should bend the other again; since it will be no matter in this way, whether it bend again more or less speedily, provided it bend only again before the other have done unbending itself; and consequently, the one delivering the other at the end of its motion, this play will always continue uniform, and so by letting go, at every turn or period of these two springs, a tooth of a wheel carried about by the ordinary motion, which counts seconds, or other parts of time equal to the periods, we shall have such a watch as is desired by us.

These thoughts I have executed in the following manner:—Let AB be one of the watch-plates, (fig. 19, pl. 8); C and M two indented barrels, wherein the small springs are enclosed. The teeth of the barrels catch those of the pinions d, d, which carry the balances e, e; and other teeth of the said barrels are caught by those of the interrupted wheel FG. Now let us imagine, that this wheel FG, being moved towards HF, by the force of the first mover of the watch, and turning the barrel C, bends the spring enclosed in it, and stops with the barrel as soon as it has bent this spring. This piece which serves to stop, is easy, and has not been thought necessary to be marked here, to avoid embarrassing the figure. But while one indented part of the interrupted wheel F G, viz. F, turns the barrel C, the empty parts opposed thereto, answers to the other barrel M, and gives liberty to its enclosed spring to unbend itself. Thus while the movement of the watch bends the small spring of the barrel C, in the same time the small spring of the other barrel M unbends of itself. I say, in the same time, except the spring C shall have done bending a little sooner than the spring M shall have unbent itself: so that the spring C being bent, and the wheel F G stopped, both of them stay in this posture, till the spring M, when it shall be quite unbent, at the end of its motion, touch a piece which delivers it. And then the spring C unbends of itself in its turn; the teeth of the interrupted wheel, which continues its motion the same way as before, since it is delivered, not being any more able to hinder it therefrom, because the barrel C now meets with the empty part H of the said wheel. But before it has done unbending itself, the indented part I, being opposite to the empty part H, that turns the barrel M, bends its spring again, and having done so, stops with it, while the spring C, making an end of unbending itself, delivers them by a reciprocal good office, and renders to the spring M the same services, which it had received from it, with an expectation of receiving the like again.

Which being well considered, it is manifest, that the same alternative motions will continue always: that the periods taken from the very moment that one spring begins to unbend, until the moment it once unbends itself again, will always be of equal duration, though the two small springs be not equally strong: that the balance of such a watch will be double, and be charged more or less, and receive delay, by advancing or recoiling, along the two arms, two equal weights, counter-balancing one another, that so the change of the situation may not at all prejudice the equality of the watch. For the rest, we may in this kind of watches spare the fusee, and consequently the string or chain. It is also easy to judge, that such watches as these may be of a size sufficiently small; that they will make no more noise than ordinary watches; that they will be as exact as pendulums, and cease not to go while they are winding up. And though the motion of the watch-wheels may be altered by many accidents, such as are, the inequality of the motion of the great ordinary spring, I mean the first mover; the more or less rubbing of the wheels according as the oil grows thinner or thicker; the rust, the verdigrise, the play of the pieces, the inequality of the teeth and the like; yet the periods of the small springs will not be concerned in all or any of them, provided the motion of the watch-wheels have always more strength than it needs to bend them again; which is in our power. And so the principle of equality here is sure by a kind of demonstration altogether geometrical, and withal very evident even to ordinary capacities.

It remains to notice, in a few words, the objections that have been made against this contrivance by some intelligent persons. They have all acknowledged, that this would be a perfectly exact watch for common use; but if employed for finding the longitudes, there would occur these difficulties, viz. that tossing of ships would shake the springs as well as other pieces; that rust would spoil them, since the saltish humidity of the sea in remote voyages spares not the very needles of compasses, though enclosed in boxes; that the changes of seasons and climates will sensibly alter the springs, especially the great heats, or rains within the tropics, which at length will somewhat untemper the steel; as is confirmed by the experiments of the illustrious academy of Florence, showing how easily heat and cold change slender springs: besides that the air, more or less condensed, will also more or less resist the motion of the balance. To which may be added, that springs are weakened by working; and lastly, that there will be always some little friction, that will make the several pieces go more or less easily, and that even in length of time they will wear out.

But I answer, that all these defects, that proceed from the imperfection of the matter, may be surmounted by a general remedy, without examining them here in particular. And that is, that for executing it in great, we may make use of massy springs, as are those of cross-bows, we being masters of them, not wanting force or place in a ship to govern a great weight that may serve to bend them continually again. Now these massy springs may be so great, and their restitution so speedy by augmenting their number, that all the above named defects will have no considerable proportion to this strength, and the aggregate of their repetitions will not be sensible till after a very long time. And it is easy to demonstrate, that by augmenting the size of the engine, and the force of the massy springs, we may make the error as small as we please, provided we pass not the bounds of conveniency, and content ourselves with an exactness sufficient for the end they are principally designed for, which is the finding of the longitudes: which answer is so clear and so universal, that all those that have considered it have expressed their satisfaction therein.

Observatio Eclipseos Lunæ totalis cum Occultationibus quarundam Fixarum, habita à Joh. Hevelio, Anno 1675, *die* ♀, 11 *Januarii S. N. Vesp.* N° 113, p. 289.

This eclipse was observed by Hevelius, at Dantzic, as follows:

Beginning of the eclipse	6h.	41m.	50s.
Total immersion	7	42	44

Emersion begins	9h.	12m.	30s.
End of the eclipse	10	20	0
Duration of total darkness	1	29	46
Total duration of eclipse	3	38	10

The clock corrected by altitudes of the stars.

The Improvement of Cornwall by Sea-sand. By Dr. Dan. Cox. N° 113, p. 293.

This sand, employed in the agriculture of Cornwall, is commonly at or near the sea shore. To distinguish it from what is useless, observe, that the wash of the sea rolls and tumbles stones and shells, &c. one over another, whose grating makes this sand. If the matter be shelfy, that is the grating of stones, it is of small value. But if it be shelly, then it is good. Of this shelly sand there are three colours. About Plymouth, and the southern coast, the sand is bluish or grey like ashes; which I conceive to be from the breaking of muscles chiefly, and oyster-shells mixed with it. Westward near the Land's-end, the sand is very white, and in Scilly, glistering. This I think happens from the mouldering of moor-stones, or a kind of free-stone mingled with very white shells called scollops. On the north sea, from about Padstow and eastwards to Lundy, the sand is rich and of a brown reddish-yellowish colour, and is mostly of the broken shells of cockles; which I guess to be of that colour there, from the wash of the Severn, which falls very dirty into the Severn sea, and perhaps that accretion of the shells may be tinged thereby.

Now besides these colours of sands, there is also a difference in the size of the grain. Even in the same harbour of Plymouth in some coves, it is very small, in others long grained, and is used variously for divers purposes. It is said that the small is best for the tenant, who only takes to tillage for four years, because it works sooner, and yields a speedy return. The larger grained is said to be better for the landlord and the land; because it lasts longer in the ground, and makes the pasture afterwards the better.

Of all these sands, the best are accounted, as to colour, first the reddish, next the blue, then the white. As to kinds, the most shelly and the coralline are best: and that which is taken up from under the salt-water, either by dredges, or being left open by the ebbing of the tide. The blown sand is accounted of no use. And generally, if sand be well drained of the salt-water, so that it may be more conveniently carried, it is better than that which has lain long drying in the sun and wind, which takes off much of its virtue.

These useful sands are carried by lighters as far up into the country as the tides will serve to that purpose, and there they are cast on shore. When brought home, it is spread on the ground intended for wheat, or usually in the first crop of four, whatever be the grain. For after four crops, it is the custom to leave the land to pasture for 6 or 7 years, before tilling it again. And the grass will be so good immediately after tillage, that it is commonly mowed the first year; and is called mowing of gratten.

The Cornish acre is 8 score yards, of 18 feet to the yard; in one of which acres good husbands bestow according to the nearness or distance. Near the sand, 300 sacks are laid; at other distances 200, or 150, or even 80. And so proportionably in greater distance, even to 20 or 30 sacks in an acre, rather than none.

The effect is usually where much sand is used, the seed is much, and the straw little. I have seen in such a place good barley where the ear has been even equal in length with the stalk it grew on. But where less sand is used, there is much straw, and but little, and that hungry grain.

After the corn is off, the grass becomes mostly a white clover, with some purple, if the land be deeper. And this grass of well sanded ground, though it be but short, yet as to feeding, giving good creams, plenty of milk, and all other good purposes, it far exceeds the longer grass, where less sand is used. Garden herbs also and fruits, in those places, are more and better in their kind. In those well sanded places also little or no snow lies; there is a continual winter spring; an early harvest, a month or 6 weeks before what is within 6 or 7 miles of the place; and such a vast difference of the air is found in so little a distance, that a man may in an afternoon travel as it were out of Spain into the Orkneys.

An Account of some Books. N° 113, p. 296.

I. Hermetis Egyptiorum et chemicorum Sapientia, ab Hermanni Conringii* Animadversionibus vindicata per Olaum Borrichium, Hafniæ, Anno 1674, in 4to.

* An account of Borrichius having been already given in the 1st vol. of our Abridgement; we shall here insert some biographical particulars concerning his opponent.

Herman Conringius was a man of universal erudition, and acquired a great reputation in all the three professions of divinity, law, and physic. He was born at Norden, in East Friesland, 1606, and died at Helmstadt, 1681, aged 75. He was of a remarkably diminutive stature, but possessed great dimension of intellect, and a very retentive memory. Besides the professorship of physic, he held other appointments in the university of Helmstadt, and had the title of councillor of state conferred upon him by the province of East Friesland, and by the courts of Brunswick, Denmark, and Sweden. He also enjoyed an annual pension from the King of France. His historical works are very voluminous. Of his writings which relate to physic, the most celebrated are his de Germanicorum corporum habitus antiqui et novi causis, 4to, 1645, and his Introductio in universam artem medicam, 4to, 1654. It is his Treatise de Hermetica Ægyptiorum medicina, which is attacked by Borrichius, in the work of which an analysis is given by Mr. Oldenburg.

The learned author of this vindication, begins with showing against his famous antagonist, that the Egyptian Hermes, as an excellent man, a great physician and chemist, has well deserved of all mankind, and consequently is highly injured by Conringius's detractions. In this part the reader will meet with a fund of learning and antiquity, and see, among many other particulars, that Pythagoras, one of the best and most solid philosophers and mathematicians among the ancients, learned his philosophy in Egypt; that the great work of transmutation is due to this Hermes; that from thence the Egyptians acquired that immense wealth, by which they raised such vast structures; that those Egyptians were so skilful in making artificial gems, that in lustre and hardness they vied with the true natural ones; those ancient artists being masters of three things in this their work, which by the Grecians were called ἀραίωσις, βαφὴ, στύψις; the first implying a laxity of pores, sufficient to imbibe the tincture; the second, a strong adhesion and due lustre of the colour; the third, a hardening again of the body of the gem, after the ingress of the tincture.

He takes notice of another particular, strictly observed among the old Egyptians, viz. that each of their physicians applied himself to the knowledge and cure of one only disease, by which he became very sagacious and expert in recovering his patients of such a malady; which way could not but conduce very much to the improvement of physic, and the benefit of the people. He observes also, that the most celebrated men of Greece travelled into Egypt to acquire knowledge, and gained every advantage from their travels they could desire.

Discoursing of the virtues of preparations made of animal substances, and particularly of the spirit of blood, he declares, that the volatile spirits of human blood are more powerful in the curing of the epilepsy, than those of the blood of other animals; refuting the assertion of Conringius, importing, that the ancients did not employ human blood but in their magics.

Examining the controversy, whether the virtues of purgatives or vomitives pass into their distilled waters, he recites an experiment he made on a dog, with the distilled water of black hellebore, which was, that having given him 12 spoonfuls of it within 4 hours, he vomited four times, and dunged twice, all very copiously.

Discussing the question about the resuscitation of plants, which he seems inclined to maintain, he alleges, for the countenancing of it, the regeneration of bodies of other kinds, and amongst them he takes notice of mercury, affirming, that that substance, having been a whole year exposed to various fires, and reduced into water, turbith, and ashes, will, by the attraction of the salt of tartar amidst the flames, return to the pristine liquor: and that lead, reverberated into minium, melted into glass, reduced into a cerusse, burnt to a litharge; in a word, tormented, torn, or burnt, as you please, it will soon rise again into genuine lead, by a bare dexterous application of lixiviate salt.

Conringius affirming, that all sorts of diseases have been cured without chemical remedies; Borrichius maintains, that the lues venerea, a confirmed dropsy, and phthisis, and cancer, and several other maladies, will very rarely be cured by mere Galenical medicaments.

In the conclusion our author shows, that Paracelsus's * manners had been too

* This extraordinary man, who assumed the pompous title of Philippus Aureolus Theophrastus Paracelsus Bombast ab Hohenheim, but whose real name is said to have been Hæchener, was born, according to some biographers, at Einsidlen, in Switzerland, according to others in the small village of Gaiss, in 1493. Very early in life he manifested a strong passion for chemistry, and travelled into various parts of Europe, chiefly with a view of perfecting himself in this his favourite study. On his return from his travels, he was appointed to a chemical and medical professorship at Basil, but he quitted this situation two or three years afterwards, owing to a disappointment he experienced from the issue of a prosecution he had instituted against one of his patients, for the recovery of a most exorbitant fee, of which only a moiety was awarded him by the judges. After this he removed to Alsace, where he resided a considerable time, and got into great repute for his real or pretended cures. He died at Saltsburg, in 1534, according to some accounts; in 1541 according to others; in either case at an age rather premature for one who pretended to keep concealed, in the hilt of an old sword which he constantly wore, an infallible remedy (which he termed Azoth) against all kinds of diseases. It would seem that Bacchus triumphed over all his arcana; for, during some years before his death, he is said to have carried his indulgence in wine to such excess, that he never went to bed sober, nor ever took off his clothes.

When we contemplate the conduct of Paracelsus, or look into his writings, we are astonished that such a man should ever have attracted that degree of notice which he once did. But credulity was the characteristic feature of the age and country in which he lived. How else could it happen, that his barbarous and unintelligible jargon should pass for science; that the sick, rich as well as poor, should have faith in one, who set at nought all the labours of preceding philosophers and physicians; who pronounced anatomy useless; who neglected to investigate the symptoms and progress of diseases; and instead of searching into their real nature and origin, assigned for their production certain fictitious chemical agents; and, to fill the measure of absurdity, pretended to have a specific remedy for all. Such, indeed, was the credulity, folly, and superstition of the age, that he made even a pretension to magic, subservient to his interests. Thus he asserted, that in the portico of the infernal regions, he had disputed with Avicenna, concerning his potable gold, &c.; and that he could produce from the semen virile, put into a phial well corked and sealed, and subjected to a process which he describes, an artificial homuncule, resembling in all respects, excepting its diminutiveness, the real human offspring! These, it is evident, are the extravagancies not merely of an impostor, but of a madman; in fact, the anecdotes recorded of him by his assistant Oporinus, scarcely leave a doubt that he was subject to frequent paroxysms of insanity. We suppose it was in some of these phrensy-fits that he wrote about his familiar demon, about sylphs, and gnomes, and fairies, &c. His collected works, the greater part of which was published after his death, amount to 3 vols. folio.

Inimical as Paracelsus was to true philosophy and science, and extravagant and empirical as he was in his conduct and writings; yet would it appear that he advantaged medicine in some degree, inasmuch as he weaned physicians from that too servile veneration, which, before his time was paid to the severely represented; but whatever they were, that that ought not to rob him of the praise due to his knowledge. Again, that being provoked by a crowd of enemies, he had indulged too far in that human imperfection, which is inclined to retaliate with recriminations; yet that he had not been a magician in the worse sense; of which crime the learned Naudæus also had upon good grounds acquitted him. Moreover, that though his servant Oporinus, gained by his master's adversaries, had virulently inveighed against him; yet may there be gathered out of that Oporinus's epistle more matter of praise for Paracelsus, than all his enemies together have deserved; forasmuch as it is there said, Fuisse in Paracelso mirabilem faciendi medicinam in omni morborum genere promptitudinem et felicitatem.—In curandis ulceribus, etiam deploratissimis, miracula eum edidisse, nullâ victus præscriptâ aut observatâ ratione.—Laudano suo ità gloriatum fuisse, ut affirmare non dubitarit, ejus solius usu se è mortuis vivos reddere posse, idque aliquoties, dum ille (Oporinus) apud ipsum fuit, declarasse!

Mentioning Paracelsus's skill in making the grand elixir, as they call it, our author recites a narration made in his hearing by the Count of Windishgratz, the Emperor's ambassador at the Danish court, concerning a person that was possessor of that great arcanum.—In this narration it is stated, that a certain monk, who resided at Vienna, in the Emperor's palace, was in possession of a purplish-red powder, by means of which he could transmute the baser metals into pure gold; and that being dangerously ill of a fever, he was questioned by the physician who attended him concerning this matter, and confessed "ex indiciis quibusdam se inductum, ut latentem alicubi quem olim Paracelsus seposuerat, lapidem philosophicum fodiendo investigaret, quæsivisse sollicite, et reperisse!"

II. The Garden of Eden, or an Account of the Culture of Flowers and Fruits now growing in England; with particular Rules how to advance their Nature and Growth, as well in Seeds and Herbs, as in ordering of Trees and Shrubs: in two parts, in 8vo, written by Sir Hugh Platt, Knt. newly reprinted.

This title sufficiently explains the nature and contents of the book.

On a Storm and some Lakes in Scotland. By Sir George Mackenzy. Communicated by Mr. James Gregory. N° 114, p. 307.

The wind here, at Tarbut, Dec. 21, 1674, was extraordinary: it broke a standard-stone, that stood as an obelisk, about 12 feet high, 5 broad, and nearly 2 feet in thickness. Whole woods were overturned, being torn up from the

doctrines of the ancients, and showed, (as indeed Basil Valentine also did,) the possibility of curing diseases by a shorter and more vigorous method, viz. by the employment of antimonials, mercurials, and other chemical preparations.

roots, though in a low situation. The wind blew from the north-west, and for a long time it had continued westerly.

There is a little lake in Straherrick, on the Lord Lovel's * lands, which never freezes all over, even in the most vehement frosts, before February; but one night's frost after that will freeze it all over, and two nights will make the ice of a very considerable thickness. I have since been told of two other lakes, one of which is on lands belonging to myself, called Loch Monar, pretty large, which steadily keeps the same course. There is another little lake in Straglash at Glencanich, on lands belonging to one Chisholm; the lake lies in a bottom between the tops of a very high hill, so that the bottom itself is very high. This lake never wants ice on it in the middle, even in the hottest summer, though it thaws near the edges: and this ice is found on it, though the sun, by reason of the reflexion from the hills in that country is very hot, and lakes lying as high in the neighbourhood have no such phenomenon. It is observable also, that about the borders of this lake the grass keeps a continual verdure, as if it were in a constant spring, and feeds and fattens cattle more in a week, than any other grass in a fortnight.

Our famous lake Ness never freezes; but, on the contrary, in the most violent frosts, the greater clouds of steams arise from it, which are soft and warm; and it is observed, that rosemary continued in the gardens about the lake's side, notwithstanding the last winter's long and violent frosts; whereas a far less violent winter usually kills all the rosemary which is in gardens that lie in warmer places, and at the sea side: and though I live near it, and in a better soil and warmer situation, yet any winter, more than usually cold, kills my rosemary, though covered over with straw and litter: whereas near Loch Ness it remained good, though uncovered, in the last sharp winter; which is attributed to the warm steams from that lake.

In Glenelg, at a place called Achigniglium, there is a little rivulet, which turns holly into a greenish stone, of which tinkers, that work in brass, make both their moulds and melting pots; and women their round whirls for spinning.

A Conjecture concerning the Bladders of Air that are found in Fishes; communicated by A. I; *and illustrated by an Experiment suggested by the Hon. Robert Boyle.* N° 114, p. 310.

Reflecting on that question, whether liquids gravitate on bodies immersed or not? I came to a resolution in my own mind, that they do gravitate; and one of the greatest reasons that occurred to me was, that a bubble of air, rising from the bottom, dilates itself all the way to the top; which is caused by the lessening

* Probably misprinted for Lord Lovat.

of the weight or pressure of the incumbent water, the nearer it is to the top. Hence also it occurred to me, that fishes, by reason of the bladder of air that is within them, can sustain or keep themselves in any depth of water. For the air in that bladder is like the bubble, more or less compressed, according to the depth the fish swims at, and takes up more or less space; and consequently the body of the fish, part of whose bulk this bladder is, is greater or less according to the several depths, and yet retains the same weight. The rule de insidentibus humido, is, that a body that is heavier than so much water as, is equal in quantity to the bulk of it, will sink; a body that is lighter will swim; a body of equal weight will rest in any part of the water. Now by this rule, if the fish in the middle region of the water, be of equal weight to the water that is commensurate to the bulk of it, the fish will rest there without any tendency upwards or downwards: but if the fish be deeper in the water, the bulk of the fish becoming less by the compression of the bladder, and yet retaining the same weight, it will sink and rest at the bottom: and, on the other side, if the fish be higher than that middle region, the air dilating itself, and the bulk of the fish consequently increasing, but not the weight, the fish will rise upwards, and rest at the top of the water. Perhaps the fish, by some action, can emit air out of this bladder, and afterwards out of its body, and also, when there is not enough, take in air and convey it to this bladder; and then it will not be wondered, that there should be always a due proportion of air in the bodies of all fishes, to serve their use, according to the depth of water they are bred and live in: perhaps by some muscle the fish can contract this bladder beyond the pressure of the weight of water: perhaps the fish can by its sides or some other defence keep off the pressure of the water, and give the air leave to dilate itself. In these cases the fish will be helped in all intermediate distances, and may rise or sink from any region of the water, without moving one fin.

So far this conjecture: in reference to which, when it was propounded to the Hon. Robert Boyle, he, reflecting on the manner how a fish comes to rise or sink in water, soon bethought himself of an experiment probably to determine, whether a fish makes those motions by constricting or expanding himself? The experiment by him suggested was; to take a bolthead with a wide neck, and having filled it almost full with water, to put into it some live fish of a convenient size, that is, the largest that can be got in, as a roach, perch, or the like; then to draw out the neck of the bolthead as slender as you can, and to fill that also almost with water: whereupon the fish lying at a certain depth in the water of the glass, if upon his sinking you perceive the water at the slender top subsides, you may infer that he contracts himself, and if, upon his rising, the water be also raised, you may conclude that he dilates himself.

On Poisonous Fish in one of the Bahama Islands. By Mr. J. L. N° 114, p. 312.

The fish here, are many of them poisonous, causing a great pain on the joints of those who eat them, which continues so for some short time, and at last, with two or three days itching, the pain is rubbed off. Those of the same species, size, shape, colour and taste, are some of them poison, others not in the least hurtful; and those that are, are so only to some of the company. The distemper to men never proves mortal. Dogs and cats sometimes eat their last. In men that have once had that disease, upon the first eating of fish, though it be those that are wholesome, the poisonous ferment in the body is revived thereby, and their pain increased.*

Observations on Extraordinary Oranges and Lemons. By Petrus Natus. N° 114, p. 313.

There was a tree found in a grove near Florence; having an orange-stock, which, it seems, was so grafted on, that it became, in its branches, leaves, flowers, and fruit, three-formed, some resembling oranges, some lemons or citrons, and some partaking of both forms in one. And particularly as to the fruit, some of this tree were mere oranges, some of them of an oblong shape like lemons, some round like common oranges, and some between both. Some taste like genuine oranges, others have an orange rind but a lemon pulp. Most are of a very strong scent, and their rind of a very bitter taste. The same tree bears also a kind of citron-lemon, yet not so many as of the former kind. It produces also a fruit, that is at once both lemon, citron, and orange. This fruit is so diversified, that some of them are half citron-lemon, half orange; others have two thirds of citron-lemon, and one of orange; others, the contrary: and of all these, some are oblong, some round, some bunchy; some smooth, some rough; some small, others large. Their pulp is so distinguished, that where the orange-pulp ends, that of lemon begins, and on the contrary. Again, the orange-pulp is narrower than that of lemon; but this is tenderer than that, and

* Several fishes, both of the East and West Indies are observed to be highly poisonous at certain particular seasons.† This is probably owing to their having fed on some highly acrimonious marine substances.‡

† Such are some species of tetrodon, and of sparus; the barracuta;‖ the rock-fish (a species of perca); the cavellee (a species of scomber); the smooth bottle-fish (a species of ostracion); and the most virulent of all, the yellow-billed sprat.

‡ Violent vomiting is one of the first symptoms with which those who have eaten of these poisonous fishes are seized. Another effect is the separation of the epidermis or cuticle in patches or spots about the hands and feet, which spots continue white in people of colour and of a pale yellow in white people, for life. The best antidotes against the fish-poison are Cayenne pepper, Madeira, and other powerful cordials. In voyages to remote parts of the globe, and especially to tropical and southern latitudes, the crews of ships may often prevent the mischievous consequences arising from the use of the fishes of those seas and rivers, by gutting them immediately after they are taken, and washing them well in clear water (either salt or fresh) before they are dressed.

‖ The barracuta is a species of *esox* or pike.

less agreeable to the taste than the genuine single fruit. And, which is no less remarkable, they have either none or very few, or empty seeds.—The first origin of this tree, was by inoculating orange on a citron-lemon stock.

An Account of some Books. N° 114, p. 314.

I. Archimedis Opera; Apollonii Perg. Conic. Libri 4; Theodosii Sphærica, methodo novâ illustrata, et succinctè demonstrata, ab Is. Barrow, è Soc. Regia, &c. Lon. 1675, in 4to.

The learned author has here delivered these three books in a brief symbolical method of expression, pursuant to the sense, propositions and demonstrations of the ancients; unless where he thought fit to enlarge, and otherwise to demonstrate some of the propositions from more easy principles of his own; pursuing herein his own former method, in which some years ago, he published an entire Euclid in 8vo. Besides, this edition contains a new version of Archimedes's Lemmata, which were not formerly published with the rest of Archimedes's works; though to be found in Forster's Miscellanies, and at the end of Joh. Alph. Borelli's edition of the three latter books of Apollonius's conics. It will readily be acknowledged, that our author had cause to find fault, as he does, with the obscurity of Rivalt's edition; who is also much complained of by Mydorgius in his Conics, and by Alex. Anderson, in his Mathematical Exercises.

II. Thomæ Bartholini Acta Medica et Philosophica Hafniensia Anni 1673. Haf. 1675, in 4to.

A continuation of a useful medical and philosophical work, of which a former volume was noticed in N° 97 of these Transactions.

III. The Epitome of the whole Art of Husbandry, with Additions of New Experiments thereto belonging: written by J. B. Gent. in 8vo. Lond. 1675.

A Proposal to Noblemen, Gentlemen and others, who are willing to subscribe towards Dr. Morison's New Universal Herbal, ordering Plants according to a new and true Method, never published before. N° 114, p. 327.

[For critical remarks on this work, see life of Morison, p. 342, volume I, of this Abridgement.]

A New Essay Instrument. By Mr. Boyle. N° 115, p. 329.

For many years I had made use of a little glass instrument, consisting of a bubble, and furnished with a long and slender stem, to compare the specific gravities of different liquors, by its sinking in them more or less. Afterwards I applied it to discover the specific gravities of several appended solids, by its being more or less depressed by them in the same liquor. For it is plain from hydrostatic principles, that any solid body, heavier than water, loses in the water as much of the weight it had in air, as water of equal bulk with the immersed solid would weigh in the air. Consequently, since gold is by far the most ponderous metal, a piece of gold, and one of equal weight of copper, brass, or any other metal, being proposed, the gold must be less in bulk than the copper or brass; and if both of them be weighed in water, the gold must lose in that liquor less of its former weight, than the brass or copper: because the baser metal as well as the gold, grows lighter by the weight of a bulk of water equal thereto; and the baser metal being the more voluminous, the corresponding water must weigh more than that which in bulk is equal to the gold. Whence the floating instrument above-mentioned would be made to sink deeper by an ounce of gold, hanging at it under water, than by an ounce of brass, or any other metal; which, on account of its greater bulk than gold, losing more of its weight by the immersion, must needs retain less, and so have less power to depress the instrument it was fastened to. And this will hold of other metals that differ in specific gravity.

This instrument may be of glass, copper, silver, or any other solid body, that is, or may be made, fit to float on the water, with a guinea, &c. hanging at it, and of a texture close enough to exclude the water. It consists of three parts; viz. the ball, the stem, and that which holds the coin.

The ball or round part BCDE, fig. 1, plate ix, if of metal, consists of two thin concave plates, exactly soldered together in the middle, and at the most distant points from the juncture, there should be two opposite holes, one in each plate, for the two other parts of the instrument. This middle part, though for brevity sake called the ball, should not be exactly round, but of any shape that shall be found fittest to make the instrument keep an erect posture steadily in the water; and it must be greater or smaller as the plates are thicker or thinner; but the general rule for its capacity is, that it should contain as much air as may serve to keep the whole instrument, when loaded, from sinking beneath the top of the stem.

The stem AB is to be soldered on to the ball at the uppermost of the two mentioned holes. It may either be hollow or solid; but it should be made very slender, that the different depressions of the instrument in the water may be the more notable; and for the same reason it should not be too short, especially if it be applied to other uses than the examining of guineas.

At the undermost of the two holes in the ball, is inserted and soldered the screw or stirrup F, fig. 2, which is a short piece of brass with a broad slit in it, capable of receiving the edge of the guinea, to be fastened in it by a turn or two of a screw. The stirrup G is made of a piece of wire bent round, and standing horizontally, that the guinea may be laid on it.

It would be convenient that the undermost stem and the screw be made by itself, that it may be at pleasure thrust on the stem, and taken off again; for by this means, if the ball of the instrument be made large enough, you may have room to put on for ballast, as occasion shall require, one, two, or three flat and round pieces of copper, lead, &c. as fig. 3, with each of them a hole in the middle, fitted to the size of the stem, so that they may be put on as near the lower part of the ball as you think fit, and then the screw may be thrust on after them; not only to take hold of the coin, or metallic mixture, to be examined, but to support the thin plates.

To adjust this instrument for examining guineas, which are by far the most usual gold coins; hang at the bottom of it, a piece of that coin you know to be genuine, and having carefully stopped the orifice of the stem, immerse the instrument leisurely and perpendicularly into a vessel full of clean water, almost to the top of the stem, and then letting it alone. If it continue in the same situation and posture, the work is done; if it emerge, you must add a little weight, either by putting into the stem, if it be hollow, some dust shot, filings of lead, or some other minute and heavy body; or else by putting on the short stem, that comes out beneath the ball, a flat, round, and perforated piece of lead, of weight sufficient to enable the guinea to depress the weight as low as it is desired. But if it sink quite under water; then to make it lighter, file or scrape off a little of the ballast plate, and take out some of the weight, put into the cavity: this being done, a mark H, fig. 1, is to be made, just at the place where the surface of the water touches the stem; then taking out the instrument, substitute in the place of the guinea, a little round plate of brass, of the same weight, or a grain or two heavier in the air; and putting the instrument into the water as before, suffer it to settle, and make another mark I, at the intersection of the stem, and the horizontal surface of the water.

It may happen, that a falsifier of money may have the skill, by washing or otherwise, of taking off much of the quantity or substance of the guinea, without altering or impairing either the figure or stamp; and thus the piece of coin will not be able to depress the instrument to the usual mark, and thereby be judged counterfeit, when it is indeed but too light. But the balance will soon resolve the doubt; for if the suspected coin have in the air its due weight, it will argue, that its great lightness in water proceeds from its not being of the requisite fineness. And if it want much of its due weight in the air, it is very probable that it is washed.

A general way for finding what coins may, or may not be examined, by this or that particular instrument proposed; first, weigh the piece of gold or silver in the air, and afterwards in water, and subtract the latter from the former.

In the next place, weigh also in the air and water a piece of copper or brass, if that be the likeliest to be employed in counterfeiting the coin, and observe their difference: then the less of these differences being subtracted from the greater, the remainder will show how much the true piece of coin will outweigh the other in water; and consequently if so many grains as this remainder amounts to, being added to the weight of the lighter metal, do make a sufficiently manifest depression of it below the mark, it would stay at without that addition, we may conclude, that probably the difference between a true and counterfeit piece of coin proposed, will be discoverable by the instrument: but it may be expedient for those that have frequent occasion to examine various sorts of coin, to have a several instrument adjusted for each of them.

With this instrument pure tin may certainly be distinguished from such as is adulterated: for as gold, being the heaviest of metals,* cannot be allayed with any other, that will not depress the instrument less than gold can do; so tin being the lightest of metals, cannot be mixed with any other, that will not sink lower than unmixed tin; still supposing the weight to be the same in the air.

In the same manner may pewter be compared and examined. For having once observed how much the instrument is depressed by a piece of two, three, or four drams, or even an ounce weight of pewter, which is known to be good, and to contain such a proportion of lead in reference to the tin; if you load the instrument with an equally heavy piece of any other mass of pewter proposed, and the instrument sink deeper, it will be a sign that the former proportion of lead may be very probably argued to exceed in the mixture. This instrument may also assist in making a pretty tolerable estimate of the fineness of gold, and its different allays with silver, or some other determinate metal. In order to which the instrument may be fitted to sink to the tip of the pipe, with some determinate weight of the finest gold, as of 24 carats. But it will be proper, that this metal in the air be some determinate weight, that is commodiously divisible into many aliquot parts. Then you may make a mixture that contains a known proportion of the metal wherewith you allay the gold: as, if it contain 19 or 15 parts of gold, and one of silver: and letting the instrument settle in the water, mark the place where the surface of the water cuts the stem or pipe. Then putting in another mixture, wherein the silver has a new and greater proportion to the gold; as if the former be an 18th or 14th part of the latter, you may observe how much less this depresses the instrument. And thus you may

* Gold was the heaviest of the then known metals; but platina, (afterwards discovered, and of which an account will be given in a future volume of these Transactions) surpasses it, when freed from all its impurities, in specific gravity.

proceed with as many mixtures or degrees of allays, as you think fit; or as many as may be conveniently distinguished on the stem: always observing, that whatever be the proportions of the two ingredients, the weight of the mass in the air be just the same with that of the pure gold.

By the same method the different allays of pure silver may be examined, on the mixing of any proportions of copper, or any other metal lighter in specie than silver. And by the same way, with a slight variation, it will not be difficult to estimate how much divers coins, whether of silver or gold, are more or less debased by the known ignobler metal, mixed in the proposed piece. These estimates, which may be made without much trouble, will come nearer the truth; not only than the estimates wont to be made by the touchstone, but perhaps also than some of those made with trouble and charges.

It may also be used to examine other mixtures, besides allayed coins; and if the instrument be adjusted to an ounce, suppose of pure copper, it may assist in making an estimate of the allay of tin, or the quantity of it added oftentimes to copper, for making different sorts of bell-metal, and of those metallic specula, whether plane or concave, called steel mirrors; as also of solders, consisting of certain proportions of silver and brass or copper; in all which, and divers others, the discovery of the proportion of the ingredients may, on some occasions, be useful to tradesmen, as well as agreeable to virtuosi.

On the Swimming Bladders in Fishes. By Mr. Ray. N° 115, p. 349.

I was much pleased and satisfied with the ingenious conjecture I found in your Transactions of May last, N° 114, concerning the swimming bladders of fishes; and persuade myself that the author has hit upon their true use, viz. to sustain or keep them up in any depth of water. For 1. It has been observed by some, and I find it in Mr. Willughby's general notes of fishes, that if the swimming bladder of any fish be pricked or broken, such a fish sinks presently to the bottom, and can neither support nor raise itself up in the water.—2. Flat fishes, as soles, plaise, &c. which lie always groveling at the bottom, have no swimming bladders that I could ever find.—3. In most fishes there is a manifest channel leading from the gullet or upper orifice of the stomach to the said bladder, which doubtless serves for conveying air into it, as may easily be tried by any one that pleases. But though air may be received into the bladder, yet is there a valve or some other contrivance to hinder the egress of it; for you shall sooner break the bladder than force any air out by this channel. Yet in sturgeons Mr. Willughby has observed, that pressing the bladder, the stomach presently swelled: so that it seems in that fish the air passes freely both ways.

Possibly the fish, while alive, may have an ability to raise up this valve, and let out air upon occasion; which yet I doubt of, because other animals have no such faculty of opening any valves made to stop the reflux of fluids. But I verily think, there is in the coat of this bladder a muscular power to contract it when the fish lists: for in many fishes it is very thick and opaque, like the coat of an artery, which has, as Dr. Willis observes, such a muscular power, as for example in all the cod kind; in some, *v. g.* the hake, it is inwardly covered with a red carneous substance, which I take to be muscular flesh; in others it is forked at the top, and to each horn has a muscle affixed. Now the muscular force need not be great, being still assisted by the water as the fish descends; the pressure of the water being much greater at the bottom than at the top, as appears by the ascending bubble. But whereas it is said, perhaps the fish can by its sides or some other defence, keep off the pressure of the water, and give the air leave to dilate itself: it may be objected, if it can do so, what needs then any air bladder? the cavity of the abdomen may serve the turn. To which I answer, that this power of dilating the abdomen by the muscles, may assist fishes to rise, whose natural place is toward the bottom; and the air compressed in the bladder dilating itself as the fish ascends, facilitates the action of the muscles. But those fishes that descend by contracting the bladder, letting the contracting muscle cease to act, will rise again of their own accord, the air within dilating itself, as we see in glass bubbles by compression of the air in them descending, which as soon as the force is removed ascend spontaneously. Besides the flat fishes I before-mentioned, all the cartilaginous kind, as well flat as long, want swimming bladders: what course they use to ascend and descend in the water, I know not. Many of the eel kind have swimming bladders; yet can they hardly raise themselves in the water, by reason of the length and weight of their tails: I suppose the air-bladder, being near their heads, helps them to lift up their head and forepart.

Middleton, June 22, 1675.

How to make all Sorts of Plants, Trees, Fruits, &c., grow to an extraordinary size; from the Journal des Sçavans. N° 116, p. 356.

The whole secret consists in sowing all sorts of grains and kernels, in beds of earth, at the very time when the sun enters into the vernal equinox, and to take them up when they are strong enough to be transplanted, at the time of the full moon; which time is always to be observed, if you would take them up and plant them again.

Advertisements occasioned by the Remarks printed in N° 114, *upon Frosts in some parts of Scotland, differing in their Anniversary Seasons and Force from our ordinary Frosts in England: of Black Winds and Tempests: of the warm or fertilizing Temperature and Steams of the Surface of the Earth, Stones, Rocks, Springs, Waters, (some in some Places, more than other in other Places;) of Petrifying and Metallizing Waters: with some Hints for the Horticulture of Scotland: by the Reverend and Learned Dr. J. Beal, F.R.S.; who by Way of Letter imparted them to the Editor.* N° 116, p. 357.

There is no part of these long and uninteresting remarks, that can be of any use to reprint in this collection.

Extract of Mr. Flamsteed's Letter of July 24, 1675, *to the Editor, relating to another, printed in* N° 110 *of these Tracts, concerning M. Horrox's Lunar System.* N° 116, p. 368.

The commonly received lunar systems, though as little agreeable with nature as with the heavens, were entertained from one astronomer to another with little alterations, from the noble Tycho to the author of the Caroline tables: but when I had found, by many curious and careful measures of the moon's diameters, that the heavens would never admit those hypotheses, which made the diameter of the perigæon moon in the quadratures larger than the full moon on the perigee; and that only the system of Mr. Horrox, which I had found in Mr. Crabtree's letters, would represent it as observed broadest at full; and afterwards that it would accord well with some observations to which the common tables agreed not within $\frac{1}{5}$ of a degree: I thought it might be worth my labour to adorn it with numbers, that it might be fit for trial as soon as it appeared in public; to which I added an explanation, to be found in the edition of his posthumous works. This system has been well approved of by several good astronomers, and Mr. Street has esteemed it so good, that he has printed a figure of it with the description of his planetary instrument; but without acknowledging the proper author, or so much as naming him more than once, and then in such terms as may persuade any person not well acquainted with Mr. Horrox's works, that he was but some inconsiderable astronomer, and Mr. Street's print a quite different system. "All, says he, being somewhat different from the limitations of the theory of Horrox, and tables therewith published." This obliged me, when I found it only different in the position of the libratory circle, to take notice of it in a letter to Mr. Collins, from whom you received the information, and with my consent printed the extract of it: in which I designed nothing but to assert to the dead an invention, which he esteemed the best he ever made, and which is the chief of his monuments: this had I neglected, I had been unjust to the dead, whose papers passed through my hands to the public; nor have, I hope, been at all injurious to Mr. Street in it.

For my assertion concerning his figure of the lunar system was, that the contrivance of it, for the motion of longitude, was no other than what was taken from Mr. Horrox's theory, and my explication; whereby I conceive no man would understand me of his numbers.

Perhaps he will say, he has transferred the libratory circle from the orbis magnus to the transverse diameter of the ellipsis. But this is not material, since the effect is still the same; and supposing the same diameter of the libratory circle, the same equations will be found, to a second; so that hereby he only has gained a pretext to call the system his own, but he has rendered the case of the libration less intelligible.

He adds, that "he has increased the quantity of the greatest libration 22 min." So that the semi-diameter of the little circle, that shows the variation, may subtend the greater equation of the apogee, the mean eccentricity being radius. This indeed is an ingenious conceit; yet it amounts to no more than an alteration; which, whether the heavens will admit of, we may justly question. I find by Mr. Horrox's papers, that he used at first 12° precisely, but on farther experience diminished it to 11° 48′.

But the main part of his defence is, that his numbers are not the same with those published with the Horroxian theory, and therefore the system is not the same. I argue not how illogical the inference is; but how little ingenuous, you may judge, in that he has published only the aforementioned coarse ones, so that for the eccentricity and variation we must believe him gratis. Nor is it pertinent what he says concerning the working of proportions in triangles mechanically. If he knew how to do it before me, no less did others much longer before him; nor am I at all beholden to him for this skill: but if he prefers calculations before it, to what purpose is this print of the lunar plate, or his contention about it.

A Total Eclipse of the Moon, observed in London, June 26, 1675, by Mr. John Flamsteed, Astronomer Royal. N° 116, p. 371.

Beginning of the penumbra	13h.	46m.	40s.
The total immersion	14	56	55
The same observed at Paris by M. Bulliald.			
Beginning of the true umbra	13	55	0
The total immersion	15	6	0

An Account of a Book. N° 116, p. 373.

The Planter's Manual: Being Instructions for the Raising, Planting, and Cultivating all sorts of Fruit Trees; whether Stone-fruits, or Pepin-fruits, with their Natures and Seasons; very useful for such as are curious in Planting and Grafting. By Charles Cotton, Esq. in 8vo, London, 1675.

Microscopical Observations. By M. Leuenhoeck. N° 117, p. 378.

After drying an optic nerve, and making a transverse section across it, M. Leuenhoeck not only observed one hole in it, but several, which made it resemble a leathern sieve; only with this difference, that the holes in the nerve were not round, no more than they were all of the same size, nor so regularly posited, as those in a sieve.

He observed the sanguineous globules, that make the blood red, were firmer and harder, when his body was much out of order; but afterwards softer, when his body was in a good state of health. And he is of opinion, that those sanguineous globules in a healthy body must be very flexible and pliant, to pass through the small capillary veins and arteries; and that, in their passage, they change into an oval figure, re-assuming their roundness, when they come into a wider space. He observed also, in the clear matter of the blood, figures of a quadrangular form, which he supposes to be some saline parts.

Optical Assertions concerning the Rainbow. By M. Fr. Linus. N° 117, p. 386.
Translated from the Latin.

1. Any the smallest drop of rain, illuminated by the sun-beams, emits from itself a perfect rainbow, not only as to the colours, but also as to their order, situation, and circular figure, very much resembling that in the heavens.

2. For the sun-beams entering a drop, and, after two refractions and one reflection, returning again from thence towards the sun, escape out of the drop all coloured, and endued with the very same colours, as red, yellow, green, blue, and purple, which are observed in the rainbow.

3. These rays thus coloured, being in rain transmitted to the eye from various drops illuminated by the sun, cause the vision we have of an iris.

4. There are two rings in each drop, a larger and a smaller, endued with distinct rainbow colours; the less of which is distant from the axis, or ray passing through the centre of the drop, by about 21°; and the larger by 78°. But the rays incident on the smaller ring, are thence reflected on the greater; from which escaping into the air, they are endued with the said rainbow colours.

5. Therefore, these colours arise from the solar beams; but not from them alone, as was hitherto thought, but also from the rays of the air itself, surrounding the solar body.

6. But neither do these colours arise from all the rays, whether solar or aërial, which enter the drop; but only from such as are emitted from the solar limb itself, and from the air adjacent to it.

7. But such rays as are thus transmitted from the solar limb, and the neighbouring air, into the drop, do not all belong to the said colours, nor do they escape coloured; but such only, whose angle of incidence is neither less than 45°, nor greater than 75°.

8. Therefore the rainbow colours proceed from the solar limb and the adjacent air, yet all the five do not immediately flow from thence; but four only, viz. red, yellow, blue, and purple; for the green arises from a mixture of the yellow and blue rays.

9. Therefore these four colours arise from the said limb; though not all from one and the same part: but two of them from one part of the limb, and other two from that directly opposite; viz. the blue and purple arise from the upper limb; and the red and yellow from the lower.

10. And there seems to be no other reason of this, viz. why from limbs so very like, dissimilar rays should arise; but that in one case, the aërial limb is above the solar; and in the other case, the solar above the aërial. And this difference seems to be sufficient, because on account of that different situation, at one time the solar rays may be bent by refraction above the aërial rays; and at other times on the contrary, the aërial above the solar.

11. Therefore these colours arise by means of the said refracted rays, yet so refracted, that by that refraction they are very much crowded together. For all the rays, from the 45th degree to the 60th, are in the less ring contracted into the space of one degree; into which narrow space, all the rays, from the 60th to the 75th degree, concur by retrogradation.

12. There are as many rainbows at once, as there are spectators.

13. A spectator observes at each moment a different rainbow.

Observations of a Lunar Eclipse, made at Paris, and compared with those made at London, June 26, 1675, O. S. noticed in Numb. 116 of these Abridgements, p. 221. N° 117, p. 388.

These observations were made by Messrs. Cassini, Picard, and Romer. By which it appears, that the beginning of the eclipse was at 1 h. 56 m. 45 s. after midnight, and the total immersion, or internal contact, at 3 h. 7 m. 45 s.

They may be useful for determining the difference of longitude between the two cities, or places of observation. And another observation, made at Paris, may be also useful, for the same purpose, which is, that a little before midnight the same day, viz. exactly 11h. 0m. 16s. Jupiter's 2d satellite emerged from the shadow of that planet, by which it had been eclipsed.

The observations of the lunar eclipse, compared for the difference of the two meridians, with that difference thence deduced, are as follow:—

Observat. D. Flamstedii.	h. m. s.	Observationes nostræ.	h. m. s.	Dif. Merid. m. s.
Pentadactil. tectus	13 55 15	Idem seu Seleucus	2 6 15	11 0
Porphyrites tectus	14 2 20	Idem seu Aristarchus	2 12 40	10 20
Sinæ limbus primus	14 5 30	Ejusd. seu Tychonis	2 16 27	10 57
Ætnæ limb. primus	14 6 0	Ejusd. seu Copernici	2 16 35	10 35
Besbici limbus prior	14 23 5	Ejusd. s. Manlii med.	2 34 15	11 5
Horminius tectus	14 26 3	Ad eund. seu Dionys.	2 34 15	10 12
Tetigit limbum primum Co- rocondometis	14 39 30	Ejusd. s. Paludis somni	2 50 20	10 50
Tetig. palud. Mæotid.	14 45 0	Eand. s. Mare Casp.	2 55 30	10 30
Mæotis tota tecta	14 15 40	Eadem	3 1 10	10 30
Immersio	14 56 55	Immersio	3 7 42	10 47

On Damps; and on Worms discharged at the Mouth of Children. By Mr. Jessop. Communicated by Mr. Lister. N° 117, p. 391.

There are four sorts of damps. The first is the ordinary sort.* The signs of its approach are, the candles burning orbiculary; the flames lessening by degrees, till they quite go out; and the shortness of breath. I never heard of any great inconvenience which any one suffered by it, who escaped swooning: but those that swoon away, and escape an absolute suffocation, are at their first recovery tormented with violent convulsions, the pain of which, when they begin to recover their senses, causes them to roar out exceedingly. The ordinary remedy is, to dig a hole in the earth, and lay them on their bellies, with their mouths in it; if that fail, they fill them full of good ale; but if that fail, they conclude their case desperate.

The second sort is called the pease-bloom damp, because they say it smells like pease-bloom. This always comes in the summer time; and those grooves are not free, which are never troubled with any other sort of damps. I never heard that it was mortal, the scent perhaps freeing them from the danger of a

* Concerning this and the other *Damps,* in caverns and mines, see note at p. 16, vol. I, of this Abridgement.

surprise. But by reason of it, many good grooves are not worked in the best and most profitable time of the year, when the subterraneous waters are at the lowest. The workmen fancy it proceeds from the multitude of red trefoil flowers, called honey-suckles, with which the limestone meadows in the Peake do much abound.

The third is the strangest and most pestilential of any. Those who have seen it (for it is visible) describe it thus:—In the highest part of the roof of those passages, which branch out from the main groove, they often see a round thing hanging, about the size of a foot-ball, covered with a skin of the thickness and colour of a cobweb. If by any accident, as the splinter of a stone, or the like, this be broken, it immediately disperses itself, and suffocates all the company. Therefore, to prevent its ill effects, as soon as it is observed, by the help of a stick and a long rope, they break it at a distance; after which they purify the place well by fire before they dare enter it again. To account for it in some degree, they say, the steam which arises from their bodies and the candles, ascend to the highest part of the vault, and there condenses, and in time has a film grows round about it, and at length corrupting becomes pestilential.

The fourth, which they also call a damp, is that vapour, which being touched by their candle, presently takes fire, and giving a report like a gun produces the like effects, or rather those of lightning.

As to the vomiting of worms: A girl in Sheffield, about eight months old, was taken with violent vomiting fits, which continued about a week, and made her so weak, that her parents began to despair of her recovery. On giving her about a pint of wormwood ale, she vomited three hexapodes, of the size and shape, fig. 4, pl. 9, all very active and nimble. The girl in a short time recovered, and was well. The surgeon brought the hexapodes to me; we killed one of them with trying experiments upon it. Remembering I had seen some very like them, which devoured the skins of such birds as I kept dried for Mr. Willughby, I gave each of the surviving hexapodes the head of a shining atricapella, which in about five weeks time they eat up, bones, feathers and all, except the extremities of the feathers and the beaks. Desiring to see what they would turn into, I gave them a piece of larus, but that it seems agreed not so well with them, for they died within two days.

I have often been puzzled to account for those phænomena, commonly called fairy-circles.* I have seen many of them, and those of two sorts; one sort

* I am satisfied that the bare and brown, or highly cloathed and verdant circles in pasture fields, called *Fairy Rings*, are caused by the growth of the agaricus orcades. The largest of one of these rings, in Edgbaston-park, was 18 feet diameter, and about as many inches broad, in the periphery

bare, of seven or eight yards diameter, making a round path something more than a foot broad, with green grass in the middle; the others like them, but of several sizes, and encompassed with a circumference of grass, about the same breadth, much fresher and greener than that in the middle. But my worthy friend Mr. Walker, a man not only eminent for his skill in geometry, but in all other accomplishments, gave me full satisfaction from his own experience. Walking out one day among some mowing grass, in which he had been but a little while before, after a great storm of thunder and lightning, which seemed by the noise and flashes to have been very near him; he presently observed a circle, of about four or five yards diameter, the border of it about a foot broad, newly burnt bare, as the colour and brittleness of the grass-roots plainly testified. He knew not what to ascribe it to but the lightning. After the grass was mowed, the next year it came up more fresh and green in the place burnt, than in the middle, and at mowing time was much taller and ranker.

As to the vomiting of strange worms, Mr. Lister observes, that a boy about nine years of age, was afflicted with great pain in his stomach, and continual vomitings. On giving him a powder containing a small quantity of mercurius dulcis, he vomited up several strange worms, two of which were brought to me at York, the one dead, the other alive, and which lived many days after it came to my hands, and might have lived longer, but that I put it into spirit of wine, to preserve it in its true shape. These worms were very caterpillars, with 14 legs, viz. 6 small pointed, the 8 middle stumps, and the 2 hind claspers; something more than an inch long, and of the thickness of a duck's quill, thin haired, or rather naked, with brown annuli, and a black head. The very same for kind that I have often seen on plants, and no doubt these would in due time, if the place had not hindered, have shrunk into chrysalids, and changed into moths, as those mentioned by Mr. Jessop would have changed to beetles.

Some Observations made in Scotland, by Sir George Mackenzie. Communicated by Mr. James Gregory, N° 117, p. 396.

Of earths I have little to say: only one of our most ordinary soils for barley in this country, is an earth dry and mixed with dung of cattle. In a place near

where the agarics grew. It had existed for some years. These larger circles are seldom complete. The large one just mentioned, was more than a semi-circle, but the phenomenon of fairy-rings is not strictly limited to a circular figure. Where these rings are brown and almost bare, upon digging up the soil to the depth of about 2 inches, the spawn of the fungus will be found of a greyish white colour; but where the grass has again grown green and rank, I never found any of the spawn existing. A similar mode of growth takes place in some of the crustaceous lichens, particularly in the lichen centrifugus. Withering's Arrangement of Brit. Plants, vol. 4, p. 222, 3d edition.

me, at Tarbut, there is a plot of ground, less than one acre, out of which for these many years past earth has been dug for that use; and in two years time it will grow up again, and fill the excavated place; so that it continually furnishes soil for the adjacent lands. Another, like this, I have in a farm belonging to myself, twenty miles distant, of the same nature and quality. Both are a stiff clayish earth, of a dark colour and moist. Both these places I have viewed these several years, and have discerned them. It will grow a foot high in two years. Nothing makes our land give greater increase of barley than sea-weed. But lands that are often used to this manure yield but bad oats, and small quantity, and the husks both of the barley and oats that grow on such lands, are thicker than those that grow on other lands, and these grains have also greater mixture of darnel.

The increase in some places in the isles is almost incredible, considering the climate and soil. For some will ordinarily yield 16 or 18 fold. And most of those lands that yield so well, are of a very sandy soil, and only manured with sea-weed. I have a piece of land in Lochbroom parish, that yields continually every year plentiful crops of barley, without ever having so much as one load of manure, or any kind of addition laid on it: and this it has done for time immemorial. Whether it be, that rains wash down matter equivalent to manure from the adjacent hills, which yet cannot be discerned, though looked after; or whether its fertility proceeds from prolific exhalations from a subterraneous cause, I will not determine.

There are also some fields that appear to be nothing else but a gathering of small pebbles, insomuch that earth cannot be well discerned amongst them; yet do they yield abundance of good corn, especially of barley; and more than contiguous lands that are not stony.

As for our herbs, I have nothing extraordinary. All I find here are in history excepting one, which grows on stony shores; but because it is not near me, I will not venture to give you its description now, lest it should be too imperfect. One particular I took notice of, which perhaps is no novelty to you, as it is none to our highlanders; but since it is to me, I shall relate it. When they want ink, they take the root of the iris palustris lutea (yellow water flower-de-luce) and infuse it 24 hours in clear fountain-water; others boil it a little. Then they take a rough white pebble, and rub it continually in the water on a knife or any piece of clean steel; and in less than an hour's time the water becomes very black, and tolerably good ink.

Our foresters allege, that when deer are wounded they lie on a certain herb, which grows plentifully in our forests, and that by its virtue the bleeding is stanched, and the wound healed. I took a quantity of it, and reduced it to a salve, with wax and butter. Its effect was, that it healed too suddenly, so that I durst not venture to use it in any deep wound: but for superficial scars it has a very sudden operation. At that time I did not know this herb by any name; but now I find it to be Asphodelus Lancastriæ verus of Johnston, or the Lancashire Asphodil.

It is very common to find Molucca beans on the shore of the Lewes or our other western isles. They are found fast to the stalk, which the common people supposed to be sea tangles, and laughed at me, when I said they were land-beans: which made me write to the Earl of Seafort, whilst he lived in the Lewes, that I supposed, these apparent tangles were the haum of the beans, which by long lying in the sea might acquire that likeness. His lordship examined the matter and found it so. And he likewise sent to me a piece of a cabbage-tree, that was found on that shore. It is observable, that the kernel of these nuts are often fresh and sound, and the people make boxes for snuff of the bean husk. Now, considering the situation of those isles with respect to any place where Molucca-beans grow, let the observers of tides consider what reciprocations must be imagined, to adjust the eastern and western constant currents of the main with the wafting of these beans, on places that lie so far out of the road of any of the direct tides: and if they grow only about the Molucca isles, or in no place on this side the equator, it would seem more probable, that they came by the northern passage, than any other way. And their freshness in the kernel shows rather that they have been kept in the cold conservatory, than in the warm baths of the other progress.

Observations made at Barbadoes, by Dr. Thomas Towns. Communicated by Mr. Lister. N° 117, p. 399.

At Barbadoes, the general draught of wine is from Madeira; which, contrary to all other wines, will not endure a cool cellar. Neither French nor Rhenish wines keep nor agree well with our stomachs, if so constantly drank as in England. Canary wine few here care for.

This island is very temperate. For the sun, notwithstanding his neighbourhood, is very gentle here, being fanned with a constant gale from the east.

I have heard it questioned, whether America have not some plants common with those of Europe, especially the more northern parts of it. To the clearing of this doubt, I observe, that purslane is here all the country over, where I have been, and even troublesome to the planter. In the fields I have often gathered a sallad of it: and it eats as well with oil and vinegar as that of our English gardens. Here is likewise a sonchus, lens palustris; I found also a melilot, or a plant so like it in all circumstances, except that the branches are not so erect, that I cannot find any difference from that of England.

The springs here are all near the sea, so that those who live up in the country have no benefit of them. They made ponds formerly to receive rain; which served well enough, being kept cool by a broad leaved weed and duck-weed, that overgrow most ponds. But now almost every sugar-plantation has a well that yields very good water.

The soil is fertile, though not above a foot or two thick, upon a white and spongy limestone rock; which affords good quarries here and there, that serve for building. Every dwelling-house, with the sugar-work, and other out-houses, looks like a handsome town; most being now built with stone, and covered with tiles or slate, brought hither in the ballast of ships, as are likewise sea-coal for forges, and so are brought cheap enough. Indeed the whole island appears in a manner like a scattered town which, with the perpetual green fields and woods, makes the place very pleasant.

The blood of negroes is almost as black as their skin. So that the blackness of negroes is likely to be inherent in them, and not caused by the scorching of the sun, especially seeing that other creatures here, that live in the same clime and heat with them, have as florid blood as those that are in a cold climate.*

An Account of some Books. N° 117, p. 401.

I. Marcelli Malpighii Anatome Plantarum; cui subjungitur Appendix, iteratas et auctas ejusdem de Ovo Incubato Observationes continens. Lond. 1675, in fol.

Having given a full account of Dr. Grew's book on the anatomy of vegetables, at p. 660, vol. I. of our Abridgement; (between whom and this author there is a great coincidence), we shall only detail the more remarkable observations contained in Malpighi's work.

The author begins with the bark, and proceeds to the woody part, and the knobs, and so on to the fabric of the buds, blossoms, leaves, and seeds: promising at the end to prepare another work about the roots and excrescences of plants.

Concerning the bark, he finds it to be made up of several parts, of which the principal are those he calls ligneous fibres, in his opinion analogous to nerves, which he says are pipes pervious to a clear liquor; the structure of which pipes consist in many square partitions, opening into one another. These vessels he finds to lie neither straight nor parallel, but to be for the most part compacted like little faggots; of which some do make a kind of net-work, whereby the

* It is the rete mucosum, not the blood, which is the cause of the blackness of the complexion in negroes.

bark becomes to be an aggregate of reticular coats, surrounding the woody part of the plant: and as to what passes through them, he says, that the juice being entered into them, is, by the heat of the season striking upon the soil and forcing up the liquor, made to ascend a little way; and then by the survening night and cold stopped for the time, but is again, by the heat of the next day sending up more juice, thrust up higher from time to time, till it gets to the top, climbing thither as it were by steps: to which ascent it is marvellously assisted by the structure of these pipes, being divided into square partitions, opening into one another, and furnished with something that performs the part of valves, endued with a spring.

From these pipes, he says, do depend and break forth horizontal ranks of bags or bubbles, crossing those fibres; into which bubbles the ascending juice, like a chyle, is discharged, and being stayed there a while, and mixed with the old juice there residing, comes to be fermented and converted into aliment.

But besides this preparation of the aliment in the bark, there is another office, which that part seems to be appointed for; and that is the increase of the bulk of plants, by adding yearly a coat or ring of fibres, which being interwoven by the above-mentioned horizontal ranks of bubbles, and by degrees consolidated and hardened, do put on the nature of wood.

The stem or trunk of plants consists, according to him, of ligneous fibres, transverse ranks of bubbles, and air-pipes. In young trees, he says, the ranks of these bubbles pass into the very pith; which pith is abounding in young twigs, until by the growth and hardening of the ligneous fibres it wastes away.

The air-pipes, called also by him spiral fibres, are in his opinion, a kind of silver-coloured plate, wreathed spirally, and so constituting an open hollow pipe, of a scaly texture, made up of little pipes and bladders, very like the lungs of insects, admitting contraction and dilatation. Whence he concludes the great necessity and use of air and respiration in all those creatures that have even but the least degree of life: which air, he says, is in plants taken in chiefly by the roots out of the earth, there being no such conspicuous air-pipes in the bark or leaves, whereas the roots are exceedingly stored with them. This air, contained in these pipes, and subject to compression and rarefaction, presses by its swelling upon the contiguous woody fibres and their adhering bubbles, and so squeezes out their juice into the neighbouring parts; which being relaxed and emptied, they admit and take in fresh liquor.

Such plants, that instead of clear liquor contain in their fibrous pipes a coloured juice, have a peculiar vessel, as in the ebulus (dane-wort), fig. 30, and in all lactiferous and resinous plants, fig. 31. And each plant seems to our author to have a peculiar vessel to contain and prepare the last specific nourishment for that plant; such as those, that hold the turpentine and rosin in some trees; there being as many several sorts of juices as there are species of plants, and therefore peculiar vessels, preparing the last and proper juice for each respective plant.

The stems of trees and their branches increase by the external addition of a new coat of fibres and air-pipes, growing about them every year, and thereby giving them a new ring of wood.

As to the knobs of plants, they are to our author nothing but the productions of new offsprings upon a new implication of fibres and air-pipes, for the shooting out of new leaves, and young sprouts or buds.

A bud is, as it were, the new fœtus or birth of a plant, or a sprout contracted in small, inclosing a tender woody part, (raised from ligneous fibres and medullar bubbles) and the rudiments of the leaves; which being enlarged by the moisture and warmth of the spring, extend themselves into the form of a sprout.

The leaves are, to our author, a considerable part of the plant, seeing that all those parts, that are wrapped up in the stem or trunk, do, when opened in the extreme and younger parts, break out into leaves; so that these seem to be nothing but appendages to the trunk lengthened and opened; the ligneous pipes and air-vessels, derived from the midst of the woody cylinder of the tender ring, running together into a bundle, and forming the stalk, and at length upon their dilatation completing the leaf. The great variety of leaves our author deduces from the transverse ranks of bubbles appendant to the woody pipes of the stem, upon the opening of the stalk.

The office of the leaves seems to him very considerable, forasmuch as, in his opinion, they perform the part of the skin in animals. They (the leaves) are furnished with all the sorts of vessels to be met within the body of plants, as air-vessels, woody-fibres, and vessels of transpiration. This opinion of his, viz. that the nutritious juice is further concocted in the leaves, he endeavours to render more probable by the consideration of the little seminal plant, which contains two leaves; insinuating also, that from the leaves there is a regress of the concocted juice into the stem, and consequently a peculiar circulation.

Esteeming the branches to be produced for the generation of the vegetable egg, he teaches, conformably thereunto, that a blossom or flower is, as it were, the uterus together with the egg or fœtus of the plant, which in due season is exposed to the air, to make it grow at length into a new offspring. In explaining the manner of the production of flowers, and their variety, he is very curious; as he also is in that of seeds; which latter he observes to be lodged in divers cases or caskets, performing the office of an uterus and the parts thereof. And seeing the seed grows in very many plants to an edible fruit, he describes the structure and parts of several fruits, viz. figs, cherries, grapes, pears, citrons, lemons, oranges, &c. &c. taking notice of the singular apparatus, formed by nature for the sake of the seed, which he calls the fœtus and the true compendium of a plant, made up of all the principal parts thereof.

But his observations about galls, and other excrescences and appendages of trees, he reserves for another discourse, shortly noticing, however, that those excrescences are not the wombs, in and by which, trees and other plants produce insects; but only the nests of the egg cast there by the animal parent, and not at all furnished by the plant itself.

Concerning the uses of the several parts of plants above described, he offers his opinion, but with great diffidence, acknowledging it difficult not to be mistaken therein.

Thus far respecting this author's anatomy of plants: touching his appendix of incubated eggs, he therein shows, with what care he has repeated his former observations upon that subject; though he still scruples to determine, which of these two, the heart or the blood, has the priority of existence in the formation of a chick: this only being certain to him, that there may be observed the first lineaments of the chick even before incubation, and that afterwards, by virtue of the incubation, there are first manifested the vertebræ, and the beginnings of the brain and the spinal marrow, together with the wings, and some flesh; the heart, vessels, and blood, yet lying then concealed: but yet, because that some rivulets do appear in the umbilical area, he thinks it probable, that the heart is then also appendant to the carina of the chick, he having seen the heart before the 30th hour: but it is a considerable time, he says, before the liquor passes through the heart and the vessels; which liquor he has observed to be first of a yellowish, then of a ruddy, and at last of a blood-red colour. Whence he again offers his conjecture, that the liquor, the vessels and the heart do exist before the blood.

II. Epistola ad D. Joelem Langelotum de Alcali et Acidi Insufficientia ad gerendum munus Principiorum Corporum naturalium: Conscripta à Joh. Bohn, Phil. et Med. D. in Acad. Lipsiensi. Lip. Anno 1675, in 8vo.

A chemical dissertation to disprove (what none will now maintain) the sufficiency of that doctrine, which ascribes the production of the principal phænomena in nature to the congress and conflict of two principles, an acid and an alkali.

III. Zymologia Chymica, or a Philosophical Discourse of Fermentation, from a New Hypothesis of Acidum and Sulphur; with an additional Discourse of the Sulphur Bath at Knaresbrough: by W. Sympson, M. D. Lond. 1675, in 8vo.

In the chemical writings of Dr. Sympson, we have before had occasion to remark, there is nothing worthy of notice. [Concerning the Harrowgate, or Knaresborough mineral waters, see the Bishop of Llandaff's observations, in the 76th vol. of the Phil. Trans. Great pains were bestowed upon their analysis by the late Dr. Garnett, who published a treatise upon them in 1794.]

A summary Relation of the Attempts made for a North-East passage. N° 118, p. 417.

It is sufficiently known to those who have made any inspection into the navigation of this and the former age, how solicitously the States of the United Provinces have laboured to encourage those, who should first discover a more compendious and shorter passage by the north, to China, Japan, and other eastern countries. But those who first ventured on this enterprise, found by sad experience, that the success did not answer their expectation and hopes.

Those who immediately succeeded them in that adventure, were not much more successful; for treading the same steps that the former had done, they were involved in the same difficulties; being misled by an opinion, that that part of the sea, which lies between Nova-zembla and the continent of Tartary, was passable, and that they might sail through that to China. But it is now well known to the Muscovites and others, that Nova-zembla is no island, but a part of Tartary; to which it is annexed on the east by a large neck of land, that the arm of sea, into which there is a passage through the Weigath-straits, is not really sea, but a lake of fresh water; the great abundance of rivers, which out of Asia empty themselves into this gulf, causing this freshness; so that it is not to be counted strange, if, especially in the winter season, these waters are strongly frozen.

Nor is it to be wondered at, that the navigation of William Barentz, otherwise an experienced mariner, was unsuccessful, who passed along the coast of Nova-zembla, as far as the 77th deg. of N. latitude: for it is well known, that most of those northern coasts are frozen up many leagues; though in the open sea it is not so; no nor under the pole itself, unless by accident, as when on the approach of summer, the frost breaks, and the ice which was near 40 or 50 leagues off the shore, breaks off from the land and floats up and down in the sea. These prodigious floats of ice were the chief obstruction to those that directed their course somewhat more to the north.

Some years since, certain merchants of Amsterdam attempted those seas with much better success than the former. Having advanced to the 79th or 80th deg. of northern latitude, they passed above a hundred leagues to the east of Nova-zembla. These being returned to their own country, with great hopes of finding encouragement to make further discoveries, petitioned the States General that they would be pleased to grant the navigation of the northern seas, and of the eastern, not yet discovered, to them.—But the governors of the East India Company, being sensible how nearly this concerned them, presented a counter petition, desiring that the petition of the said merchants might for the future be referred to them and their consideration.—The merchants finding their petitions thus crossed, they addressed themselves to the King of Denmark, who immediately granted their demands. Under his protection therefore they equipped two or three ships, such as they judged most proper for this voyage. On which the governor of the Dutch East India Company raised a considerable sum of money, and easily persuaded the mariners to desist from so dangerous a voyage, as they represented it; and that the merchants might have no just cause to complain of the said company, the mariners went to sea; but neglecting the directions and orders of those merchants, they steered their course directly for Spitzberg, took in their lading of fish, and returned home.

Upon which the East India Company omitted nothing to find out a passage through the north-eastern sea, for those who were to return into Europe from the East Indies.—There was then much discourse of the Gulf of Anin, by which a passage was said to be open into the Tartarian Sea: and they had some hints from the people of Japan and the Portuguese, about the country of Jezzo, which lay above Japan. But not resting satisfied with the bare relation, in the years 1652 and 1653, they sent out some skilful persons to discover those coasts; who passing beyond Japan, the 50th degree of N. latitude, arrived on the coast of Jezzo, where they fell into a narrow sea, yet broad and convenient enough to lead into the Northern Ocean. The opposite shores they called Het Compagnie land, and an island seated in the middle of the gulf they called Het Staten Eyland.—Whether this land of Jezzo be annexed to Japan or not, the inhabitants of both countries doubt; because vast and inaccessible mountains interpose, which hinder the communication. Neither does it as yet clearly appear, whether this land of Jezzo be a part of Tartary, or whether by an arm of the sea divided from it. The Chinese affirm, that Tartary runs 300 China leagues eastward beyond their famous wall: so that if we follow these, the country of Jezzo and Japan may seem to be annexed to Tartary; but those of Jezzo say, that there runs an arm of the sea between them and Tartary: which opinion may seem to receive some confirmation from what those Hollanders affirm, who were shipwrecked some years since on Corea, a peninusula of China, where they saw a whale, upon whose back stuck a harping-iron of Gascony. It is therefore most probable, that this whale passed from Spitzberg through the nearest arm of the sea, rather than through the more remote.—After the experiments made by the governors of the East India Company, in the years 1652 and 53, they resolved to proceed no further on the discovery; as well because the Emperor of Japan interdicted the navigation of foreigners into Jezzo, in regard, as they say, of the vast tribute which he annually raises from the silver mines there; as because they thought it may little conduce to their advantage, to have this compendious way of navigation discovered. And therefore they have thought fit to prohibit all further search into the navigation to Jezzo, and the adjacent countries; for which very reason they have also endeavoured to conceal their Austral plantations.

Now concerning that tract or space which lies between Spitzberg, Nova-zembla, and the Straits of Jezzo, we have no reason to entertain any doubt; especially as many of the Muscovite itineraries assure us, that the coast of Tartary runs not northward from Nova-zembla, but turns very much towards the east; so that the head land of Nova-zembla is far the most northern part of all Tartary.

It remains now to inquire by what course, and in what season of the year, this voyage is best to be undertaken? It is hardly to be doubted, but that the strait which lies between Spitzberg and Nova-zembla may be passed; and the course to be directed to 78, 79, or 80 degrees of north latitude. If any shall proceed farther in the same work, he will find the passage shorter; for drawing a line from our seas through the 78th or 79th degree of latitude, to the Strait of Jezzo, it will be very near a straight line: but if any would, from the same degree of latitude, having passed Nova-zembla, choose to steer toward the coast of Tartary, and coast along by it, till he meet with some strait, he would find his course somewhat longer, but perhaps safer and better.

As to the time of the year, wherein this navigation ought to begin; it may be in the beginning of the spring, viz. in the month of March, when it is confessed by most men, that the winds and seas are favourable to those that sail to Spitzberg, and the places near the pole; and that they may run all that course from these parts in 12 or 13 days: but when they have passed so far, if any man would design to sail to the Straits of Jezzo, he must steer his course towards the south. But then those motions of the winds and seas, which were favourable to those who sailed northward, will be contrary to those who stand southward; and they may long enough expect northern gales, which seldom blow till towards the latter end of summer, viz. in the month of August. If therefore any man would contrive to dispatch his voyage in the shortest time, it would be safest to make choice of that time of the year, where he might soonest make Spitzberg and return again, which might be in the beginning of summer: yet it would be safer to set out sooner, if the wind permit. And although this course should happily succeed, it follows not that I should advise them to observe the same in their return homeward; for things of that nature must be left to the prudence and conduct of discreet pilots and mariners, who ought to shun all near approach to the coasts and islands which they shall encounter, for fear of the ice; and that they always make choice of the most open seas, which are least infested with it, and in which the colds are more moderate. For experience has sufficiently taught, that whole large seas are never known to be frozen, but only the sea coasts, from the plenty of fresh waters that run into the ocean, and the snows melted in it. And the same experience has taught, that there is not that danger from the floating ice, as is vulgarly apprehended, especially in seas not subject to violent storms, and in the 6th or rather the 8th month of the year.

When the nature of this sea, and of its several straits shall be more perfectly discovered, it is not to be doubted but that the whole voyage to Japan may be performed in five or six weeks at the most. But in case it should happen, that the ships should be forced to winter there, this might be done without much danger; provided they avoided the unadvised example of the Dutch, who being necessitated to pass the winter in the most northern climates, planted themselves there upon the highest lands, in huts framed of thin boards; whereas they ought to sink their houses under ground, and to heap much earth over them; since it is scarcely possible for men to subsist in such an excessive severity of winter, unless they shelter themselves deep under the earth.

Extract of a Letter from a Spanish Professor of Mathematics, proposing a New place for the first Meridian, and pretending to evince the Equality of all Natural Days, as also to show a Way of knowing the true Place of the Moon. With an Answer to the same by Mr. Flamsteed. N° 118, p. 425, &c.

In this letter, the Spanish professor pleads for the propriety and convenience of all nations adopting some common place, as a first meridian, from whence all nations might count their longitudes: a thing which never could be accomplished; but, instead of which, all astronomers have, and probably always will, compute the longitude from the meridian of their own observatories.—He next would persuade us, from observations made with his own pendulum clock, that all the natural days are of equal length in all seasons throughout the year, and consequently that there is no such thing as the equation of time. This is easily refuted by Mr. Flamsteed, who proves the inequality of the days, and the equation of time, both on account of the obliquity of the ecliptic, and of the earth's unequal motion in her elliptical orbit. Nor does Mr. Flamsteed think more favourably of the professor's instruments, or lunar observations, which he shows are very erroneous.

An Account of two Books. N° 118, p. 435.

I. Jacobi Barneri, Ph. et Med. D. Augustani, Prodromus Sennerti* Novi, seu Delineatio Novi Medicinæ Systematis, &c. Augustæ Vindelicorum. An. 1674, in 4to.

The works of Sennertus being in the library of most physicians, it would be superfluous to insert an account of his system here.

II. A Description of Helioscopes, and some other Instruments, made by Robert Hook, F. R. S. London, 1675, in 4to.

Of the contents of this book, as far as they relate to the instruments therein described, I need say nothing here: I shall only touch upon some passages in the postscript, in which I find one of our tracts concerned. The postscript takes the liberty of reflecting on a passage in N° 112 of these Transactions, viz. about the invention of applying a spring to the balance of a watch; finding fault with the same for not having noticed, that "this invention was first found out by an Englishman, and long since published to the world," and complaining thereupon of "unhandsome proceedings."

As the former part of this accusation directly concerns the editor of the Transactions, and the latter is so ambiguously worded, as that it may be referred to the said author, as well as to the French Journal des Scavans, it was thought fit to acquaint the impartial and candid reader with the plain truth of this matter.

It is certain then, that the describer of the helioscope, some years ago, caused to be actually made some watches of this kind, yet without publishing to the world a description of the same in print; but it is as certain that none of those watches succeeded, and that nothing was done since to mend the invention, and to render it useful, that we know of, until M. Huygens sent hither a letter, dated Jan. 30, $16\frac{74}{75}$, acquainting us with an invention of his of

* This celebrated teacher of physic, who, from being the son of a shoemaker, came to be rector of the university of Wittemberg, and physician to the Elector of Saxony, was born at Breslaw in 1572, and died at Wittemberg in 1637, aged 65, during the prevalence of a pestilential epidemic of which he caught the infection. His collected works amount to 2 volumes folio. They exhibit a view of the opinions and observations of philosophical and medical writers from the days of Hippocrates and Aristotle unto the beginning of the 17th century. There is more of erudition than of originality in his works, and some will doubtless think that he occupied himself too much with the doctrines of the ancients. He promoted, however, the cultivation of chemistry in Germany, being the first who read lectures on that branch of experimental knowledge in the university of Wittemberg. Among the practical dissertations of Sennertus, those on fevers are esteemed the best.

very exact pocket watches, the nature and contrivance of which he imparted to us, as he used to do other inventions of his, in an anagram; which he soon after in a letter of Feb. 20, $16\frac{74}{75}$, explained to us by a full description; for which the Royal Society thought fit to return him thanks, yet so as to intimate to him, that Mr. Hook had some years ago invented a watch of the like contrivance.

Not long after this there came over, in the Journal des Scavans, a printed description of M. Huygens's invention, with a delineation of the figure of the same, which the editor of the Transactions produced at the public meeting of the Royal Society; where M. Hook not only saw it but took a copy of the figure itself at the same time, unwilling to let the person who presented it there take it home without permitting him first to copy it. Which done, M. Huygens's explanation of his own way, with the figure of it, was, at his desire, printed the 12th of March $16\frac{74}{75}$, in N° 112 of the Transactions; the describer of the helioscope well knowing, that it was designed to be published in one of those Tracts; who, if he had given to the author of them the least intimation, importing that he desired notice might be taken at the same time of his invention of the like kind, it would have been certainly done, as has been done on other occasions, witness several of the same tracts, wherein divers discoveries of this accuser have been formerly both printed, and vindicated from the usurpation of others; though indeed it was not necessary it should there be done now, since the said animadversor could speak for himself in print as soon as he pleased, as having laudably made use of late of the press for publishing himself his own inventions.

This is the plain truth of the matter, in which, whether there be any thing on the part of the editor of the Transactions, that deserves that name of "unhandsome proceedings," he very willingly leaves to all ingenuous readers to judge: besides it might justly be considered, that pregnant and inventive heads, well versed in mathematics and mechanics, and furnished with a genuine method of investigation, may, and often do, fall upon the same discoveries and inventions about the same time, especially if their minds have been long addicted to and engaged in the same researches: of which, if there be occasion, several considerable instances may be produced to verify the assertion. One of which, and fit to be noticed at present, is, that when the editor of the Transactions lately showed to the accuser that way of M. Leibnitz, concerning exact portable watches, which was printed in N° 113 of these Tracts, he acknowledged that though he had known that way too, ever since An. 1660, yet he never declared it to any body, and therefore could not say M. Leibnitz had taken it from him.

Thus I shall dismiss him, not doubting, but that all candid readers will blame him for the expression he uses p. 30 of his said postscript, which is, that he "forbears now to mention any further the carriage of the writer of the Transactions in this affair:" and only adding, that if this writer of mechanics shall think fit to explain what he means by it, he will certainly meet with a full answer, vindicating the integrity of the editor in such a manner, that all impartial and good men shall be abundantly satisfied with it.

*Pneumatical Experiments made with the Air-pump. By M. Huygens and M. Papin.** N° 119, p. 443.

To mix several liquors together by means of the air-pump, two small glasses were employed, one of which could enter into the other, and the least of them was fastened to the hook of an iron-wire, passing through a small hole in the top of the receiver, and tied close about with an eel-skin; and the larger glass set under it; and the said wire was so ordered till the recipient was exhausted. Then, by means of the iron wire, the small glass was let down into the larger, till the liquors they contain mixed themselves. Thus, some aquafortis was poured into the upper glass, and spirit of wine into the lower, and the receiver was so well exhausted of the air, that the spirit of wine boiled up with large bubbles, and the aquafortis emitted some small bubbles. After both these liquors were well purged of air, the upper glass was sunk into the lower, so that the spirit of wine was mingled with the aquafortis; when there was yet seen a very considerable ebullition.

Now, in order to know whether the aquafortis imparted to the spirit of wine some new vigour, or force, to make it bubble; some aquafortis was mixed with spirit of wine, without the receiver; the quantity of the former being somewhat more than that of the latter. This mixture being put in vacuo, instead of boiling up more strongly than the spirit of wine, as it was thought it would have done, it only cast up some few bubbles: which showed that the ebullition which was seen when they were mixed within the vacuum is of the same nature with all those that are made of acids and alcalies. For in the very instant of mixture, they make great ebullitions, but soon after they destroy each other, and lose the properties they had before. It is also probable, that the aquafortis and the spirit of wine would boil always when they are mingled, were it not that the pressure of the air prevents this ebullition from being sensible, and appears only when that pressure is removed.

It was also found, that the solution of common salt boils when mixed in vacuo

* This was Nicholas Papin, author of the celebrated digester, for making soup of bones, and of some ingenious books, as well as several papers inserted in the Philosophical Transactions, volume 10, 15, 16, 24. His uncle Nicholas, and his cousin Isaac, were also learned French philosophers.

with spirit of wine, and the solution of saltpetre still more. The same experiment was also made with common water, mixed in vacuo with aquavitæ purged of air, when the ebullition was also found to be very great. It is somewhat remarkable, that common water mixed with aquavitæ, and put within the vacuum, bubbles up very well, though the former be there in greater quantity than the latter; whereas, a mixture of aquafortis and aquavitæ did not there bubble up at all.

In order to know whether these ebullitions produced any new air; a gauge was put into the receiver, which is a glass tube, filled either with water freed of air or with mercury, 4 inches long; and it was observed, that when the liquors were mixed together, the water in the gauge rose very nimbly to the top; and then exhausting this new air, the gauge, water subsided again by degrees, in like manner as when the common air is drawn out: and thus it was found, that all these kinds of ebullition produce an air which expands itself like common air.*

It seems remarkable, that the air produced by these ebullitions, is not all of the same nature. For it has been found experimentally, that the air formed by

* On this occasion it may be noticed, as it is recorded in the journal book of the Royal Society, An. 1668, April 30, that Mr. Boyle gave an account to the said Society of the experiments he had then made about generating new air, or extricating that air which was lurking before in several bodies: at which time he mentioned also some ways of examining whether the substance thus produced be true air or not. And long before that time, viz. An. 1664, the 15th of March, as appears by the same journal, Mr. Boyle mentioned to the Royal Society that corals or oyster shells pounded, and put into distilled vinegar, might prove fit substances to produce air wholesome for breathing. At which time he also proposed, that some fit animal might be put into a receiver of his exhausting engine, and the air pumped out till the creature grew sickish; and that then some new air might be produced in the receiver by a contrivance of making distilled vinegar work upon the substances before-mentioned; to see whether by this means the animal would recover.

About the same time Sir Christopher Wren also suggested, to put a fermenting liquor in a glass ball, and to fit a stop-cock to it, and tie a bladder about the top of the stop-cock, by which means the air, to be generated by the fermenting liquor, would pass into the bladder, and on the turning of the stop-cock be kept there in the form of air. Mr. Hook also mentioned several liquors, which by their working on one another would produce an air; as oil of tartar and vitriol; spirit of wine and turpentine. And he made before the Royal Society the following experiment: He took a common glass phial with two pipes, and some pounded oyster shells and aquafortis; and as soon as the latter was by one of those pipes poured on the former, and the hole stopped with good cement, the ebullition caused by the shells being corroded by the aquafortis, in a very little time blew up the bladder, tied on to the other pipe, so as to swell it very plump with air; which expansion remained till the Society rose, after they had ordered the vessel to be carefully locked up till their next meeting, which being the week after, the bladder was then found somewhat shrunk. The like experiment was made with bottled ale, supposed to yield a more wholesome air for respiration, &c.—Orig.†

† The elastic fluid extricated in the instances mentioned in this note is not common or atmospheric air, but fixed air or carbonic acid gas; a species of air unsuited to the purposes of respiration, and even deleterious to animals that are placed in an atmosphere of it and compelled to breathe it, though but for a short space of time.

the mixture of aquafortis and copper,* remains always air, and always keeps up the water in the glass at that height to which it first raised it; but on the contrary, the air produced by the mixture of oil of tartar and oil of vitriol,† almost all vanishes of itself in 24 hours.—On mixing equal parts of aquafortis and aquavitæ, and putting two equal quantities of the mixture in two small glasses, with two equal bits of iron, one in each, and including one of the glasses in vacuo, there was seen a very great ebullition, and the liquor became black; whilst the other glass that was left without the receiver, made no ebullition, but remained always transparent, or rather white than black. After these two glasses had stood 12 hours, the iron in the glass in vacuo was almost all dissolved, whereas the other was very little diminished. This experiment succeeds quite the contrary when made with aquafortis alone and copper; for then the solution is less within the vacuum than without it.

Oil of olives makes no ebullition, neither with vinegar nor with spirit of wine, when they are just mixed, either in vacuo or out of it. But having mixed together without the receiver, some of that oil and vinegar and spirit of wine, and put this mixture in vacuo, it did not boil up so soon as when there was no oil; but then the bubbles, which it afterwards made were larger, and they began to appear again from time to time, so that some of them were seen a quarter of an hour after the receiver had been exhausted. Possibly this may be owing to the oil swimming on the top, retaining the more volatile parts of the spirit of wine, which would otherwise fly away as soon as the air is begun to be pumped out, and at the same time hinders the surface of the liquor below from easily rising up into bubbles; because to make them do so, the parts of the oil that stick close to each other must be separated. When therefore the volatile parts are collected in a sufficient quantity, able to surmount the resistance which the oil makes to it, they issue out with greater violence than if nothing had detained them.

All these ebullitions hitherto spoken of, are greater in vacuo than in the open air: but it is not so with lime. For taking two equal glasses with two equal quantities of water, and putting the one of them in vacuo, the other in the open air, there was let fall into both at the same time two equal parcels of lime, one into each; and it appeared, that that which was in vacuo did indeed throw up some large bubbles, but yet fewer of them than that which was in the air: and having taken it an hour after out of the receiver, and stirred the lime,

* The aëriform body thus produced by the action of aquafortis on copper, was *nitrous gas*. It appears, therefore, that the discovery of this species of permanently elastic fluid is due to the authors of the above related experiments, to which Mr. Boyle's previous pneumatical inquiries had served as a clue.

† Fixed air or carbonic acid gas.

it was found to have only the consistence of dirt, whereas the other had the consistence of slacked lime. The reason of which may perhaps be, that the volatile salts of the lime may exhale while the receiver is exhausting.

Some plaster of Paris was also slacked in vacuo, and the ebullition of it there appeared much more than in the open air. When it is not touched, the bubbles that issue out leave large holes in it, and then it settles very uneven; but taking care to stir it till the bubbles escape, and pressing it when it begins to settle, it becomes very smooth, and has fewer little holes than the common plaster.

An Account given by a French Author, (Denys Papin) in his Book on the Origin of Fountains, printed 1674, *at Paris*; *to show that the Rain and Snow-waters are sufficient to make Fountains and Rivers run perpetually.* N° 119, p. 447.

In order to give a gross estimate of the quantity of rain,* compared with the quantity of water running away in springs and rivers; it will be first necessary to agree on the way of measuring these two sorts of water. Some persons say, that a cubic inch of water yields, in 24 hours running, 144 muids, (the name of a French measure, holding 280 French pints;) others say it yields but 70 of that measure. But I have reason to believe that it yields 83 of this measure; and it is known that a vessel of two feet deep, long and broad, holds one muid of water.

This being supposed; it follows, that a vessel which contains 83 muids of water, is able to furnish in 24 hours as much as will make an inch of water run continually. So that, if a conservatory should hold 3378 muids of water, it would furnish for a whole year a sufficient quantity to make an inch of water run constantly. And if it were as large again, it would furnish two running, and so on in proportion. Then for the measure of the rain and snow water; I have found that from October 1668, to October 1669, there had fallen so much of it, as amounted to the height of 18 inches 7 lines; and from the same month of 1670 to the same month 1671, there had fallen only so much as came to the height of 8½ inches; and from January 1673 to January 1674, to the height of 27½ inches. Of which, taking the medium, we have 19 inches and

* The like to which has been attempted here, and proposed to the Royal Society some years since, by Sir Christopher Wren, who, by the contrivance of a rain-bucket, had taken an account of all the water that fell for a considerable time; and by his weather-clock had, among other particulars, not only taken in the measuring of the quantity of rain that falls, but also the time when it falls, and how much at each time. Which instrument, if put into practice, would be of excellent use; forasmuch as it may also serve, by some additions made to it by M. Hook, to record the weight of the air, the drought, moisture, heat and cold of the weather, the sun-shine, the quarters and strength of the winds: and all this to be performed by only one motion, driving all the parts of the instrument; which is therefore the more considerable, that of itself it records its own effects.—Orig.

$2\frac{1}{3}$ lines. This supposed, we must for our purpose measure, or make an estimate, of some river, as it runs from its very source to a place where some rivulet enters into it, and see whether the rain water that falls about its course, if it were put into a large reservoir, would be sufficient to make it run a whole year.

I have observed the River Seine, in its course from the source of it unto Ainay le Duc, where a rivulet enters that swells it. And this I shall take for the subject of the examination I intend to make.—The course then of this river, from its spring to the said Ainay le Duc, is about three leagues long, and the sides of its course extend themselves on the right and left about two leagues on each side, where there are other little rivers that run another way: and since these rivulets require water to maintain them, as well as the Seine, I shall count but half that space of the sides, and say, that the place where the Seine passes, has, from its source to Ainay le Duc, three miles long, and two miles broad. So that, if a reservoir were made of this size, it would be 6 leagues square in surface, which being reduced to fathoms, it would according to the measure abovementioned, make $31\frac{1}{4}$ millions of fathoms in surface. In this conservatory imagine, that during a whole year, there has fallen rain to the height of 19 inches $2\frac{1}{3}$ lines, as before said; this height of 19 inches and $2\frac{1}{3}$ lines gives nearly 281 millions of muids of water.—All this water thus collected, in the quantity just now expressed, is that stock which is to serve to make the river run for a whole year, from its source to the place before named, and which must also serve to supply other occasions and losses, such as are the feeding of trees, herbs, vapours, and extraordinary swellings of the river while it rains, and the deviations of the water running another way.

Concerning the measure or estimate of the water of this rising river, it would be difficult to determine it precisely, as well as what quantity it furnishes. Yet, so far as I was able to judge, it can have no more than 1000 or 1200 inches of water always running, compensating the less quantity it has at its source with the greater it has towards Ainay le Duc: which I so judge by the comparison I make of these waters with those of the river of the Gobelins, in the state of it near Versailles, where it has 50 inches of water, according to the measure taken of it. So that I judge it will be enough to allow 24 or 25 times as much to our river. For the channel of it is to be 4 or 5 fathoms broad, and is but shallow.

These particulars being thus supposed, then, according to the measures agreed on, 1200 inches of water furnish in 24 hours, after the rate of 83 muids of water to an inch, 99600 muids of water; hence, in a whole year, which is near 366 times as much, they will furnish near $36\frac{1}{2}$ millions of muids. This river then sends away within its banks in a year, no more than about $36\frac{1}{2}$

millions of muids of water. But taking this quantity out of the 281 millions that are in the conservatory above described, there will remain yet above 188* millions of muids, which amounts to almost five times as much, and which serves to furnish for the losses, diminutions, and other wastes, above noticed. So that there needs but the sixth part of the rain and snow-water that falls in a year, to run continually through the whole year.

On Damps in Mines. By Mr. Jessop. N° 119, p. 450.

Damps are most generally observed to come about the latter end of May, and to continue during the heat of summer; and in those places which have damps all the year long, yet they observe them to be most violent at that season.—I never heard of damps that kindled of themselves, although I have been told, that in some places they have been kindled by the motion of the sled, in which they draw their coals.—Damps generally are held to be heavier than the air; but this was manifestly lighter, for it lay towards the top of the bink.—On the breaking of the fulminating damp there proceeds a dark smoke of the smell and colour of that which proceeds from fired gunpowder.

Some damps will quite extinguish all those fires that are let down into them, be they never so many successively, or never so great; and fire is observed to be so far from curing, that it often creates damps in places not otherwise subject to them. Indeed they are a present remedy, if by their means a circulation of air through the infected place can be made, otherwise they do hurt; and those grooves in which they are forced to break their rocks by the help of great fires, are seldom free from damps.—Damps are common both in dry and wet grounds.—Damps are observed to be most pestilential, and to kill the soonest, that are in grooves not stirred for many years; especially if such grooves have formerly had great fires in them.

The general opinion of the workmen is, that there are some damps which kill by reason of the noisome steam. For they say there is no groove that wants air, be it never so deep; but the air stagnating in very deep grooves or pits, the grosser parts must needs at length separate themselves by their own weight, and subsiding to the bottom, there corrupt, and consequently get malignant qualities, especially in the summer time, when the sun promotes the fermentation.—Damps will often follow the water, and particularly this sort of fiery damp.†

* There is some mistake in these numbers, as 244½ would remain.

† Concerning the nature of these damps, see volume I, p. 16, of this Abridgement.

An Account of some Books. N° 119, p. 454.

I. A Philosophical Discourse of Earth, relating to the Improvement of it for Vegetation and the Propagaton of Plants. By J. Evelyn, Esq. F.R.S. Lond. An. 1676, in octavo.

The author first describes what he means by earth; then endeavours to show the several sorts and kinds of earth, as they reside in their several beds, with the indications, by which we may discover their qualities and perfections; and lastly, how we may best improve it to the uses of the husbandman, the forester, and the gardener.—In the second part, he not only notices the fitness of our senses in giving their verdict of the several qualities of earths; but also acquaints us with the microscopical examination he has made of divers sorts, both of earth and soils or dungs.—In the third part he teaches first, how we may improve the best earths, and apply remedy to the worst, only by labour, stirring, ventilating, shading and reposing; which being the least artificial, approach the nearest to nature: where he notes among many other excellent particulars, that the bare raking and combing only of a bed of earth, different ways, may diversify the annual production: commending irrigation, or watering, as one of the richest improvements that ever was put in practice, especially where fat and impregnated waters may be had without grittiness, or being over harsh and cold.

Secondly, he delivers what farther advancement may be expected from stercoration or manuring the ground with composts; discovering the qualities latent in their several ferments, and how to apply them by a skilful and philosophical hand, without which they do more hurt than good.

II. A Description of the Islands and Inhabitants of Feroë, &c. written in Danish, by Lucas Jacobson Debes, M.A. and Provost of the Churches there. Translated by J.S. Doct. of Phys. in 12mo.

The islands described in this book are 17 in number, subject to the King of Denmark, lying under 62 deg. 10m. of north latitude: concerning which, the describer gives an account of the fertility of their lands and waters, and of the inhabitants, besides their government, churches, schools, &c.

Among the observables of these islands, are the following: A dangerous whirlpool near one of them, called Monk; which is most mischievous to ships in still weather, but avoidable with a fresh gale of wind.—The ordinary declination of the needle on Feroë, is by our author affirmed to have been 13° 13′ to the north-west, An. 1659.—The tides are strongest here, three days before, and three days after, the new and full moon; and a north-east and a south-west moon, make highest water in all the principal streams of these islands.—The famous whirlpool, or sea-gulf, under Norway, called the Maalstrom, is by Kircher and others erroneously said to run down under the land of Norway, and run out again at another sea-gulf within the Sinus Bothnicus, as this author undertakes to prove.

In Feroë there are no trees, but only some shrubs of juniper. Abundance of turf compensates that defect.

On the sand near the sea side is found in some places a kind of pellucid stones, so hard that with them you may write on glass: they are white, or of a bluish white; others yellow: some of them may be so well polished, that they serve for rings.—No grain will come to maturity there but barley. They abound in pastures; and in several places grass is so plentiful and juicy, that oxen feed on it both winter and summer, growing sometimes so fat, that one ox yields a 100 pounds of tallow: which rich pastures, our author observes, always lie to the north-east and north; as he notes also, that in Iceland the north part is more fertile in grass and cattle than the south; and that Greenland likewise is found to be much more given to grass on the north-east side, than on the west side.—The ground is manured with sea-weeds, laid on heaps to rot; by which they get good crops of barley.—Their plants are, turnips, carrots, coleworts, lettuce, cresses, penny-royal, scurvy-grass, beccabunga, sorrel, angelica, tormentil and radix rhodia.—When extraordinary snow falls, and shepherds are not present to drive their sheep under shelters, the sheep assemble close together; and the snow so covering them, that they cannot be seen for a while; at last the countryman perceives a damp arising from the snow by reason of their warmth, and so goes and makes a passage for them to get out. Sometimes, when they cannot be found by reason of excessive snow, they will remain a whole month under the snow, eating the grass by the roots, and the wool off one another. Their sheep, generally, are white in the north part, but black in the south; and being brought white from the north to the south, they will change colour; yet so as to grow first spotted about their legs, then on their thighs, then under their bellies, and at last all over.

III. The Gentleman's Recreation in four Parts; viz. Hunting, Hawking, Fowling, Fishing. Collected from Ancient and Modern Authors, Foreign and Domestic, and rectified by the Experience of the most skilful Artists of these times. Lond. in octavo, 1674.

Experiments about the weakened Spring, and some unobserved Effects of the Air. By the Hon. Robert Boyle. N° 120, p. 467.

Exper. I. Having put filings of crude copper into a crystalline glass of a conical shape, into which was poured some strong spirit of salt, to the height of about a finger's breadth above the filings; and then closing the vessel with a glass-stopper well fitted to it, we suffered it to continue unmoved in a window for some days, till the liquor had both obtained a high and darkish brown colour by the solution of some of the copper, and lost that colour again, growing clear like common water, and then taking out the stopper, without shaking the liquor, thereby giving access to the outward air, the upper surface of the liquor in a few minutes resumed a darkish brown colour, which penetrating deeper and deeper, at the end of about a quarter of an hour the whole body of the liquor appeared to be likewise tinged. The conical glass being again well stopped, the menstruum again, in very few days, lost its tincture; which, the stopper being taken out, it regained as before. Afterwards, keeping the glass in the same place with the same filings and menstruum in it, for a month or two together, the liquor would not any more grow clear.

Exper. II. Having taken another such glass, wherein the liquor was grown clearer than is usual. We took out the stopple, and left it open for about half an hour, but did not perceive the liquor to have acquired any colour, not even at the top. But putting in the stopple, and leaving the vessel closed for two or three hours, it acquired a faint colour, tending to a green; wherefore, taking out the stopple again, and leaving the glass unstopped for 20 or 24 hours, in all that time it had not regained its wonted dark colour, but was only arrived at a green, deep enough, but neither true nor very transparent.

Exper. III. Some strong spirit of salt having been kept upon filings of copper till the solution was become of a dark brown colour, about three spoonfuls of it was put into a receiver that might hold eight or ten times as much: being kept in vacuo about half a year, it retained its colour. But the vessel being opened, and the external air admitted, the solution in about an hour was turned into a fine transparent green, though no precipitation of any muddy substance appeared by any sediment to be made.

Exper. IV. On putting into another conical glass some filings of copper, with a convenient quantity of spirit of salt; after a few weeks it lost its muddy tincture, and appeared like common water. On unstopping the glass, to give free access to the outward air, we observed, that in some hours its operation on the liquor was scarcely sensible, but within about 24 hours the menstruum had acquired, not just its former colour, but a somewhat faint and moderately transparent green: so that this tinged menstruum, as it had been very slow in losing its colour, so it did but slowly and imperfectly regain it.

Exper. V. We put some filings of copper, with a mercurial gauge, in a conical glass, fitted with a ground stopple, and poured on the filings rectified spirit of fermented urine made per se, to the height of an inch or better above them; then carefully stopping the glass several hours after, the mercury in the sealed leg was considerably depressed; and gently drawing out the stopple, to let in the air, we perceived it to have a manifest effect on the mercury.

Exper. VI. On a proper quantity of clean filings of good copper, we poured strong spirit of fermented, or rather putrified urine, to the height of an inch above the copper; and having let down a mercurial gauge, so that it leaned on the bottom and side of the glass, we closed it very well with a stopple, and set it in a quiet and lightsome place, noticing at what mark the quicksilver rested in the open leg of the gauge. This done, suffering the menstruum to work upon the filings, which it did rather slowly and very calmly without producing any noise or sensible bubbles, it acquiring by degrees a very pleasant blue colour, which by degrees grew fainter and fainter, till at the end of three or four days the liquor was grown very pale. On taking out the stopple, that the air without the glass might have access to that within, within four or five minutes, if not less, the upper part of the liquor, that was contiguous to the air, had acquired a fine blue colour, which descended deeper and deeper; so that in less than a quarter of an hour from the first unstopping of the phial, the liquor was become throughout of a rich blue colour, and which in a few minutes longer turned opaque. Carefully closing the phial again with the same stopple as before, the liquor began again, in two or three days, to lose of its colour. In most of these experiments I forbore to shake the glass, lest it should be suspected, that the agitation of the liquor might have raised some little fine powder, that might have been supposed to have been precipitated from the tincture, and, being thus mixed with the liquor again, restore it to its former colour.

Exper. VII. On covering the bottom of a conical glass with a convenient quantity of filings of good copper, we poured on them as much strong spirit of sal ammoniac as served to swim about a finger's breadth above them; and having let down a mercurial gauge, and proceeded in all respects as in the foregoing experiment; without producing any noise or sensible bubbles, acquiring by degrees a very pleasant blue colour, and afforded us also the phenomenon we chiefly looked after; which was, that for two or three days together the mercury in the sealed leg of the gauge descended till it appeared to be near a quarter of an inch lower than at first; by which it appeared, that the spring of the air contained in the cavity of the glass, and communicating with that in the open leg of the gauge or syphon, was weakened in comparison of that in the closed leg.

Exper. VIII. A mercurial gauge having been put into a conical glass whose bottom was covered with beaten coral, some spirit of vinegar was poured in, and then the glass stopple closing the neck exactly, on the working of the menstruum on the coral, store of bubbles were for a good while produced, which successively broke in the cavity of the vessel, and their accession compressed the confined air, in the closed leg of the gauge three divisions, which I guessed to amount to about the third part of the extent it had before: but some hours after, the compression made by this newly generated air grew, manifestly fainter, and the imprisoned gauge-air drove down the mercury again, till it was depressed within one division of its first station; so that in this operation there seemed to have been a double compressive power exercised; the one transient, by the brisk agitation of vapours; the other durable, from the aërial on springy particles, either produced or extricated by the action of the spirit of vinegar on the coral.

A considerable quantity of spirit of vinegar being put upon minium in a conical glass, furnished with a glass stopple and a mercurial gauge, continued several days without any sensible depression of the mercury in either leg; nor did any change appear in the gauge, on removing the stopple, though it was evident by its sweetness, that it had made a solution of a great portion of the minium.

Exper. IX. Putting some filings of copper into a phial, capable of holding two or three ounces of water, we poured on them strong spirit of sal ammoniac made without quicklime, till the liquor reached near an inch above them. In 4 days it had acquired a deep blue tincture, but lost again so much of it, that it was pale almost like common water. On unstopping the phial, in less than a minute it acquired a deep blue tincture on the surface to the thickness of the back of a knife, the whole liquor becoming of the like colour in four or five minutes more; and the glass being presently stopped again, it appeared not at the end of nine days to have lost its tincture.

Exper. X. Putting into a round phial, holding about 8 ounces of water, filings of copper, and the mercurial gauge, we poured on the metal strong spirit of sal ammoniac, till it reached to a good height in the phial; which then being hermetically sealed, and set by in a south window, it quickly acquired a deep blue tincture. In 12 days the liquor little by little became so diluted that it was pale, and almost like water: during this stay of the glass in the window, the mercury in the open leg appeared to be impelled upward; after 9 o'clock at night the hermetic seal was broken off, on which there was produced a noise, and the mercury in the shorter and closed leg was briskly impelled up near three eighths of an inch; and though the orifice at which the air had access was scarcely wide enough to admit a middle sized pea, yet within a minute and a half the surface of the liquor, being held between the eye and the candle, appeared to have acquired a very lovely and fair colour, that reached downwards a quarter of an inch; so that the phial seemed to contain two very different liquors swimming one on the other; and the coloration piercing deeper and deeper, within five minutes in all the whole liquor had attained a rich blue colour.

Some Pneumatical Experiments made in the Air-pump on Plants. By M. Huygens and M. Papin. N° 120, p. 477.

Having passed into the little hole in the top of a receiver a sprig of balm, so that the top of the plant was within the receiver, and the roots without: then closing the rest of the hole with cement, that it might be kept void a good while, the exhausted receiver was then taken away in this manner: the edge of the wide orifice of the receiver was well ground, so that it every where touched exactly a glass-plate, which had also been very smoothly ground to serve for a cover to it, and spreading a piece of lamb's skin wetted over the plate, and thus applying it to the engine, the receiver was set over it: but in one place there was a hail-shot of lead, which kept the receiver from being exactly applied to its cover, that so the air might more freely get out: and having afterwards covered all with another large receiver, the pump was plied. All being well exhausted, the engine was shook so as that the little receiver fell off from the hail-shot, and stood every where close to the skin, expanded over the cover of the glass plate. Then suffering the air to re-enter into the great receiver, this air pressing on the small one, kept it so closely fastened to its cover, that it was impossible to sever them.

Having taken away the small exhausted receiver, with the plant half shut up in it; the whole was put into a great glass filled with water, with the root downwards; then there were formed little water-drops on the leaves that were in vacuo. Being left 10 days in this condition, during that time there were entered about two spoonfuls of water into the receiver, which in all appearance had been pressed through the plant.

After this, in order to know whether any air had been formed there; the receiver was replaced on the engine, and having whelmed a larger over it, there was but very little air formed in the small one, because the great receiver was almost exhausted before the air included in the little one could lift it up. Yet at last it did raise it, then inclining the engine to one side, that the small receiver might not be applied to its cover on the air re-entering; and after this manner both the recipients were filled in the same time. Then observing the leaves of the plant, they were not withered, though they were not grown; only the leaves had in the middle a little changed their colour, and had a smell somewhat sourish; but the next morning the plant was quite spoiled. It may be believed that the pressure of the air had made the water enter into this plant with such violence, as to have spoiled the leaves in the middle where they were most tender; though the water still keeping the leaves extended, they withered not; but when the air came to act upon them, the parts of the plant that had suffered so much were soon corrupted by it.

This being done, the experiment was next made the contrary way, that is, with the leaves in the air, and the roots in a bottle of water that was in vacuo; and immediately air-bubbles were observed issuing out at the end of the roots in vacuo. After this, putting water upon the leaves, to see whether this air came from thence, these bubbles began to cease; and having taken away the water wherein the leaves were, the bubbles began again to issue out at the roots as before, and so continued for 24 hours after, but in a small quantity, till at length they quite ceased. During these 24 hours the roots lengthened about 4 lines or one third of an inch; which is a little less than they usually do in the air.—The plant was kept in this condition for 4 days on the engine, observing from time to time to draw out the air that entered by the leaves; then it began to wither, and the roots shot no more.

Another time, putting two twigs of balm, into separate phials full of water; in 5 days they both shot roots. Including in the vacuum the one which had the longest roots, without taking it out of its phial: at the end of three days, observing that it was withered in vacuo, it was taken out, and the phials were changed, to see whether that which had remained in the air, and throve very well in common water, would also thrive in water freed of air; and whether that which was withered in vacuo, would revive in the common water, and in air. Four days after, the twig that had been in vacuo was quite spoiled, and the other still verdant, but not thriving; and it was observed that it did not begin to shoot in the water freed of air till ten days after it had been put in.

This experiment drew another after it, viz. to know whether water purged of air, was less fit than common water, to make plants vegetate. For this end two phials were filled, the one with water purged, the other with common water, and having put a twig of balm in each, both were left in the air. The twig in the common water shot at the end of 6 days, and the other in 10 days after it had been put in.

Having repeated this experiment once more; the twig in the water freed of air, began this time to shoot the 3d day, and the other in the common water, again the 6th day. But it was remarkable, that the twig in the water purged shot only one root, which grew very long; and on the 9th day only it began a little to shoot another, which lengthened but one line in 2 days; whereas the twig in the common water had then 9 or 10 roots, and all very long, having lengthened 5 lines or more in a day.—Although this experiment appeared at first contrary to the preceding, yet it still confirmed the first opinion, that the air which is mixed in common water serves for vegetation.

After this some experiments were made on firmer plants. A green piece of sallow wood was put, part in air and part in vacuo, as above described. On putting into water that part which was in the air, the water presently began to mount, and to pass through the middle of the wood, and incessantly formed bubbles in the receiver, for the space of 24 hours; which was the water passing through the wood, and in part changed into air. For, making the same experiment with a piece of buff, the water mounted also, and passed through it, but it formed no bubbles. Meantime, if there be valves in wood, they must needs be unable to resist the pressure of the air; for it is noted in sallow, as well as in elm, that the water passes through them with the same ease, what end soever is put in vacuo.

One day also having put the upper end of a little elm branch in the vacuum, and the lower end in the air, and drenching this latter in water, as had been done the roots of balm in the first of these experiments; it was a full hour before there appeared any drop of water on the elm leaves in vacuo; whereas on the balm leaves the drops appeared presently. The cause of which may be the firmness of elm wood. But it may be hard to know why water passing through wood forms bubbles, and in passing through leaves forms nothing but drops.

Having inverted also this experiment, that is, the leaves in the water without the recipient, and the lower end of the branch in vacuo, nothing appeared in two hours time; having cut a little off the upper end of the branch, which was very tender, a little moisture appeared at the end in vacuo, but enough only to form one drop; and there appeared no bubble of air. Then cutting the branch a little lower, there was formed one drop of water at the end in vacuo, but it fell not. And having cut the branch yet a little more, the drop of water fell down in vacuo. This shows, that they were not the valves of the plant that hindered the water from passing whilst the branch was entire; but rather that it was the great tenderness of the leaves, suffering themselves to be compressed by the pressure of the air, so that the water could not insinuate itself between their parts.

An Account of some Books. N° 120, p. 481.

I. Francisci Willughbeii de Middleton Armigeri, è Reg. Societate, Ornithologiæ Libri tres; in quibus Aves omnes hactenus cognitæ, in methodum naturis suis convenientem redactæ, accuratè describuntur; Descriptiones Iconibus elegantissimis et Vivarum Avium simillimis, æri incisis, illustrantur: Totum opus recognovit, digessit, supplevit Joh. Rajus, pariter è Soc. R. Sumptus in Chalcographos fecit illustriss. D. Emma Willughby, vidua. Lond. 1676, folio.

The design of this work is not to give pandects of birds, or to collect indiscriminately what has been already published, whether true or false, on this subject: but to illustrate and set in order the history of birds, partly by describing the birds themselves from ocular inspection, partly by borrowing the description of those, of which the author and editor themselves could not get a sight, from the best writers; endeavouring principally to describe all the different known species of birds, and to reduce them to their several classes.

The work itself is divided into three books, the first treating of birds in general; the second, of land-fowl; and the third, of water-fowl. In the first are described the principal, both external and internal, parts of birds, such as are either peculiar to them, or show a peculiar structure and use in them. In the external parts the author observes, that the pectoral muscles in birds are the thickest and strongest of all, serving for the motion of their wings that require great strength; whereas in man, the crural muscles are stronger than those of his arms; whence, if flying were either possible or fit for man, his legs, furnished with a succedaneum to wings for compressing and beating the air, would serve him better for that purpose than his arms. In the internal parts, he notes, among many other things, the considerable difference there is between the brain of birds and that of man and quadrupeds; adapted in birds more for the exercise of the locomotive faculty, than for imagination and memory.

Discoursing, in this part, of the generation of birds, he thinks it highly probable, that their females have in them, from the time of their being first born, all the eggs or the primordials of eggs, that they shall lay as long as they live; which he thinks to be true of human and all other females; making the incubation of the eggs of fowl to be equivalent to the gestation or bearing of other animals; and calling the ovum a uterus expositus, forasmuch as it ministers aliment to the fœtus of those that are commonly called oviparous, like as the womb does in the viviparous.

Treating of the age of birds, and of some of their observable properties and qualities, he remarks that they live long, that their structure somewhat resembles the formation of a ship; that some of them, as partridges and pigeons, lead a conjugal life; and that of these there are more males than females, as among those where one male is sufficient for many females, there are more females than males; that some of them are very ingenious, and imitate the human voice, as parrots, thrushes, blackbirds, jackdaws, starlings, nightingales, of which last, and of parrots, he relates some very extraordinary things. Concluding this first book with an accurate division of birds, and with a catalogue, both of such as constantly abide in England, and such as migrate.

In the second book, treating of land-fowl, he considers first those with hooked beaks and claws; and secondly those that have them more straight, The former are either carnivorous or frugivorous. Concerning the carnivorous or rapacious, he takes notice; 1. That, though Aristotle says they fly solitary, yet vultures have been observed to fly in flocks, 50 or 60 together. 2. That the females of the ravenous birds are larger, stronger, and of greater courage than the males; nature seeming to have been so provident as to furnish those females with such advantages, because they must procure food, not only for themselves, but also for their young ones. Of the frugivorous he observes, amongst other particulars, that, as quails eat hellebore, and starlings hemlock, without any harm to themselves, so parrots not only eat innoxiously the seed of carthamus or bastard saffron, but also grow fat thereby, which is a purgative to man.

Amongst those that have straight beaks and claws, he observes: that the cassowary as well as the pelican is without a tongue; swallowing not only bits of iron, as the ostriches, but also red-hot coals, yet not digesting the iron, but voiding it whole as the ostrich does; that capons may be made to keep, feed, call together, and cover under their wings, young chickens, just as hens do; adding the method for accustoming them to it; that the custom of making use of pigeons for carrying letters is as ancient as the siege of Mutina or Modena, in the time of Hirtius and Brutus; that pigeon's flesh is good for paralytical persons; that swallows distilled with some castoreum, pyony roots, and white wine, are an approved remedy against the epilepsy, &c.

The third, treating of water-fowl, is subdivided into three parts; the first contains those birds that live near water, but not in or upon it. The second, those that live much in the water, being fissipeds, having their toes severed, and long shanked, and of the amphibious kind, partaking of the nature of both those that live near water and swim in it. The third, those that are palmipeds, whose toes are joined together with a membrane. Of those that live near wet places, some again live upon fish or slime, as woodcocks, snipes, curlews, &c. or on insects. Of the piscivorous, the stork is by our author noted to be seen in England, only when he is driven thither by high winds, or other accidents. The penguin is observed to dig deep holes, like conies, on the sea shore, and to make the whole ground thereabout so hollow, that the seamen walking over it often fall in knee deep. The anser bassanus, the Soland goose, breeding in the isle of Bassa, near Edinburgh, lays and hatches no more than one egg at a time. They come thither in spring, and fly away in autumn, but whither is not known. The colymbus minor, or didapper, has such a structure of parts, that he moves much more easily under water than on its surface or aloft; he raises himself from the water with great difficulty, but when he is got up into the air, he can then continue his flight long enough. The swan is very long-lived, and takes almost two months time in hatching her eggs; and the wild kind of swans have their wind-pipes passing into the sternum, and there reflecting or turning back; the use of which is thought to be, that when this bird sometimes for near half an hour with his whole head and neck dives to the bottom for food, turning up his feet on high, there may then from that part of his wind-pipe, which is included in the said sheath of the breast, as from a repository, be furnished air sufficient for so long a time of diving.

But we must refer to the author himself for many other curious particulars: among them, of the extraordinary melodious singing of some birds; the annual moulting of all birds; the medicines to be prepared out of some of them and their very excrements; the artificial nests of many of them; the tasting of the Indian raven of nutmegs, on which he feeds, &c.

II. The Comparative Anatomy of the Trunks of Plants; together with an Account of their Vegetation grounded thereupon, by Nehemiah Grew, M. D. and F. R. S. in 8vo.

As there has been a very happy concurrence of these two eminently learned persons, Signor Malpighi and our present author Dr. Grew, both Fellows of the Royal Society, in making and exhibiting their ingenious and accurate beginnings, concerning the anatomy of plants, and thereby giving a new country of philosophy; so they have both been very industrious in pursuing this subject, in many things confirming each other's observations, and in some few supplying one another's defects.

In general, it is noted by our author, that here are found some things which are little less wonderful within a plant than within an animal; that a plant, like an animal, has organical parts, some of which may be called its bowels; that every plant has bowels of divers kinds, containing divers liquors; that even a plant lives partly on air, for the reception whereof it has peculiar organs. Again, that all the said organs, bowels, or other parts, are as artificially made, and as punctually for place and number composed together, as all the mathematical lines of a flower or face; that by these means the ascent of the sap, the distribution of the air, the confection of several sorts of liquors, as lymphas, milks, oils, balsams, with other acts of vegetation, are all contrived and brought about in a mechanical way.

In particular, we find in the first of the two parts of this book; 1. A description of six several trunks of plants, as they appear to the naked eye; viz. of borage, dandelion, colewort, holyhock, wild cucumber, endive. 2. An accurate description of several trunks, and parts of trunks, as they appear through a good microscope; which parts are, the bark, the wood, and the pith. Of the bark he describes the skin, the parenchyma, and the vessels; the last of which he finds in the bark to be always and only sap vessels. In some, he finds sap-vessels to be only lymphæducts; in others, lymphæducts and lactiferous; in others lymphæducts and resiniferous; lastly, in some, two kinds of lymphæducts, and one of a sort of resinous. He also asserts the analogy between the vessels of an animal and a plant. 3. Having described the bark, he proceeds to the woody part; and here in the several trunks he considers their two general parts, namely, the parenchymous part or insertions, and the vessels; the vessels have likewise much variety, yet are of two general kinds, namely, sap-vessels and air-vessels, whereas it is proper to the bark to have only sap-vessels. 4. Lastly, he describes the pith; first in general, proving it to be, as to its substance, the same with the parenchyma in the bark, and the insertions in the wood; stating its being compounded of two parts, a parenchyma and sap-vessels; the parenchyma made up of bladders, of very different sizes and shapes in different plants, and being of such a texture, that the sides of the greater bladders are composed of lesser, in the same manner as the sap-vessels are, but greater fibres made up of lesser.

The second part of this book gives an account of the vegetation of trunks, grounded on the foregoing anatomy, and showing the use that may be made of the same, in order to explain the manner of vegetation: under the seven following heads:

1. The motion and course of the sap. 2. The motion of the air; that it first enters the plant by the trunk, but chiefly by the root, and is thence in a peculiar manner distributed throughout the whole plant. 3. The structure of the parts; where he explains the unison of the bark to the body of the tree, with the cause of it: considers the various surface and falling off of the bark; the lessening of the pith in the elder-branches; the ruptures of the pith, and for what ends made; further, how the air-vessels come to be less in the trunk of the same plant than in the root; and those of the first year usually much less than those of the years following; as also, how the air-vessels come to be formed always late in the year. 4. The generation of liquors, depending on the structure and formation of the parts; where he shows, that the concurrence of two specifically distinct liquors is as necessary to nutrition in plants as in animals; and that the vessels are the chief viscera of a plant; the viscera of an animal being but vessels conglomerated, and the vessels of a plant, only viscera drawn out at length. To which he adds a particular explanation how a vinous sap is made, how a resinous, oily and milky; likewise, how the liquors of plants come to be white; what is a rosin properly so called; what a gum; what a mucilage. 5. The figuration of trunks; where he renders the cause of a shrub, a tall tree, a slender, and a thick tree, as also of the round or angular

shape of a tree. 6. The motions of trunks; where occurs the cause of their ascent and descent into the ground; their horizontal and spiral motion; and whence solar and lunar plants are distinguished; some winding together with the sun in its diurnal motion, by south from east to west; and others with the moon in its monthly motion, from west to east. 7. The nature of trunks, as variously fitted for mechanical use; where he shows why woods are soft, hard, clevesome, tough, or durable; why the heart of timber is most durable; and why some trees have heart, and others not; likewise, whence the toughness of flax, and what sorts of plants serve for the best tow; giving lastly an account how all prosperous conjunctions in grafting may be known, and what is the chief use of grafting, viz. to accelerate the growth of good fruit.

III. The Royal Almanack, &c. by N. Stevenson, one of his Majesty's gunners.

A continuation of the same almanack as published the former year, and before described.

Continuation of the Pneumatical Experiments, made by M. Huygens and M. Papin. N° 121, p. 492.

To know whether the vacuum would be of use to the preservation of bodies, an apple was inclosed, which had a little speck of rottenness, with some water in the same recipient, thereby to promote its corruption in case any should take place. But no change of that nature was observed.

The 7th of June were inclosed in a receiver two nosegays of roses, one suspended at the top, the other having its tail in a little vessel full of water, together with a gauge 4 inches long, to know whether any air would be there produced. Two days after, the roses were a little withered, and the water already risen to 8 or 10 lines, near the top of the gauge; and after that, the changes of these flowers became still less, so as not to be much more withered. The roses in the water were as much withered as the others, and as soon. I shall keep them in this condition as long as I can. Other roses which had been included at the same time, but with air, grew mouldy in less than 8 days.

At another time was inclosed one single rose-bud in a very small glass, to discover whether it would keep its scent. At the end of 15 days it looked a little less fresh, but was not at all withered; and having taken it out, it had still its good smell; but afterwards it lost all, both colour and smell, in less than two hours. Its leaves did not appear moist in the vacuum, though they did so as soon as they were in the air. Which shows, that the leaves had acted as sponges, discharging their humidity by the pressure of the air. Some gilliflowers put into the receiver changed but very little, only they looked as if they had been dipped in water.

Strawberries at the end of two days looked less fresh, but after that, seeing they changed no more in 15 days, they were taken out of the vacuum, when they had still the smell and taste of strawberries; but they had also contracted a very ungrateful taste of the cement used to close them up. At another time some strawberries were inclosed without cement, making use of a skin after the manner described formerly; and then nothing new was observed, except that their taste kept good, but was a little sour, and that they yielded a little water.

Some cherries inclosed were all burst except two; in two days more they had a little changed their colour, and those two that before remained whole, were now burst like the others. After that no more change in them was observed.

Having inclosed in the vacuum one cherry with 11 large currants, the cherry burst presently, but after that, it changed not, only it appeared turned, as the currants also did.

Having inclosed in the vacuum four raspberries and three currants, the latter appeared also to be turned, and the raspberries looked less fresh than before. But it is now more than five months, and no change is perceived in them.

Hitherto had been employed only small receivers, which just held the little fruit put in them, and the red currants seemed to keep well enough; so that one day we filled a large glass with them, hoping to keep that as well as the small receivers. But were surprised five days after to see that bubbles were formed in the turpentine, which had been put about the glass, in the place where it was fastened to its cover, and that these bubbles were burst outwards; and afterwards, having seen that the cover held fast to the bolthead no longer, it left no doubt of the currants having produced air enough to lift up the glass, and to form in the turpentine the bubbles we had seen, and it appeared by the smell that they had fermented. They were yet good, except some that had lost almost all their taste and all their acidity.

The same thing happened with a very small receiver, that could hold no more than one cherry and one red currant. These fruits yielded also air enough to lift up their receiver seven days after they had been included. This effect is rather to be ascribed to the cherry than the currant.

Another time we inclosed some of the same kind of cherries, a whole large glass full, and found that from the second day they had yielded air enough to lift up the cover. We took away part of the cherries, and inclosed the rest again; and this second time they did not raise the glass till the eighth day.

The cherries looked fair, but they had lost much of their taste, and afterwards they were spoiled in less than an hour.

Having inclosed three pears of that sort called rouselet, in a like-figured glass, which could hold no more, they lifted up the glass at the end of five days; but they were not changed, only one of them was a little softer.

A peach was inclosed in such a glass emptied of air, with a gauge to it; and the first six hours the quicksilver in the gauge was risen about an inch; yet it was not till the 13th day that the glass was lifted up; and the peach appeared to have kept very well till then; but after that it rotted in a very little time. Inclosed bread yielded no air in a whole month.

Roasted mutton included, with a gauge, in four days had yielded no air; but after six weeks the mercury was risen to the middle of the gauge, and having taken out the meat it smelt rank. Raw beef being inclosed, with a gauge; in two days the quicksilver was risen an inch in the gauge; and after six weeks the mercury was got almost to the top of the gauge, and this meat had contracted a much worse smell than that which had been roasted. Keeping for 15 days a piece of fresh butter in vacuo, it smelt stronger than when first put in: yet it could still be eaten on bread; whereas another piece of butter kept in the air the same time was altogether unfit to be used.

Extract from a Letter of Mr. Flamsteed, Regius Professor of Astronomy at Greenwich, to Sir Jonas Moor, Knight, &c. containing his Observations of the late Lunar Eclipse, Dec. 21, 1675. N° 121, p. 495. *Translated from the Latin.*

Correct time of clock.			The Phases.
14^h	29^m	30^s	.... Between the cusps, 2085 = 17′ 16″.
14	55	45	.... The shadow nearly touched Hæmus.
15	0	30	.... It had certainly touched Hæmus.
15	11	30	.... The right cusp from Meræotis 1235 = 10′ 14″.
15	35	0	.... The lucid parts about 2800 = 23′ 11″.
15	42	30	.... The shadow near Macra.
15	52	45	.... Between the cusps about 2288 = 18′ 57″.
16	7	15	.... The end: for the limb appeared, and the moon round.
16	8	0	.... The limb very plainly seen through the tube.
16	15	30	.... The penumbra, which to the naked eye resembled an eclipse.
16	19	30	.... Diameter of the moon taken about 3757 = 31′ 5″.
16	23	0	.... Still, and afterwards, the limb, after the eclipse, seemed rather obscure.

I observed besides, that the shadow always appeared much more distinct at the horns than the other parts of the moon. At the first observation, or a little sooner, the horns were parallel to the horizon. Then also Porphyrites and the great black lake were equally out of the shadow, viz. about the length of Meræotis. The shadow went out near the upper Hyperborean lake, the penumbra remaining, which exhibited the eclipse to the naked eye as far as $4^h\ 15\frac{1}{2}^m$. But the limb left by the eclipse did not recover the brightness of the other limb till $4^h\ 28^m$, or later. The times of the phases were corrected by altitudes of Arcturus and the bright star in the Crown; to the taking of which altitudes I applied myself at intervals, when clouds came over the moon, those stars sometimes shining out very bright in the other part of the heavens.

A Letter of Mr. Franc. Linus, from Liege, the 25th of Feb. 1675, N. S. being a Reply to the Letter printed in Number 110, by way of Answer to a former Letter of the same Mr. Linus, concerning Mr. Isaac Newton's Theory of Light and Colours. N° 121, p. 499.

Hon. Sir.—In yours of Dec. 17, you say, I may rest assured, first, that the experiment was made in clear days; 2dly. that the prism was placed close to the hole, so that the light had no room to diverge; and 3dly. that the image was not parallel, as I conjectured, but transverse to the axis of the prism. Truly, sir, if these assertions be admitted, they do indeed directly cut off what I said of Mr. Newton's being deceived by a bright cloud. But if we compare them with Mr. Newton's relation of the experiment in the Phil. Trans. N° 80, it will evidently appear, they cannot be admitted, as being directly contrary to what is there delivered. For there he tells us, the ends of the coloured image he saw on the opposite wall, near five times as long as broad, "seemed to be semicircular." Now these semicircular ends are never seen in a clear day, as experience shows. From whence it follows, against the first assertion, that the experiment was not made in a clear day. Neither are those semicircular ends ever seen when the prism is placed close to the hole: which contradicts the second assertion. Neither are they ever seen when the image is transverse to the length or axis of the prism: which directly opposes the third assertion. But if, in any of these three cases, the image be made so much longer than broad, as easily it may by turning the prism a little about its axis, near five times as long as broad, than the one end thereof will run out into a sharp cone or pyramis, like the flame of a candle, and the other into a cone somewhat more blunt, both which are far from seeming semicircular; whereas, if the image be made not in a clear day, but with a bright cloud, and the prism not placed close to the hole, but at a competent distance from the same, as you see it placed in the scheme of the experiment in N° 84, then these semicircular ends always appear with the sides thereof straight lines, just as Mr. Newton there describes them. Neither is the length of the image transverse, but parallel to the length of the prism. Out of all which it evidently follows, that the experiment was not made in a clear day; nor with the prism close to the hole; nor yet with the image transverse, as is now affirmed, but by a bright cloud, and a parallel image, as I conjectured; and I hope you will also now say, I had good reason so to conjecture, since it so well agrees with the relation: and experience will also show you, if you please to make trial, as it was made, in a dark chamber, and observe the difference between such an image made by a bright cloud, and another made by the immediate rays of the sun: for, the former you will always find parallel, with the ends semicircular; but the latter you will find transverse, with the ends pyramidical as aforesaid, whenever it appears so much longer than broad.

More might be said out of the same relation, to show that the image was not transverse. For if it had been transverse, Mr. Newton, so well skilled in optics, could not have been surprised, as he says he was, to see the length thereof so much to exceed the breadth; it being a thing so obvious and easy to be explained by the ordinary rules of refraction. That other place also in the next page, where he says the incident refractions were made in the experiment equal to the emergent, proves again that the said oblong image was not transverse but parallel. For it is impossible that the transverse image should be so much longer than broad, unless those two refractions be made very unequal, as both the computation according to the common rules of refraction, and experience testify. Wherefore Mr. Newton had no reason to tax P. Pardies of hallucination, for making those two refractions very unequal: for that learned optician very well saw, that in a clear day so great an inequality of length and breadth could not be made, unless those two refractions were also made very unequal. These places, I say, might be added to the former, and further here explained if need were; but there being no need, I cease to detain you any longer herein.

Mr. Isaac Newton's Considerations on the former Reply; together with further Directions, how to make the Experiments controverted aright: Cambridge, November 13, 1675. N° 121, p. 501.

SIR—When you showed me Mr. Line's second letter, I remember I told you that I thought an answer in writing would be insignificant, because the dispute was not about any ratiocination, but my veracity in relating an experiment, which he denies will succeed as it is described in my printed letters: for this is to be decided not by discourse, but new trial of the experiment. What it is that imposes on Mr. Line I cannot imagine; but I suspect he has not tried the experiment since he acquainted himself with my theory, but depends upon his old notions, taken up before he had any hint given to observe the figure of the coloured image. I shall desire him therefore, before he returns any answer, to try it once more for his satisfaction, and that according to this manner.

Let him take any prism, and hold it so that its axis may be perpendicular to the sun's rays, and in this posture let it be placed as close as may be to the hole through which the sun shines into a dark room, which hole may be about the size of a pea. Then let him turn the prism slowly about its axis, and he shall see the colours move upon the opposite wall, first towards that place to which the sun's direct light would pass, if the prism were taken away, and then back again. When they are in the middle of these two contrary motions, that is, when they are nearest that place to which the sun's direct ray tends, there let him stop; for then are the rays equally refracted on both sides the prism. In this posture of the prism let him observe the figure of the colours, and he shall find it not round as he contends, but oblong, and so much the more oblong as the angle of the prism, comprehended by the refracting plains, is larger, and the wall on which the colours are cast, more distant from the prism; the colours red, yellow, green, blue, purple, succeeding in order, not from one side of the figure to the other, as in Mr. Line's conjecture, but from one end to the other; and the length of the figure being not parallel but transverse to the axis of the prism. After this manner I used to try the experiment: for I have tried it often; sometimes to observe the circumstances of it, sometimes in order to further experiments, and sometimes to show it to others, and in all my trials the success was the same. But whereas Mr. Line thinks, I tried it in a cloudy day, and placed the prism at a great distance from the hole of the window; the experiment will not succeed well if the day be not clear, and the prism placed close to the hole, or so near at least, that all the sun's light that comes from the hole may pass through the prism also, so as to appear in a round form if intercepted by a paper immediately after it has past the prism.

When Mr. Line has tried this, I could wish he would proceed a little further to try that which I called the experimentum crucis, seeing, if I mis-remember not. he denies that as well as the other. For when he has tried them, which by his denying them, I know he has not done yet as they should be tried, I presume he will rest satisfied.

Three or four days after you gave me a sight of Mr. Line's second letter, I remember I thereupon showed the first of these two experiments to that gentleman whom you found with me, when you gave me that visit, and whilst I was showing it to him, R. H. a member of the Royal Society came in, and I showed it to him also. And you may remember that R. H. two or three years ago in a letter read before the Royal Society, and transmitted to me, gave testimony, not only to the experiments questioned by Mr. Line, but to all those set down in my first letter about colours, as having tried them himself; and when you read Mr. Line's letter at a meeting of the said Society, and was pleased to do me the favour to propound the experiment to be tried in their presence, R. H. spake of it to them as a thing not to be questioned. But if it have not yet been tried before them, and any of them upon Mr. Line's confidence doubt of it, I promise when I shall have the happiness to be at any more of their assemblies, upon the least hint, to show them the trial of it; and I hope I shall not be troublesome, because it may be tried, though not so perfectly, even without darkening a room, or the expence of any more than half a quarter of an hour; although if Mr. Line persist in his denial of it, I could wish it might be tried sooner there, than I shall have an opportunity to be among them.

An Extract of another Letter of Mr. Newton, the 10*th of January* 167$\frac{5}{6}$, *relating to the same Argument.* N° 121, p. 503.

—— By Mr. Gascoin's letter* one might suspect, that Mr. Linus tried the experiment some other way than I did; and therefore I shall expect till his friends have tried it according to my late directions. In which trial it may possibly be a further guidance to them, to acquaint them, that the prism casts from it several images: one is, that oblong one of colours which I mean; and this is made by two refractions only. Another there is, made by two refractions and an intervening reflection; and this is round and colourless, if the angles of the prism be exactly equal; but if the angles at the reflecting base be not equal, it will be coloured, and that so much the more, by how much unequaller the angles are, but yet not much unround, unless the angles be very unequal. A third image there is, made by one single reflection, and this is always round and colourless. The only danger is in mistaking the second for the first. But they are distinguishable, not only by the length and lively colours of the first,

* This letter was written to the editor, December 15, 1675, from Leige, where M. Gascoine, having been a scholar of Mr. Linus, now deceased, resides. In it are contained these words, to which Mr. Newton, to whom it was communicated, seems here to have respect, viz. Mr. Linus tried the experiment again and again, and called divers on purpose to see it, nor ever made difficulty to show it to any one, who either by chance came to his chamber as he was doing it, or showed the least desire to see the same. So that for point of experience, Mr. Newton cannot be more confident on his side than we are here on the other; who are fully persuaded, that, unless the diversity of placing the prism, or the largeness of the hole, or some other such circumstance, be the cause of the difference betwixt them, Mr. Newton's experiment will hardly stand.—Orig.

but by its different motion too: for whilst the prism is turned continually the same way about its axis, the second and third move swiftly, and go always on the same way till they disappear; but the first moves slow, and grows continually slower till it be stationary, and then turns back again, and goes back faster and faster, till it vanish in the place where it began to appear.

If, without darkening their room, they hold the prism at their window in the sun's open light, in such a posture that its axis be perpendicular to the sun beams, and then turn it about its axis, they cannot miss of seeing the first image; which having found, they may double up a paper once or twice, and make a round hole in the middle of it, about $\frac{1}{2}$ or $\frac{3}{4}$ of an inch broad, and hold the paper immediately before the prism, that the sun may shine on the prism through that hole; and the prism being stayed, and held steady in that posture which makes the image stationary; if the image then fall directly on an opposite wall, or on a sheet of paper placed at the wall, suppose 15 or 20 feet from the prism, or further off; they will see the image in such an oblong figure as I have described, with the red at one end, the violet at the other, and a bluish green in the middle: and if they obscure their room as much as they can, by drawing curtains or otherwise, it will make the colours the more conspicuous.

This direction I have set down, that nobody into whose hands a prism shall fall, may find difficulty or trouble in trying it. But when Mr. Linus's friends have tried it thus, they may proceed to repeat it in a dark room with a less hole made in their window shutter. And then I shall desire that they will send you a full and clear description, how they tried it, expressing the length, breadth and angles of the prism; its position to the incident rays, and to the window shutter; the largeness of the hole in the window shutter, through which the sun shined on the prism; what side of the prism the sun shined on; and at what side the light came out of it again; the distance of the prism from the opposite paper or wall on which the refracted light was cast perpendicularly; and the length, breadth, and figure of the space there illuminated by that light, and the situation of each colour within that figure. And if they please to illlustrate their description with a scheme or two, it will make the business plainer. By this means, if there be any difference in our way of experimenting, I shall be the better enabled to discern it, and give them notice where the failure is, and how to rectify it. I should be glad too, if they would favour me with a description of the experiment, as it has been hitherto tried by Mr. Linus, that I may have an opportunity to consider, what there is in that which makes against me.

An Account of some Books. N° 121, p. 505.

I. Pharmaceutice Rationalis, sive Diatriba de Medicamentorum Operationibus in Humano Corpore; Pars Secunda; Auth. Tho. Willis, M. D. &c. E. Theatro Sheldoniano, 1675.

As we did not deem it necessary to retain the analysis given of the first part of this book, and inserted in N° 99, so, for the reason there assigned, we omit an account of this.

II. Collegium Experimentale sive Curiosum, in quo primaria hujus Seculi Inventa et Experimenta Physico-Mathematica, An. 1672, quibusdam Naturæ Scrutatoribus spectanda exhibuit, et ad causas suas naturales demonstrativâ methodo reduxit Johannes Christophorus Sturmius,* Mathem. ac Phys. in Academ. Altdorfina, Prof. P. &c. Norimbergæ, An. 1676, in 4to.

The learned author of this piece, in the invitation he made to some students in philosophy, at Altdorf in Germany, that they would unite under his conduct for examining some of those discoveries and experiments made of late years, remarks that in this one age, not yet elapsed, there has been far more progress made in natural philosophy, than in many ages before, and chiefly by means of that happy experimental method, employed by the Royal Societies of England and France, and the noble colleges of Rome, Florence, Venice, &c. by which method he says, the disputatious way of wrangling about mere scholastical and fruitless notions being laid aside, things themselves from the inmost recesses of nature have been searched into, and many of them discovered and brought to light. Which, having first explained in his college lectures, he now in this volume gives them to the public at large. The collection consists of 16 essays, of a philosophical nature, treated of; which he acknowledges to have been, at least for the most part treated of, by some of the most eminent philosophers of

* John Christ. Sturm, a celebrated mathematician and philosopher, was born at Hippolstein, 1635, where he died in 1703. He was first minister of a church in Germany during five years; and then became professor of mathematics and natural philosophy, at Altdorf in Germany. He exerted himself greatly, and was very useful, by his lectures and otherwise, in explaining and diffusing the knowledge and discoveries made in that remarkable age, the 17th century; as is manifest by his writings, particularly the curious work above noticed. In 1684 the author gave a second, and much larger part, of the like collection of discoveries made till that æra; with an appendix of further additions and explanations to the particulars in the 1st part. The same year also he published a large collection of letters to Dr. Henry More, of Cambridge, on the controversy concerning the weight and spring of the air. Sturm published also a German translation of Archimedes; and several mathematical works of his own; as, Mathesis Enucleata, in one volume 8vo.; and Mathesis Juvenilis, in two large volumes, 8vo.

this age; amongst whom he names the illustrious Bacon, Descartes, Torricelli, Boyle, Roberval, Paschal, Huygens, and Guerick.

Of these 16 essays, The 1st explains the diving-bell, its structure and use.—The 2d exhibits the chief optical phænomena of a darkened chamber.—The 3d relates the effects of the Baroscope, or the Torricellian quicksilver tube.—The 4th shows some experiments of metalline weights floating, and without any other support freely pendulous in water and air.—The 5th represents some other hydrostatical phænomena, about glass-bubbles, at first floating on the surface of the water, and afterwards subsiding by affused oil, &c.—The 6th deduces some consectaries from the before asserted spring and weight of the ever-gravitating air.—The 7th contains some experiments of a glass-tube, 36 feet high, filled with water, and declares their causes from the equilibrium of the air and so much water.—The 8th shows some phænomena in very narrow tubes, viz. the ascent of water in them above its other superficies; with the reason of it.—The 9th exhibits the effects, and declares the causes of several sorts of thermometers.—The 10th mentions a very paradoxical experiment of Franc. Lana.—The 11th represents, and reduces to its principles, the force of mechanical powers.—The 12th discourses of an art, both of writing any thing in any tongue, to be read and understood by any nation of what language soever, and of reading and understanding any thing written in any tongue.—The 13th contains some considerable effects and experiments of the air-pump, partly made by others, partly by himself; all reduced to their proper causes.—The 14th declares the uses and reasons of the effects of hygrometers and chronometers.—The 15th explains the structure of microscopes and telescopes, proving their effects by many observations.—The 16th exhibits divers pleasant spectacles of a portable darkened room, and of a catoptro-dioptrical lantern, adding the genuine reasons of them; and for an appendix subjoins his new, as he esteems it, perpetual almanack, comprehending not only the immoveable feasts fixed to certain days of the months, but also the moveable ones, of Easter, Pentecost, and the rest depending on them; as also the motions of the sun and moon through the 12 celestial signs, with the moon's menstrual increases and decreases, &c.

Besides an appendix, of further particulars about the diving machine, the weight of the air, the new pocket watches, &c.

III. England's Improvements, in two Parts, &c. By Roger Cook, London, 1675, in 4to.

In the former of these two parts, the author shows, how the kingdom of England may be improved in strength, employment, trade, and wealth, by increasing the value of lands, &c. And, in the latter part, how the navigation of England may be increased.

IV. The College of Physicians Vindicated, and the True State of Physic in this Nation faithfully represented, &c. By Charles Goodall, M. D. London, 1676, in 8vo.

In this vindication of that learned body, the London College of Physicians, their utility to the public and to the profession, in suppressing quackery and maintaining the dignity and true interests of the medical character, is strenuously advocated.

Of the Incalescence of Quicksilver with Gold. By B. R. Communicated in Latin. Translated by Mr. Oldenburg. N° 122, p. 515.

In this long paper Mr. B. R. enters into an inquiry whether or not the commixtion, or amalgamation of gold with mercury, is accompanied with a sensible production of heat; which he decides in the negative, in respect to common mercury, and in the affirmative with regard to purified, or (as he terms it) ennobled mercury. I took, says he, to one part of the mercury, sometimes half and sometimes an equal weight of refined gold reduced to a calx or subtle powder. This I put into the palm of my left hand, and putting the mercury upon it, stirred it and pressed it a little with the finger of my right hand, by which the two ingredients were easily mingled, and grew not only sensibly but considerably hot, and that so quickly, that the incalescence sometimes arrived at its height in a minute. I found the experiment succeed, whether I took as much, or only half as much gold as mercury; but the effect seemed to be much greater when they were employed in equal weight. And to obviate a suspicion, which, though improbable, might possibly arise, as if the immediate contact of the ingredients and the skin produced a sense of heat, which was not due to the action of the metals upon each other; I had the curiosity to keep the mixture in a paper, and found not its interposition to hinder me from feeling the incalescence, though it much abated the degree of my sense of it.

I tried also the same mercury with refined silver reduced to a very fine powder; but I could not perceive any heat or warmth at all; though I am apt to think, that if I had had a sufficient quantity of leaf-silver to have made the experiment with, I should, after some time, have produced an incalescence, though much inferior to what the same quantity of mercury would produce with gold. I shall now add, that in order not to be thought to impose upon myself, I not only made trial in my own hand, when in different tempers as to heat and cold, but the hands of others, who were not a little surprised and pleased at the event. And this I did more than once or twice; by which means I had, and still have, divers witnesses of the truth of the experiment. Among whom is the learned Secretary of the Royal Society; to whom having given the ingredients, I desired him to make the experiment in and with his own hands, in which it proved successful within somewhat less than a minute.*

And what makes this incalescence the more considerable is, that being willing to husband my mercury, a great part of which had been, as I guessed, stolen from me before I employed it, I made these trials but with a drachm at a time, which scarcely amounts in quantity to the largeness of half a middle sized bean; whereas, if I could have made the experiment with a spoonful or two of quicksilver, and a due proportion of gold, it is probable the heat would have been intense enough, not only to burn ones hand, but perchance to crack a glass phial; since I have sometimes had of this mercury so subtile, that when I employed but a drachm at a time, the heat made me willing to put it hastily out of my hand.

I doubt not but what I have related and hinted has made you curious to know somewhat further of this mercury: and I confess, that if there be any truth in what some of the most approved spagyrists have delivered about a solvent of gold that seems akin, and perhaps is not much nobler than one that I had; it seems allowable to expect, that even ours should be of more than ordinary use, both in physic and alchymy. But the misfortune I had of having lost a considerable quantity of it, being afterward increased by the almost sudden death of the only operator I trusted in the making of it; I was altogether discouraged from repeating such a troublesome preparation, especially being diverted by business, sickness, and more pleasing studies. And though I have not forgotten some not despicable trials that I made with our mercury, yet since they are not necessary to the question that occasioned this paper, I shall pass them over in silence, and only observe some few things I had almost forgotten to tell you; namely first, that whereas it is usual to take four, five or six, nay eight or 10 parts of common quicksilver to make an amalgam with one of gold, even when both are heated by the fire; I found our mercury so congruous to that metal, that it would presently embody with no less than an equal weight of it, and produce a pretty hard amalgam or mixture, in which the mercury was so diffused, that the gold had quite lost its colour. Secondly, I shall add what for aught I know has not been yet observed, that this power of penetrating gold and growing hot with it, is so inherent, not to say radicated, in our mercury, that after it had been distilled from gold again and again, I found it retain that property. And lastly, as it may be suspected that this faculty may be quickly lost, (as that of the prepared bononian stone to receive light, has been complained of as not

* Since this was written, the President of the Royal Society, Lord Visct. Brouncker, made the same experiment with some of the same mercury, in his own hand, with good success.—Orig.

durable) I found by trial that a single drachm of mercury, made after a certain manner, did the third or fourth year after I had laid it by, grow so hot with gold, that I feared it would have burnt my hand.

I shall therefore admonish those inquisitive spagyrists, that may be desirous to try, whether their purified mercury be incalescent, that they be not too hasty to conclude it is not so; nor to reject it, unless they have made the trial with gold duly prepared. For I have found, that my mercury did not grow hot with the smallest filings of gold I could make (though indeed within a few hours after it did, without the help of fire, embody with it into a hard amalgama,) which argued that the corpuscles of the metal were not yet small enough to be suddenly penetrated by the quicksilver: nor will every calx of gold serve our turn, as I have found by employing, without success, a very fine and spongy calx made after an uncommon way, the golden particles having, as it seemed, some extremely fine though unobserved dust of the additament sticking to them, which hindered the adhesion of the mercurial ones. Now the calx of gold that I most used, as finding it still to do well, was that made by quartation* (as Alchymists call it.) But because it is not so easy, as even chemists that have not tried imagine, to make good calces of gold, and that in the way newly mentioned there needs fusion of the gold and of silver (for which many chemists want conveniences,) and they are often imposed on by common refiners, who here usually sell in wires such silver for fine (which indeed it is comparatively,) as I have found not to be without mixture; I shall add, that by making an amalgama the common way with pure gold and vulgar mercury, and dissolving the mercury in good aquafortis, there will remain a powder, which, being well washed with fair water to dulcify it, and kept a while in a moderate fire to dry thoroughly without melting it, will become a calx, which I have more than once used with our mercury with good success. It is true, both in this way and in that (by quartation,) aquafortis, which is a corrosive liquor, is employed to bring the gold to powder, and therefore in a diffident mind some suspicion may arise, that the incalescence may proceed only from the action of the acid particles of the menstruum, which yet adhering to the corpuscles of the gold works upon the quicksilver, as aquafortis is known to do: but, to omit those answers that cannot be given in few words, after I have taken notice, that, if the effect depends not on our mercury (as prepared) but only on the calx, it appears not, why this should not grow hot with common mercury as well as with ours; I

* That is, by melting together one part of fine gold, and three or four parts of cuppelled silver, and then putting the mass, wherein the metals are mixed, almost per minima, into purified aquafortis, which dissolving the silver only, leaves the gold in the form of a fine calx.—Orig. [The term *calx* of gold is here improperly used; for in the operation of *parting* with aquafortis, the gold is left in its reguline state.]

shall need to add, for the removal of this subtle scruple no more, than this plain experiment, (which I twice or thrice made,) namely, that taking instead of a calx of gold, a competent number of leaves of gold, such as book-binders and the apothecaries use, this gold that was without the help of salts reduced by beating to a sufficient thinness (insomuch that 70 odd leaves did not weigh a scruple,) I found (more than once) upon putting two or three times the weight of our mercury to them, that a smart heat was presently produced in my hand.

Observations on several Passages in the last two Months Transactions, relating to some unobserved Qualities of the Air; to the Mixing and Fermenting of Liquors in vacuo; to the History of Birds; the Anatomy of the Trunks of Vegetables; Baroscopes, &c. N° 122, p. 533. *Anonymous.*

Corrosions of bodies, especially with a sensible motion, by emitting effluvia, which may also be springy themselves, if saline, may further bend the springy particles of the air, giving it a greater elasticity. On the contrary, other corrosions not so emitting, by different ways, may weaken this spring.

In the first experiment, (in a late paper by Mr. Boyle) the solution may acquire a brown colour by the menstruum's imbibing the particles of copper into itself, which would alter its texture; and the impeding particles, diversifying the refractions and reflections, may modify the rays of light; or, as it is in Mr. Newton's theory, the solution may be qualified only to admit such rays whose degrees of refrangibility, and mixture with other rays, may be predisposed to give a brown, to which effect the air did much contribute.

So perhaps in the second experiment the delay of regaining was to be imputed to the cold or moisture, or other critical circumstances of the air, having less of those dissolving particles which make it a menstruum on those bodies.

Those ingenious Frenchmen, excited by Mr. Boyle, the glory of learning, as well as of our nation, having given us an attempt of mixing and fermenting liquors in vacuo, do prosecute fermentation, particularly of that in the growth of plants; how it may be impeded or promoted by the absence or presence of the air.

In the continuation of January, by which and what Mr. Boyle has written, we see the nature of the air, its effects on bodies when present; by procuring its absence. Now Sir W. Petty can no more complain at the idle employment of weighing air; for the doctrine being well illustrated, and the theory established, we thus descend again to more useful and grounded experiments; as the method mentioned by my Lord Bacon, to proceed from experiments to axioms and assertions; from these, as too general, to particular and useful experiments, which before we could not do, till we had cleared the doctrine from wide and extravagant guesses.

The industrious author of the History of Birds gives me occasion to wish, that some other would undertake that of other creatures, as minerals, insects, fishes, &c. reducing them to their classes with philosophical observations, useful to illustrate many passages in experimental philosophy.

By the Royal Almanack we see it is the ingenuity of this age, that being freed from the slavish opinion of the government of the planets, they cancel their power in events, and show their operation on the masses of matter, and some peculiar sympathizing bodies, as in the essay of hidden qualities in the air, which is the true end and perfection of astrology, and natural magic.

By accurate baroscopes we may regain that knowledge which still resides in brutes, and we forfeited by not continuing in the air, as they do for the most part, and by intemperance corrupting the crasis of our senses. I remember that Kircher, in his description of China, speaks of a stone, (how true I know not) which being made into a human shape, by nature or art, by change of colour prognosticates fair or foul weather.

Some Pneumatical Experiments on Animals in the Air-pump. By M. Huygens and M. Papin.* N° 122, p. 542.

We included in the vacuum an insect which resembles a beetle, but is a little larger; and when it seemed dead, on giving it air again, it soon after recovered. Putting it in the vacuum again, and having left it there for an hour, we readmitted the air, the insect required then much more time to recover. Including it the third time, and having left it there two days, on giving it air again, it required above ten hours before it began to stir again, yet it recovered again at last; but having put it in again the fourth time, and left it there eight days, it never recovered more.

Having inclosed a butterfly, on readmitting the air, the top of its back, which before was much swelled, shrunk in more than it did before, and the insect never recovered.

Several birds, mice, rats, rabbits, and cats, being inclosed, some of them recovered by quickly giving them air again, before the engine was quite exhausted; but none of them revived that had been in a perfect vacuum. M. Guide made frequent dissections of such animals thus killed, and observed among other things, that their lungs fell to the bottom in water. And he says that the solidity or closeness of the lungs of animals that die in vacuo is owing to the blood, which is propelled into the lungs by the vena arteriosa, so strongly

* Compare No. 62 and 63 of these Tracts, where many experiments of this kind, made by Mr. Boyle, are recorded.

presses the bronchia of the arteria aspera, that it expels the air out of them, and presses their sides together. But it appears not probable that the blood of the vena arteriosa can thus compress those bronchia, being inclosed in its vessels, which hinder it from compressing others. And the bronchia being harder than the arterial vein, they will compress it more easily than be compressed by it. It is therefore far more probable, that if the lungs be compresed that compression be made by the pleura, which may be swelled within the breast, as the skin is swelled without.

But it is not necessary that the lungs be compressed in vacuo, to make them subside in water. For we have often put pieces of lungs and whole lungs in the vacuum, and they remained there extremely swelled; but as soon as the air was again admitted, they became very flat and red, and sunk to the bottom in water. Which shows, that it is sufficient for getting the air out of the lungs, to render them close and red: and we have not been able to produce this effect but by means of the exhausting engine. For we have left lungs a whole night between two plates, with a great weight upon them, to endeavour to express the air out of them, but without success; and those lungs still floated on the water. We have also tried to make the air re-enter into the lungs after they were rendered solid in the engine, which was very easy; for drawing them out from the bottom of the water, and blowing into the aspera arteria, the lungs swelled again, and resumed their ordinary colour, and floated on the water. And this is what happens to the lungs of infants newly born.

Promiscuous Experiments with the Air-pump. By M. Huygens and M. Papin. N° 122, p. 544.

Putting a covered receiver, a fourth part of which was filled with water, over the flame of a candle, the water boiled very quickly, yet the glass not much heated, so that the water boiled near a quarter of an hour with a great ebullition, and the glass was no more than tepid. After it was removed away from the flame, the water continued a great while boiling, and began again from time to time. On making the same experiment with a gauge, all the bubbles that issued out of the water did not make the mercury rise sensibly. Afterwards exposing this receiver to the frost, the ice which was formed in it was not quite free from bubbles, though the water had boiled in the vacuum, which, one would think, should have driven out all the air: yet the bubbles were there far less numerous than in ice made of ordinary water. The quicksilver was not much raised in the gauge. Afterwards this ice was melted, and the water abroad put to freeze again, still without taking it out of the vacuum, and this second time it was much freer from bubbles.

After this, spirit of wine was made to boil in vacuo in the same manner as the water, and it boiled much sooner; and it made the mercury rise about an inch in the gauge. After taken from the fire, it continued boiling; and even sinking the receiver into cold water, it boiled much more strongly.

Having caused some gunpowder to be burnt with a burning glass; we were much surprised to see that it burnt grain by grain, none of the grains kindled fire in those which touched. Another time when the sun had less force, we could not at all kindle the powder, but made it only boil and emit store of smoke. We put a gauge in the same recipient, by means whereof we observed that all that smoke produced no air; for the quicksilver did not rise in the tube. This smoke falling on the paste-board, on which the powder was put, appeared yellow of the colour of brimstone. Then we took out the powder that remained, being like a black mass, and having put it upon burning coals, it burned like salt-petre; so that the sulphur was almost all exhaled.

Another time having put the weight of 12 or 15 grains of powder in a glass shaped like a cupping-glass, capable of holding 14 ounces of water, and having set fire to it, the powder boiled and smoked as usual. Afterwards seeing that the grains began to crack very near one after another, we took away the burning concave, for fear all should be kindled together; but it was already too late, for the corns continued to crack longer than a second of time, and at last all kindled, though there was then nothing left to heat them but the fire which they had kept within themselves. The receiver was lifted up above a foot high without breaking.

Another time putting the weight of 18 grains of powder with a gauge into a receiver, holding seven pounds of water, and firing the powder, it made the quicksilver rise to the height of an inch and a half in the gauge; and I am very well assured that all that air was not come from without; for that part of the receiver to which the cover is applied had always been under water.

From what has been related it may be concluded, that there is a fifth part of air in gunpowder, supposing, as other experiments show, that air is about 1000 times lighter than water. For, in this experiment, the mercury rose to the 18th part of the height where the air commonly sustains it; and consequently the weight of 18 grains of powder yielded air enough to fill the 18th part of a receiver that contains seven pounds of water. Now this 18th part contains 49 drachms of water: wherefore the air that takes up an equal space being 1000 times lighter weighs $\frac{1}{1000}$ of 49 drachms, which is more than three grains and a half. It follows therefore that the weight of 18 grains of powder, which were employed in the experiment, contained more than three and a half of air, which is about the fifth part of 18 grains.

It may also be calculated, how many times this air has been compressed in the powder; but this calculation is more uncertain than the former, because we know not whether this air took up more or less than the fifth part of the space which the powder possessed. But yet it is certain, that though it had even taken up three-fourths of the whole room of the powder, and that the 14 grains of the other matter had taken up no more than the one remaining fourth part, still this air would have been compressed about 300 times. To calculate this, suppose that the space of a cubic foot can hold only 72 pounds of gunpowder, which contain more than 14 pounds of air, by the foregoing calculus; which quantity of air is therefore found inclosed in the three-fourths of a cubic foot. Now this space usually contains but about six drachms of air; wherefore to make it hold 14 pounds of air, which is near 300 times six drachms, it must needs be that the air be compressed near 300 times.

There is reason to believe, that this compression is much greater, because a cubic foot can hold much more than 72 pounds of powder, and because also that the fifth part of the weight must not in appearance possess alone the three-fourths, and all the rest take up no more than one-fourth of the space possessed by all the powder.

We should therefore make no difficulty to believe, that all the effect of gunpowder comes from the air which is compressed therein, and especially in the salt-petre; for we have not observed that brimstone yields air. Possibly also we may find in time, that all other fulminations, ebullitions, and fermentations, that make such surprising motions, are nothing else but air compressed expanding itself.*

An Account of Two Books. N° 122, p. 549.

I. Cours d'Architecture, enseigné dans l'Academie Royale d'Architecture, Premiere Partie; par M. Francois Blondel,† de l'Academie Royale des Sciences, &c. à Paris, 1675, in fol.

* With these very curious experiments and deductions do nearly agree those of Mr. Robins, recorded in the Philosophical Transactions about 70 years afterward; and on which he founded his celebrated theory of the force of fired gunpowder; as we shall hereafter have occasion to notice in its proper place.

† Francis Blondel, a celebrated mathematician, architect, and military engineer, was born at Ribemond, in Picardy, 1617. While yet but young, he was chosen regius professor of mathematics and architecture at Paris. He enjoyed many honourable employments in the navy and army; and was intrusted with the management of several negociations with foreign princes. He arrived at the dignity of maréchal de camp, and counsellor of state, and had the honour to be appointed mathematical preceptor to the Dauphin. Blondel was a member of the Royal Academy of Sciences, director of the Academy of Architecture, and lecturer to the Royal College; in all which situations he sup-

In this first part of architecture are explained the origin and principles of it, and the practice of the five orders, according to the doctrine of Vitruvius and his principal followers, with the doctrine of the three most renowned architects that have written among the moderns, viz. Vignole, Palladio, and Scamozzi.

For the improvement of this art the king of France has instituted an academy at Paris, which meets weekly to confer about the improvement of that subject; teaching the most correct rules of architecture, and so to form a seminary of young architects, to be encouraged by certain prizes for such as shall excel, who are to be sent afterwards, at his majesty's expence, into Italy, to perfect their knowledge and skill, and so to be made capable to serve him in the survey of his buildings all over his kingdom. And because the knowledge of the precepts of architecture is not sufficient by itself alone to make an architect, this skill supposing many other accomplishments, the king has appointed, that in the academy should be taught the other sciences that are absolutely necessary to architects, such as geometry, arithmetic, the mechanics, hydraulics, gnomonics, fortification, perspective, &c.

This first part being merely practical as to the five orders, the author has altogether applied himself to show, with great plainness, which are the most correct practices to be used, beginning with that of Vitruvius, and explaining him by the practice of his best interpreters or imitators, such as Philander, Daniel Barbaro, Serlio, Alberti, &c.

In the second part the author intends more fully to explain what has been but touched on in this first part, and to compare the sentiments which architects have entertained among themselves concerning the best examples of the ancients.

II. Remarks upon two late ingenious Treatises: the one, an Essay touching the Gravitation and Non-gravitation of Fluid Bodies; the other, Observations touching the Torricellian Experiment, so far as they may concern any passages in the Enchiridium Metaphysicum; by Dr. Henry More, London, 1676, in octavo.

Instead of giving an account of this tract, the reader may compare with it what has been not long since published by those two eminent philosophers, Mr. Boyle and Dr. Wallis; the former in an hydrostatical discourse, occasioned by

ported his character with dignity and applause. He was also well versed in the belles lettres, as appears by the comparison he published between Pindar and Horace. Blondel died at Paris in 1686, being the 69th year of his age. His chief mathematical works were: 1. Cours d'Architecture, in 1675; 2. Resolution des Quatre Principaux Problemes d'Architecture, 1676; 3. Histoire du Calendrier Romain, 1682; 4. Cours de Mathematiques, 1683; 5. L'Art de Jetter des Bombes, 1685; besides a new method of fortifying places; also some other separate works, and many ingenious pieces inserted in the Memoirs of the Academy of Sciences, particularly in the year 1666.

the objections of this learned author against some explications of new experiments made by the said Mr. Boyle; which was printed in 1672, among some other tracts, viz. new experiments touching the relation betwixt flame and air; of the positive or relative levity of bodies under water; of the air's spring on bodies under water, &c. The latter, in a discourse of gravity and gravitation, grounded on experimental observations, presented to the Royal Society, printed 1675. These pieces being well considered, and the doctrine of hydrostatics well understood and applied, will make it easy to judge of the whole controversy here treated of.

END OF VOLUME TEN OF THE ORIGINAL.

A Particular Answer of Mr. Isaac Newton to M. Linus's Objections to his Experiment with the Prism, printed in N° 121, *in a Letter from Cambridge, Feb.* 29, 1675-6. N° 123, p. 556. *Vol. XI.*

Sir,—By reading Mr. Linus's letter, which you showed me at London, I retained only a general remembrance, that Mr. Linus denied what I affirmed, and so could lately say nothing in particular to it; but having the opportunity to read it again in N° 121 of the Transactions, I perceive he wished to persuade you that the information you gave him about the experiment is as inconsistent with my printed letters as with experience; and therefore, lest any who has not read those letters should take my silence in this point as an acknowledgment, I thought it not amiss to send you something in answer to this also.

He tells you that, "Whereas you assure him, first, that the experiment was made in clear days; secondly, that the prism was placed close to the hole, so that the light had no room to diverge; and thirdly, that the image was not parallel but transverse to the axis of the prism; if these assertions be compared with my relation of the experiment in N° 80, it will evidently appear they cannot be admitted, as being directly contrary to what is there delivered." His reasons are these:

First, that I said, "the ends of the long image seemed semicircular, which, says he, never happens in any of the three cases abovesaid." But this is not to set me at odds with myself, but with the experiment: for it is there describe to happen in them all; and I still say, it does happen in them. Let others try the experiment, and judge.

Further he says, that "the prism is placed at a distance from the hole in the scheme of the experiment in N° 84." But, what if it were so there? for, that is the scheme of a demonstration, not of the experiment, and would have served for the demonstration, had the distance been put twenty times greater than it is. In the schemes of the experiment N° 80 and N° 82, it is represented close, and close enough in the scheme N° 83. But Mr. Linus thought fit to wink at these, and pitch upon the scheme of a demonstration, and such a scheme too as has no hole at all represented in it. For the scheme, N° 84, (fig. 5. pl. 9) is this; in which the rays are not so far distant from one another at GL, but that the hole, had I expressed it, might have been put there, and yet have comprehended them. But if we should put the hole at x, their decussation, yet will it not be any thing to his purpose; the distance xG or xL being but about half the breadth of a side of the prism ($\frac{1}{4}$AC), which I conceive is not the 20th part of the distance requisite in his conjecture.

3. He says, that "more might be said out of my relation to show that the image was not transverse; for if it had been transverse, I could not have been surprised, as I said I was, to see the length thereof so much exceed the breadth, it being a thing so obvious and easy to be explicated by the ordinary rules of refraction." But on the contrary, it may rather be said, that if the image had been parallel, I could not have been surprised to see the length thereof so much exceed the breadth, it being a thing so extremely obvious as not to need any explication. For who that had but common sense, and saw the whole prism, or a good part of it, illuminated, could not expect the light should have the same long figure upon the wall that it had when it came out of the prism? Mr. Linus, therefore, while he would strengthen his argument by representing me well skilled in optics, does but overthrow it. But whereas, he says, "I could not have been surprised at the length, had the image been parallel, it being a thing so obvious and easy to be explicated by the ordinary rules of refraction;" let any man take the experiment entire as I have there delivered it, that is, with this condition, that the refractions on both sides the prism were equal, and try if he can reconcile it with the ordinary rules of refraction. On the contrary, he may find the impossibility of such a reconciliation demonstrated in my answer to P. Pardies, N° 84.

In the last place he objects, that my saying in N° 80, "that the incident refractions were, in the experiment, equal to the emergent," proves again, that the long image was parallel. And yet that very saying is a sufficient argument that I meant the contrary, because it becomes wholly impertinent, if applied to a parallel image; but in the other case is a very necessary circumstance. What is added, therefore, of P. Pardies, might have been spared, especially since that learned person understood my discourse to be meant of a transverse image, and acquiesced in my answers.

This in answer to Mr. Linus's letter: and now, to take away the like suspicions from his friends, if my declaration of my meaning satisfy not, I shall note some further passages in my letter, whereby they may see how I was to be understood from the beginning, as to the aforesaid three circumstances.

For the day; I express every where that the experiment was tried in the sun's light, and in N° 80, that the breadth of the image by measure answered to the sun's diameter; but because it is pretended, I was imposed upon, I would ask, what the experiment as it is advanced to that which I called the experimentum crucis, can have to do with a cloudy day? for if the experimentum crucis, which is that which I depend on, can have nothing to do with a cloudy day, then is it to no purpose to talk of a cloudy day in the first experiment, which does but lead on to that. But if this satisfy not, let the Transactions, N° 83, be consulted; for there I tell you how, by applying a lens to the prism, the straight edges of the oblong image became more distinct than they would have been without the lens; a circumstance which cannot happen in Mr. Linus's case of a bright cloud.

For the position of the prism; I tell you, N° 80, that it was placed at the sun's entrance into the chamber, and I directed to make a hole in the shutter, and there place the prism, and in the next page I say again, that the prism ABC is to be set close by the hole F of the window EG; and accordingly represent it close in the figure. Also in another page I tell you, that the distance of the image from the hole or prism was 22 feet; which is as much as to say, that the prism, suppose that side of it next the hole, was as far from the image as the hole itself was, and consequently that the prism and hole were contiguous. Also in the next page, where instead of a window shutter I made use of a hole in a loose board, I tell you expressly, that I placed the board close behind the prism. All these passages are in my very first letter about colours, and who therefore would imagine that any one that had read that letter, should so much as suspect that I placed the prism, I say not at so great a distance as Mr. Linus supposes, but at any distance worth considering.

Lastly, for the position of the image, it is represented transverse to the axis of the prism in the figures N° 80, N° 83, and N° 85. And in N° 88, where I made use of two cross prisms, I tell you expressly, that the image was cross to both of them, at an angle of 45 degrees. The calculations also, N° 80, are not to be understood without supposing the image cross. Nor are my notions about different refrangibility otherwise intelligible; for, in Mr. Linus's supposition, the rays that go to the two ends of the image are equally refracted. So for colours; the red, according to my description, falls at one end of the image, and the blue at the other; which cannot happen but in a transverse image. The same position is also demonstrable from what I said in N° 80, about turning the long image into a round one, by the contrary refraction of a second prism, further explained in N° 83. For this is not to be done in Mr. Linus's surmise of a parallel image, and therefore had Mr. Linus considered it, he could never have run into that surmise.

This I suppose is enough to manifest the three particulars; any one of which being evidenced, is sufficient to take away the scruple. And therefore Mr. Linus's friends need not fear but that the further directions I sent them lately for trying the experiment, are the same with those I have followed from the beginning; nor trouble themselves about any thing but to try the experiment right. But yet, because Mr. Gascoin has been pleased to insinuate his suspicion, that I do differ from himself in those directions, I shall not scruple here to reduce them into particulars, and show where each particular is to be found.

1. Then he is to get a prism with an angle about 60 or 65 degrees, N° 80. If the angle be about 63 degrees, as that was which I made use of, N° 80, he will find all things succeed exactly as I described them there. But if it be larger or less, as 30, 40, 50, or 70 degrees, the refraction will be accordingly larger or less, and consequently the image longer or shorter. If his prism be pretty nearly equilateral, such as I suppose to be usually sold in other places as well as in England, he may make use of the largest angle. But he must be sure to place the prism so, that the refraction be made by the two planes which comprehend this angle. I could almost suspect, by considering some circumstances in Mr. Linus's letter, that his error was in this point, he expecting the image should become as long by a little refraction as by a great one; which yet being too gross an error to be suspected of any optician, I say nothing of it, but only hint this to Mr. Gascoin, that he may examine all things.

2. Having such a prism, he must place it so, that its axis be perpendicular to the rays N° 84. A little error in this point makes no sensible variation of the effect.

3. The prism must be so placed, that the refractions on both sides be equal, N° 80: which, how it was to be readily done by turning it about its axis, and staying it when you see the image rest between two contrary motions, as I explained in my late descriptions, so I hinted before, N° 80. If there should be a little error in this point also, it can do no hurt.

4. The diameter of the hole I put $\frac{1}{4}$ of an inch, N° 80, and placed the prism close to it, even so close as to be contiguous, N° 80. But yet there needs no curiosity in these circumstances. The hole may be of any other size, and the prism at a distance from the hole, provided things be so ordered, that the light appear of a round form, if intercepted perpendicularly at its coming out of the prism. Nor needs there any curiosity in the day. The clearer it is the better;

but if it be a little cloudy, that cannot much prejudice the experiment, so the sun do but shine distinctly through the cloud.

These things being thus ordered, if the refracted light fall perpendicularly on a wall or paper at 20 feet or more from the prism, it will appear in an oblong form, cross to the axis of the prism, red at one end, and violet at the other; the length five times the breadth, more or less according to the quantity of the refraction; the sides, straight lines, parallel to each other; and the ends confused, but yet seeming semicircular.

I hope, therefore, Mr. Linus's friends will not entertain themselves any further about incongruous surmises, but try the experiment as Mr. Gascoin has promised. And then, since Mr. Gascoin tells you, That "the experiment being of itself extraordinary and surprising, and besides ushering in new principles into optics, quite contrary to the common and received, it will be hard to persuade it as a truth, till it be made so visible to all as it were a shame to deny it:" if he esteem it so extraordinary, he may have the privilege of making it so visible to all, that it will be a shame to deny it. For I dare say, after his testimony nobody else will scruple it. And I make no question but he will hit of it, it being so plain and easy that I am very much at a loss to imagine what way Mr. Linus took to miss.

Extract of three Letters from Sig. Cassini, on his Observations of the Lunar Eclipse of Dec. 21, 1675, and Remarks on Mr. Flamsteed's Account of the same. N° 123, pp. 561, 563, 564. *Translated from the Latin.*

In this eclipse, two of the principal circumstances we have determined exactly, viz. the middle time of the eclipse and its magnitude. The middle is deduced not only from a comparison of the beginning and end, but also of two equal phases, which are very easily determined, viz. when the distance of the horns was equal to the semidiameter of the moon, taken before the eclipse, being 15′ 18″. That is, when the beginning of the eclipse was estimated at $2^h\ 24^m\ 35^s$ after midnight.

	h.	m.	s.
End of the total darkness, a like penumbra being left as was in determining the beginning	4	15	25
Duration of the whole eclipse comes out	1	50	50
The semiduration therefore	0	55	25
And the middle of the eclipse	3	20	0
A 6th part of the circumference is cut off	2	38	5
And, again, end of total obscuration	4	2	25
The interval or duration	1	24	20

	h.	m.	s.
Its half is	0	42	10
Hence the middle of the eclipse	3	20	15

Agreeing with the former determination to the quarter of a minute.

We agree exactly with Mr. Flamsteed, in the situation of the shadow, and in the magnitude of the eclipse. For it is observed by both of us, that the shadow never went beyond Porphyrites, though it was deeply immersed in the penumbra. Next to Porphyrites is a small whitish mountain, which we then called the Companion of Aristarchus, because it is scarcely distant by its own diameter from Porphyrites. That little mountain was immersed in the shadow at $2^h\ 51^m\ 15^s$; and it emerged at $3^h\ 8^m\ 25^s$; and all the time between it was in the shadow next to Porphyrites. We both of us observed also, that in the height of the eclipse the shadow nearly reached to Corsica; yet was never covered by it, a small interval being left; the distance of which limit being taken from the nearest limb of the moon, was 8′ 17″; whereas Mr. Flamsteed found the distance of the island itself a little farther from the same limb, being 8′ 39″. We also observed the island, or rather peninsula, Macra to be a long time adjacent to both the shadows. We noticed that this began at $3^h\ 28^m\ 15^s$, and that it continued at the same distance for a quarter of an hour.

A Transit of the Moon above Jupiter, Feb. 28, 167$\frac{5}{6}$, Old Style, in the Morning, observed at Greenwich. By Mr. Flamsteed. N° 123, p. 566.

Correct time of Clock. h.	m.	s.	Distances and Altitudes.		
4	20	15.	... Jupiter's distance from the moon's bright limb......	26′	9″
4	47	0.	... The moon's diameter taken	31	30
4	49	30.	... Jupiter from the nearest cusp	26	28
4	52	15	... Jupiter had passed over a right line drawn through the cusps, by a 10th part of the distance, or about 3′, as conjectured by the eye through the tube.		
4	56	0.	... Jupiter from the cusp	27	33
5	1	15.	... From a right line through the cusps.............	7	53
5	3	30.	... From the cusp	28	22
5	7	25.	... From the right line	9	58
5	10	50.	... From the same	11	55
5	15	50.	... From the cusp	30	27
5	21	20.	... From the farther limb, dubious	62	4
5	26	0.	... From the nearest cusp	33	0
5	31	25.	... From the right line through the cusps	20	9

Correct time of Clock.			Distances and Altitudes.		
5^h	37^m	0^s. .	From the cusp	36′	15″
5	41	10. .	The moon's altitude being $10\frac{1}{2}°$, the diameter about	31	53
5	48	30. .	Diff. of altitude of Jupiter and the moon's lower limb	23	1
5	52	40. .	Jupiter from the nearest cusp	41	40
6	9	40. .	From the cusp, dubious	47	29

An Account of Books. N° 123, p. 567.

I. Ἀρχιμήδους τοῦ Συρακουσίου Ψαμμίτης, καὶ Κύκλου Μέτρησις: Εὐτοκίου Ἀσκαλωνίτου εἰς αὐτὴν υπόμνημα, &c. Cum Versione et Notis Joh. Wallis, SS. Th. Doct. Geometriæ Professoris Saviliani. Oxonii è Theatro Sheldoniano, 1676.

Though this tract of Archimedes's Arenarius has been formerly twice printed in Greek, and thrice in Latin, yet the learned Dr. Wallis saw reason for publishing another edition; presenting us with many emendations in the original, and with a new version in Latin, also adding some short explanatory strictures. Indeed the book seemed to deserve these pains, being not only an elegant and acute piece, worthy of Archimedes, but also an excellent monument preserving both a piece of remote antiquity, as is that of the hypothesis of Aristarchus Samius revived by Copernicus, and that of the Doric dialect in prose. Besides, it exhibits the foundation laid of the art of numbering, or rather noting of numbers, now in use amongst us, with Saracenic or rather Indian cyphers. And it accommodates those numbers α, β, γ, δ, ε, &c. not only to numbers proportional in a decuple ratio; but also to any others, in any ratio whatever, that are in a continual proportion from the unit: and they are the same with what is commonly called unit, root, quadrat, cube, biquadrat, viz.

$$\alpha \quad \beta \quad \gamma \quad \delta \quad \varepsilon \quad \zeta \quad \eta, \text{ \&c.}$$
$$1 \quad a \quad aa \quad a^3 \quad a^4 \quad a^5 \quad a^6, \text{ \&c.}$$

This book exhibits a number exceeding that which is equal to the number of the sand, capable to fill up, not only the whole earth and its cavities, but also the whole world.

To this tract of the number of the sand, is added that other of the same Archimedes, touching the dimension of a circle, because it is several times quoted in the former, as the foundation of his calculus; nor did it want emendation. To it is annexed Eutocius's short commentary on the said dimension, which exhibits a specimen of the form and manner, wherein the later Greeks used to write their comments on the more ancient authors; and it shows also how laborious it was to make multiplications, divisions, and extractions of roots before the use of the Indian cyphers was introduced, as well as in what manner they were performed.

II. Observationes Medicæ circa Morborum Acutorum Historiam et Curationem ; Auth. Thoma Sydenham, M. D. Lond. in 8vo.

Sydenham's works are, or ought to be, in the library of every physician.

III. De Consensu Vet. et Novæ Philosophiæ Libri IV, seu Promotæ per Experimenta Philosophiæ Pars Prima: Authore J. B. Du Hamel, P. S. L. et Regiæ Scientiarum Academiæ à Secretis, in 12mo.

In this second and considerably augmented edition the learned author performs four things, in so many books.

In the first, he gives an account of the principles of the Platonic philosophy, and shows the difference between it and the Peripatetic; delivering in the same the natural theology of the Platonists, and discoursing, from their principles, of the existence of God, and his providence and concourse; then of the origin as well as the spirit of the world.—In the second book he explains, first the principles of Aristotle, treating at large of the nature and origin of forms. Next, he treats of the Epicurean philosophy, as less difficult and more obvious; discoursing of atoms, their nature and figures of continuity, and the manner of the cohesion of atoms, as also of vacuity, &c.—Thirdly, he explains the Cartesian principles; where he has first a long discourse about the nature of a physical body, endeavouring to evince, that the essence of it consists not in three dimensions, and to show that the idea of these (than which Descartes contends we can have no other of a body,) is the idea only of a mathematical, not a physical body. Secondly, he treats largely of the nature and law of motion. Thirdly, of the elastic motion, with its causes, and the manner in which it is communicated.—In the third book he treats amply of the four elements, commonly so called, fire, air, water, and earth: where occur many considerable observations concerning fire and air. The Epicurean notion of fire is here explained, as well as the Cartesian, and those particulars discussed, that seem difficult in the latter. There are also recited many phænomena of flame, and the latent fire in lime and other bodies ingeniously discoursed of: also, what is the nature and use of the air, what the nature of the æther, with the many experiments about the spring of the air, made in the Machina Boyliana, in England and elsewhere, &c.—In the fourth are explained the principles of chemistry, the mixture and dissolution of bodies, fermentation, &c. This also is full of new experiments and observations, made here, in France, and other countries.

IV. Of Education, especially of Young Gentlemen, in two parts, the second impression with Additions; Printed at the Theatre, Oxon. 8vo.

That learned knight Sir Henry Wotton, did long since, at the end of his Elements of Architecture, promise, as devoted to the service of his country, a philosophical survey of education, which is indeed, says he, a second building or repairing of nature, and a kind of moral architecture. This he promised An. 1624; and he made many essays, and began some chapters, but could never bring his design to so much perfection, as could give satisfaction to his own mind and intentions. In this our author, who is pleased to conceal his name, is very full and punctual, with instructions proper for all conditions of human life, particularly for the generous: having reduced the best of ancient and modern advisos into a compact method, and mixed it with a very great variety of his own seasonable suggestions.

V. Bathoniensium et Aquisgranensium Thermarum Comparatio, variis Adjunctis illustrata, à R. P. Lond. 1676, 8vo.

A treatise on the Bath and Aix la Chapelle mineral waters.

VI. Vinetum Britannicum, or a Treatise of Cider, and such other Wines and Drinks, as are extracted from all manner of Fruits growing in this Kingdom; with the Method of Propagating all sorts of Vinous Fruit Trees. And a Description of a New-invented Engine or Mill, for the more expeditious and better Making of Cider. Also the Method of making Metheglin and Birch Wine; with Copper-plates. By J. W. Gentleman, in 8vo.

This is done by the worthy author of Systema Agriculturæ in folio, and is esteemed an ingenious and useful work.

Observations made during Travels from Venice, through Istria, Dalmatia, Greece, and the Archipelago, to Smyrna. By Mr. Francis Vernon. N° 124, p. 575.

In Istria we saw Pola, an ancient republic. There remains yet an amphitheatre entire: it is of two orders of Tuscan pillars, placed one over another, and the lower pillars stand on pedestals, which is not ordinary; for commonly they have nothing but their bases to support them. There are besides a temple dedicated to Rome and Augustus, a triumphal arch built by a lady of the family of the Sergii, in honour of some of her kindred, who commanded in these countries; besides several inscriptions and ancient monuments in divers parts of the town.

In Dalmatia I saw Zahara, anciently called Jadera, which is now the metropolis of the country. It is now very well fortified, being encompassed on three sides with the sea, and that part which is toward the land well defended by all the contrivances of art, having a castle and a rampart of very lofty bastions to guard it. Here are several ancient inscriptions. What is most worth seeing in Dalmatia, is Spalatro; where is Dioclesian's palace, a vast and stupendous fabric, where he resided, when he retreated from empire. It is as large as the whole town; which indeed is built out of its ruins, and from which it is said to take its name. The building is massive; within it is an entire temple of Jupiter, of 8 sides, with noble porphyry pillars, and admirable cornices. Before it is a court, adorned with Egyptian pillars of that stone called pyropoicilos, and under it a temple, now dedicated to St. Lucia; and in the town several fragments of antiquity, with inscriptions and other things, worth observing.

Four miles from Spalatro lies Salona, which shows the ruins of a large town. About as much farther from Salona stands Clissa, on a rocky hill, an eminent fortress of the Venetians, which is here the frontier against the Turks. At Lesina there is nothing remarkable; only that Biondi, who wrote our English history, was a native of it. Trau is ancient, and shows good signs of its being so. Here I spoke with Doctor Stasileo, who published the fragment of Petronius Arbiter; the manuscript of which I saw. Passing the gulf of Budua, I saw the mountains of Antivari, the plain of Durazzo and Apollonia, and came to Sassino a small island, from whence we could see the town of Valona, and the mountains Acroceraunia, now called the mountains of Chimæra.—I staid a fortnight at Corfu, and had time to view all that was considerable in the island, particularly the gardens of Alcinous, or the place where they are supposed to have been, now called Chrysida; a most delicious situation: also the ancient port, now called Necrothalassa, and several foundations of ancient fabrics. In Zante I saw but few antiquities. What is modern is very flourishing, and the island rich and plentiful.

From Zante I went to Patras, a town in Achaia, of good note among the ancients. Near it is a great mountain, mentioned in Homer by the name of Petra Olenia. In the town are several massive ruins, and among them the remains of a large church, dedicated to St. Andrea, who, they say, was martyred there. The plain about it is very fruitful, full of springs and rivulets; finely wooded with olive-trees, cypresses, orange and lemon-trees. The citrons here are counted the best in the Turkish empire, and are sent as presents to Constantinople. Indeed all their fruits are in very good esteem.

In Athens I spent two months. Next to Rome, I judge it the most worthy to be seen for antiquities. The temple of Minerva is as entire as the rotunda. I was three times in it, and took all the dimensions, with what exactness I could; but it is difficult, because the castle of Athens, in which it stands, is a garrison, and the Turks are jealous, and brutishly barbarous, if they perceive that any one measures it. The length of the Cella, or body of the temple without side, is 168 English feet, the breadth 71. The portico of the Doric order, which runs round it, has eight pillars in front, and 17 on the sides; the length of the portico is 230 English feet. The fust or shaft of the pillars, is 19¼ feet in circumference: the intercolumnium, 1¼ of the diameter of the pillars.—The temple of Theseus is likewise entire, but much less, though built after the same model: the length of its cella is but 73 feet, the breadth 26. The whole length of the portico, which goes round it, 123 feet. It is a Doric building, as is that of Minerva; and both are of white marble.—About the cornice, on the outside of the temple of Minerva, is a basso-relievo of men on horseback, others in chariots; and a whole procession of people going to a sacrifice, of very curious sculpture. On the front is the history of the birth of Minerva.—In the temple of Theseus on the front, within side the portico, at the west end, is the battle of the Centaurs; and at the east end there seems to be a continuation of that history: There are also several figures of women, who seem to be Pirithous's bride, and those other ladies that were at the wedding. On the outside the portico, in the spaces between the Triglyphs, are represented several of the feats of Theseus, most in wrestling with several persons, in which he excelled: all his postures and looks are expressed with great art. Others are monsters, which he is made encountering with, as the bull of Marathon, the bear of Calydon, &c.

There is a temple of Hercules, a round fabric, only of six feet diameter, but neat architecture. The pillars are of the Corinthian order, supporting an architrave, and frieze, wherein are done in relievo the labours of Hercules. The top is one single stone, wrought like a shield, with a flower on the outside, which rises like a plume of feathers.

The tower of Andronicus Cirrhestes is yet standing; it is an octagon, with the figures of eight winds, which are large, and of good workmanship; and the names of the winds remain legible in fair Greek characters, Apeliotes, Euros, Boreas, Sciron, Zephyros, each wind pointing to its quarter in the heavens; and the roof is made of little planks of marble, broad at bottom, and which meet all in a point at top, making an obtuse pyramid, of about 32 or 36 sides. In the castle there is a delicate temple of the Ionic order; the work of which is very fine, and all the ornaments most accurately engraven: the length is 67 feet, the breadth 38.—The pillars which remain of a portico of the Emperor Adrian, are very stately and noble: they are of the Corinthian order, and above 52 feet in height, and 19½ in circumference, and are channelled; 17 of them are now standing, with part of their cornice. The area of the building to which they belong is near 1000 feet in length, and about 680 in breadth.

Without the town, the bridge over the Elissus has three arches, of solid stone work; the middlemost being near 20 feet broad. There is still to be seen the stadium, whose length I measured, and found it 630 feet, near to what the precise measure of a stadium ought to be, viz. 625.—Towards the southern wall of the castle, are the remains of the theatre of Bacchus, with the portico of Eumenes, which is near it; the semidiameter is about 150 feet. The whole body of the stage 256.

Thebes is a large town about 50 miles distant from Athens, but few antiquities are found in it, only some inscriptions and fragments of the old wall, and one gate, which they say was left by Alexander, when he demolished the rest.

Corinth is two days journey distant; the castle or Acrocorinthus is standing, and is very large. Most of the town is demolished, and the remaining houses are scattered, and at a great distance from one another. So also is Argos, whose compass is about four or five miles, as the houses now stand, but if they stood together they would scarcely exceed a good village. Napoli della Romilia, but a few leagues distant from Argos, is a large town and full of inhabitants, and where the bashaw of the Morea resides.

Sparta is quite forsaken; Mestra being the town which is inhabited, four miles distant from it. Large ruins are seen thereabout, almost all the walls, several towers, and foundations of temples, with pillars and chapiters, being demolished; a theatre remains pretty entire. It might have been anciently about five miles in compass, and about a quarter of a mile distant from the river Eurotas. The plain of Sparta and Laconia is very fruitful, long, and well watered. It may be about 80 miles in length. The mountains on the west side of it very high, inhabited by the Maniotes. But the plain of Calamatta, which anciently was that of Messene, seems to be richer. Corona is very abundant in olives. Navarrino, thought to be the ancient Pylos, has a strong castle, fortified by the Turks, and is the best port in all the Morea. Alpheus is by much the finest and the deepest river, and justly extolled by all the ancient poets, and chosen for the seat of the Olympic games. The plains of Elis are very pleasant and large, fit to breed horses in, and for hunting; but not so fruitful as that of Argos and Messene, which are very rich. The best woods I saw in Peloponnesus are those of Achaia, abounding with pines and wild pear, the ilex and esculus-trees, and where water runs, with plane-trees.

Arcadia is a pleasant champaign, and full of cattle, but is encompassed with rugged hills. Lepanto is very pleasantly seated on the gulf, which runs up as far as Corinth; and without the town is one of the finest fountains in Greece, very rich in springs of water, and shaded with large plane-trees, not inferior to the spring of Castalia on Mount Parnassus, which runs through Delphos, except in this, that the one was chosen by the Muses, and the other not; and that poetical fancies have given immortality to the one, and never mentioned the other. Delphos itself is very oddly situated on a rugged hill, to which is an ascent of about two or three leagues, and yet that is not a quarter of the way to come up to the peak of Parnassus, on the side of which hill it stands. It seems very barren to the eye; but the fruits are very good where there are any.

The wines are excellent, and the plants and simples found there very fragrant, and of great efficacy.

About Lebadia, and all through Bœotia, the plains are very fertile, and make amends for the barrenness of the hills which encompass them; but in winter they are apt to be overflown, and turned into lakes, which renders the Bœotian air very thick. These vales I found much planted with cotton, and sesamum, and cummin, of which they make great profit, carrying on a great trade at Thebes and Lebadia.

From Thebes I went into the island of Eubœa or Negropont, and saw the Euripus, which ebbs and flows much after the manner of our tides; only the moon, and sometimes winds, make it irregular. The channel, which runs between the town and a castle, which stands in an island over against it, is about 50 feet broad, and there are three mills on it which show all the changes and varieties that happen in the current. Near the Euripus, and opposite to the town, is shown a port, which they say was Aulis, and it is not improbable; for it must be thereabouts. Between Negropont and Athens is a high hill, called Agiomacuri, formerly very dangerous, but now guarded by Albaneses; it is part of Mount Parnassus: and near it on the left hand lies Mount Pentelicus, from whence the Athenians anciently brought their stone; where is a new convent of Calogeri, one of the richest of all Greece.

In going from Athens by sea, I embarked in a port, which lies just by Munychia; that which they call Porto Piræo lies behind it, a mile distant, which is a large port, able to contain 500 vessels. The ruins of the town are yet remaining, and of the walls, which joined it to the city of Athens. I sailed by Porto Phalero, the ancient haven of Athens, which is rather a road than a port.

Advertisements on the Vinetum Britannicum, mentioned in the last foregoing Number, sent to the Editor by the Rev. Dr. J. Beal, Rector of Yeovil in Somersetshire, and one of his Majesty's Chaplains. N° 124, p. 583.

There is nothing in this paper worth preserving.

The Lunar Eclipse, An. 1676, *Jan.* 1, *in the Morning, N. S. observed at Dantzic by M. Hevelius.* N° 124, p. 589.*

	h	m	s
Beginning of the penumbra	2	36	40
Beginning of the eclipse	3	30	0
The penumbra vanished	6	8	0

* See on this eclipse Mr. Flamsteed's observation, No. 121 foregoing; also his and Cassini's remarks, in this same No. 124, just preceding.

In this eclipse it is to be noted, that all the sections never entirely covered Mount Porphyrites: but it remained clear in the very limit of the shadow, even in the greatest obscuration.

An Account of some Books. N° 124, p. 591.

I and II. Memoires pour servir à l'Histoire Naturelle des Animaux; to which is joined another tract totally different, entituled, La Mesure de la Terre. A Paris, 1671, in fol.

These two works came from the joint body of the Royal Academy of Sciences at Paris, being part of their memoirs. They have been partly noticed already in these Philosophical Transactions, viz. Nos 49 and 112, but now again more particularly.

These memoirs give us the anatomical descriptions of 13 species of exotic animals; of which five, viz. a cameleon, castor, dromedary, bear, and gazelle, were formerly published, and described by the same persons in a book in quarto, printed at Paris, 1669; which now are reprinted here in a more magnificent manner, and augmented with the number of eight species, which are, two lions and a lioness, a chat pard, supposed to be engendered by a leopard and a she cat, a sea fox, a lupus cervarius or lynx, an otter, a civet cat, an elk, and a coati mondi of Brasil.

First, they discourse of two lions and one lioness, and, among other observations, they remark that one of the two male lions sickened of a surfeit: as having been informed, some months before he died, that he would not only refrain coming out of his den, but hardly eat; and that therefore some remedies were ordered for him, and among the rest, not to eat any other flesh but that of young animals, and to eat them alive. To which his keepers, to render this food the more delicate for him, added the extraordinary preparation of fleaing lambs alive, and to let him eat divers of those: which at first recovered him by restoring his appetite and some chearfulness. But yet, say they, this food in all appearance bred too much blood, and such as was too subtile for this animal, to which nature has not given the industry or care of fleaing those creatures it feeds on, it being probable that the hair, wool, feathers, and shells, which all animals of prey devour, are a kind of necessary corrective to prevent them from filling themselves by their greediness with too succulent a food.

Next comes the chat pard, wherein they chiefly note the defect of spermatic vessels, and of other parts absolutely necessary to generation, which they found did not proceed from castration, but from some other cause: where they take occasion to observe, that the sterility which is ordinary in some of those animals that are born of two different species, must have in this subject a very particular cause. For, say they, that which renders mules sterile is not the defect of any of the organs necessary to generation, since the difference which may be found in the conformation of the matrix of a mare and of that of a she ass cannot, as some pretend, be a ground of this cause of sterility; the mare, in which something is deficient that is found in the she ass, not being destitute of any of the parts absolutely necessary to engender, because it does engender; and the difference of the organs being not the cause of barrenness, forasmuch as the difference of organs which is between the species of horses and asses, hinders not the breeding of mules, which issue from the mixture of those two species. Whence Aristotle, following Empedocles, imputes this defect only to the temperament of those animals, whose parts have contracted a hardness that renders them incapable to contribute to a new mixture; so that if it be true that most of the animals which are born of the mixture of two kinds, are notwithstanding fruitful, they are inclined to believe that the conformation of this chat pard was peculiar and accidental, and that the defect of the parts which it wanted, and which made it incapable of engendering, proceeded not from that mixture of species, which by changing the conformation of the parts cannot so spoil the same as to render it unfit for the functions, and is yet less capable to make a mutilation; but may more easily cause some vice in the temperament, which is a very natural sequel of mixture; and lastly, that it is probable, that if the mule be the only animal which the confusion of species renders sterile, there is something particular in those animals that have engendered it, which is not found in others, and that is perhaps, as Aristotle thinks, the hardness of the matrix in mares and asses, which like an earth is rendered sterile by driness; whereas that reason has no place in leopards, foxes, and others, which are animals fecund enough to transmit to their offspring the strong dispositions they have for generation, notwithstanding the resistance which the mixture of different species may bring. [Why some hybridous animals are prolific, and the generality of them not, is a matter wholly inexplicable.]

The third is the sea fox,* in whose stomach they found a branch of the sea herb varec, and a fish of five inches long, without its head, scales, skin, and bowels, all having been consumed, except the muscular flesh, which was yet entire. And as to its guts, they observe, that the upper part of them had a peculiar structure, and instead of the ordinary circumvolutions of guts, the cavity of these was distinguished by many transverse separations, composed of the membranes of the intestine turned inwards, which separations were half an inch distant from one another, and turned helically, like a snail shell; which may

* A species of shark. Squalus vulpes. Linn.

be considered as the reason that the food is a long while in passing, though the way be but short.

The fourth is the female lynx, which is one of the animals that have short guts, of which kind the lion is also one, whose guts they found hardly longer than three times the length of his body: which argues speedy digestion and great voracity.

The fifth is the otter, the difference of which from the castor they have very carefully observed; as they have also the peculiar connection of the spleen of the otter, which they say is different from that of almost all other animals, in which that viscus is generally fastened to the stomach, whereas in this otter it was fast to the epiploon. And as to a foramen ovale they have found no appearance in this otter, that it had ever had a hole that could give passage to the blood from the vena cava into the arteria venosa; which, they say, agrees well enough with that remark which all the ancients have made, viz. that the otter is constrained from time to time to rise above the water to breathe, which a castor does not, as having a far greater facility to be a long while without respiration.

The sixth is the civet cat, which they were glad they had the opportunity to compare with a castor, as those two animals agree in those organs that are very peculiar to them, which are the receptacles wherein that liquor is collected that is so remarkable for its scent, but is very sweet in the one, and very unpleasant in the other. Which made them search whether there was not some particular reason of this diversity of smell; but to them it appeared not that there was any other cause than the diversity of the temperament of these animals, the civet cat being hot and dry, drinking little, and living in hot and dry countries; but the castor, living now in the water then upon the earth, and being a very moist creature, has not heat enough to concoct and perfect its humidity.

They had, it seems, two of these cats, a male and a female, which were so like one another outwardly, that there was not so much as any distinction of sex that appeared; the male, on the dissection, being found to have its genitals hid and shut up within, and the vessel that contains the odoriferous liquor being altogether alike in both. Which vessel is a pouch or sack under the anus, not under the tail, as Aristotle puts it in his hyena, which they make the same with the civet cat, and is different from the matrix, both very accurately described by them. As to the odoriferous liquor, they found it come forth in the male as well as the female, out of a great number of glandules that are between the two coats that compose the pouches, which were in the male very large, and very small in the female; the male yielding also a civet more pleasant than the female, though authors generally affirm the contrary. They found not that the smell of the civet becomes more perfect by being kept a while, nor that it is of an offensive smell when new, as Amat. Lusitanus affirms; this smell not seeming to them better after a year's time than at the time of the dissection.

The seventh is the elk, of which they examine very solicitously its claws, together with the tradition of this animal curing itself of the epilepsy, to which it is said to be very subject, by putting one of his feet into his ear; whence the claw of that foot is also much celebrated among the vulgar, as a specific against that distemper. Of its brain they take notice, that the glandula pinealis was of an extraordinary size; and consider that lions, bears, and other bold and fierce animals, have that part so very small that it is hardly discernible, and that the same is exceedingly large in those that are very timorous, as the elk; this animal being esteemed to be so fearful that it even dies of fear when it has received the slightest wound, it having been observed that it never survives when it sees any of its own blood.

The eighth and last is the coati mondi, a Brasilian animal, recorded by Margrave, de Laet, and others, in whose books the description of that animal differs only in the description here made of it, that in the former the authors describe not their teeth, which have a peculiar conformation, nor the spurs on their feet; and that they make its tail much longer than the whole body; which in this coati was but short in comparison; but it may have been eaten off by the animal itself, as de Laet says, that this kind of creatures are wont to gnaw their tail,* and sometimes quite off, which when they do they die of it.

The other treatise, being a new and very accurately performed Mensuration of the Earth, has been fully described in N° 112 of these tracts.

III. Britannia Antiqua illustrata; or, the Antiquities of ancient Britain, derived from the Phœnicians, &c. The first volume. By Aylett Sammes, of Christ's College in Cambridge, since of the Inner Temple, London, 1676.

The learned and curious undertaker of this great work has endeavoured, in this his first volume, after Bochart, to attribute the first discovery of Britain to the Phœnicians, and to make a German nation, and not the Gauls, the first planters of it, and to impute that near agreement which was between the ancient Britons and Gauls, in point of language and customs, not to their being originally the same people, but to the joint commerce with the Phœnicians, the ancient and great navigators throughout the world.

From this commerce with the Phœnicians he deduces the original trade of this island, the names of places, offices, and dignities, as also the language, manners, idolatry, and other customs of the primitive inhabitants, illustrating

* Monkeys are also apt to gnaw their tails.

many old monuments out of approved Greek and Latin authors; delivering also a chronological history of this kingdom, from the first traditional beginning until the year of our Lord 800, when the name of Britain was changed for that of England; together with the antiquities of the Saxons as well as the Phœnicians, Greeks, and Romans, having prefixed a curious map of the ancient world, representing the progress of the Phœnicians in their remote voyages, and the countries which they discovered, with the names they imposed on them.

To observe some of the things that are most suitable to the nature of these tracts; I shall first take notice of that inquiry, whether Britain was ever part of the continent? which he answers in the negative by various arguments, particularly that which, from the likeness of the soil, concludes a conjunction of earth: and shows that it was nothing more than the same vein of ground which ran under water from one country to another, which he illustrates from philosophical considerations.

Secondly, I shall take notice of the most ancient philosophical order of people in Britain, the bards, a Phœnician appellation of men, who in poetical strains were wont to sing, not only of the praises of the Gods, the essence and immortality of the soul, the virtues of great men, but also of the works of nature, the course of celestial bodies, and the order and harmony of the spheres; though afterwards by their degeneracy they gave an opportunity to the druids to get the upper hand of them; who yet notwithstanding did not abolish all the customs and doctrines of the bards, but retained the most useful parts of them, of which that of the immortality of the soul was one, to which they added the soul's transmigration, according to the opinion of Pythagoras; about whose time, or a little after, it is believed that the Greeks entered this island. These druids had, after the bards, a government that was universal over the whole country, both in civil and religious affairs; and they were exempt both from the services of war, and from paying any taxes; by which immunities many were invited to enter themselves into that order and discipline. It seems this order of men was in so great reputation, that the Gauls, though they had themselves druids in their country, yet sent their children into Britain, to be instructed in the mysteries of the druids here.

Thirdly, that as the Britons were originally a branch of the Cimbri, a people of Germany, who anciently came and seated themselves in Britain; so the Saxons that were invited hither after a revolution of so many ages from that time, were a true branch of those very Cimbri that had seated themselves so long ago before them in this island. Nor need it to be wondered that if the ancient Britons and the later Saxons be derived from the same stock, (the Cimbri), they should understand nothing of each other's language at the Saxons entrance: for the continuance of time, and the mixture of the Britons with the Phœnicians, Grecians, Gauls and Romans, in several ages, might be the cause of that difference; though it is not to be doubted but that there are many words in the British tongue which agree with the Saxon, and which probably they had in use long before the arrival of the Saxons themselves.

Of Shining Flesh. By Dr. J. Beal. N° 125, p. 599.

At Yeoval, in Somersetshire, a neck of veal, which seemed to be well coloured, and in good condition in every respect, being killed in the evening of the day before, was hung to a shelf in a little chamber. The following day in the evening, the veal shone so bright that the woman of the house was affrighted. She calls up her husband; who hastens to the light, as fearing fire, and seeing the light come only from the flesh, he caught it in his left hand, and struck it with his right hand, as endeavouring to extinguish the flame, but without effect; as the flesh shone as much as before, if not more, and his hand with which he struck the flesh appeared all in a flame, as bright and vivid as the flesh of the veal was, and so it continued, whilst he went from place to place, showing it to others. And though he thrust his blazing hand into a pail of water, the flame was not extinguished; but his hand shone through the water: at last he took a napkin and wiped his hand, till he wiped off all the light. The next day the veal was dressed, and it eat very well.

Another time boiling the chitterlings and feet of a porker, and when cold, putting them into souse liquor or pickle, in a low room to the north, which had little light at mid-day, and was very dark, as soon as night set in: in four days all those parts of the guts, and the claws of the feet, which floated on the top of the pickle, began to shine, but the parts immersed under water gave no light; the light increased daily more and more in all the parts that floated. In three days more, the light seemed as bright as the clearest moon-shine; and thus it continued to shine, but fainter and fainter, and in fewer parts, almost a week longer; for, being often tumbled about, by degrees the whole sunk into the pickle, and then all the light ceased. Whilst the light was vivid, by rubbing the hands on the shining part, a strong light was communicated to them. Suspecting this luminousness might be owing to the pickle, the pork was wiped dry with a napkin, yet the flame was rather increased than diminished. No perceptible warmth was found in these parts. I noted that by this acquired blaze, the face and hands appeared a great deal larger than they really were.—The pickle in which the pork was put, was made only of pure water, bran, and bay-salt, and so far from shining, that it quenched the light by degrees of some shining flesh; whereas the mackerel-pickle, mentioned in N° 13, which was boiled with a mixture of sweet herbs, by a little stirring became so luminous, that a drop of it in the palms of children's hands, appeared as broad as a shilling, or broader.

It is to be noted, that the mackerel-pickle was thick and not transparent, till it was stirred and flaming; the pork-pickle was clear or transparent, yet shined not in any part.

A Discourse concerning the Spiral, instead of the supposed Annular, Structure of the Fibres of the Intestines. By Dr. Wm. Cole. N° 125, p. 603.

The mechanical reason of the peristaltic motion of the intestines, is by some anatomists deduced principally from annular fibres, constituting according to the received doctrine, one of their coats; others are of opinion, that they might be rather numerous, though small, sphincter-muscles, than single fibres, to which that motion is to be attributed; muscles being in most, if not all other instances, acknowledged to be the adequate instruments of motions analogous to this; and fibres, though absolutely necessary, yet being no otherwise so than as a number of them being collected and fitly disposed, they constitute a muscle.

This conjecture seemed to me more probable, than the vulgarly received opinion: but yet several difficulties occurred to me, which ever of the two suppositions were adopted.—For, first, I conceived it might be doubted, how the actuating matter or impression should be transmitted from one fibre or muscle to another, along the whole tract of the intestines; since nature's usual way for the propagation of animal motion, is by a continuation of vessels, or at least fibres, whether they be concave or not, from the part where it begins to that to which it is imparted, either for the conveyance of some actuating substance, or the communicating an impression. But there being in the annular supposition no such continuation of vessels or fibres, a lateral contiguity being all that can be pretended, it might perhaps be urged, that the influent and moving matter might be transmitted by mutual inosculations between the contiguous fibres along their sides; which, if there be no communication by vessels, was the only way I could solve the doubt; for the notion of an impression would hardly serve, since it seemed not evident, that there could be in that supposition of a continuity of fibres, tensity enough in the intestines to carry on such a motion. But to this I considered—Secondly, That such a supposition seemed not very agreeable to nature's methods, which ordinarily makes use of vessels, for the transmission of the fluid substances in the bodies of animals, not lateral emissaries; except where some great inconvenience is designed to be prevented by the help of such conveyances; as, for instance, by the anastomoses, discovered to be between veins and between arteries, in which vessels the blood running with a large and rapid stream, should any of them chance to be obstructed, the circulation so necessary to life must needs be intercepted, without some lateral conveyance of it into others of the same kind: which inconvenience yet I supposed would hardly be alleged in the present case; that fabric of those vessels seeming to be designed for extraordinary emergencies, but these being according to the present supposition, the constant and necessary ducts of this actuating matter. But nevertheless—Thirdly, It seemed difficult to solve this intestinal contraction, though these lateral apertures were supposed: for if fibres, whether considered as single or as constituting a muscle, be contracted according to their length from some influent matter, it must be from a distension of them in breadth; and, in order to that, this matter must undergo some confinement in the part to be distended; but if they have lateral perforations, how can it be supposed that such a distension can happen, when the matter designed to effect it has so ready a passage forth, especially its determination from the impelling cause being in right lines downward? But indeed I think no anatomists have observed, that muscles, supposing these such, receive their actuating matter in at their sides, or, when their motion ceases, send it forth that way; but all so far as has been observed, are fenced with a considerably compact and comparatively impervious membrane.—Fourthly, I considered that all muscles are observed to have two tendons, one at each extremity, by the approach of one whereof toward the other, its motion, which is contraction, is performed; but it seems hard to conceive, that these tendons should coincide, as in this supposition they must, and if they do, I presumed it would be difficult to determine what part of these circular muscles the tendons are, and where the motion should begin in each; it being observed, that all muscles are fastened to some, either simply or comparatively, immovable part, toward which they move, and by which the instinct of motion is from the nerves conveyed to them: but no anatomists having discovered that any one part of these muscles, or moving fibres, whichsoever they be, has any stricter cohesion than another with any of the adjacent parts, I conceived I might be allowed the liberty to doubt of the hypothesis, especially if I could satisfy myself better by another. For instead of those solutions, there occurred to me a third way, which seemed more mechanically adjusted to solve the phenomenon, viz. That those fibres, which have been esteemed annular, might perhaps be spiral, and so be continued down in one tract to the lowest extremity of the intestines: which if true, it seemed probable to me, that when either a bare motion shall be impressed on them at their beginning, or any substance impelled into them, the motion must be successively continued all along their tracts, and must therefore whilst it lasts, by abbreviating these fibres, straiten the intestine, and so thrust forward what is contained in it, especially if they proved to have a muscular fabric. I afterwards put this to the experiment, which I first made in a portion in the upper intestines of an ox, which, by reason of their largeness of proportion to those of most other species of animals, seemed fittest for the trial; afterwards in those of sheep and calves, beside the repetition of it in oxen, and not only in the smaller intestines, but in the colon and cœcum also. The circumstances and result of which trials are as follow:—

To effect a due disjunction of the membranes and fibres I caused the intestines of oxen to be boiled five or six hours, of sheep 4; whereby the parts were so loosened, that the two outward coats, viz. the common one, and that consisting of right fibres, were easily separated soon after taken out of the water, leaving those reputed annular ones naked. These at the top of the intestine I attempted to separate from each other; and when those which had been decurtated by the unequal cutting of the knife, were taken off, I found, first, that I could not separate a single fibre from its fellows to any considerable distance, all of them appearing to be very small, and in the separation running smaller and smaller, and by reason of their implication or stricter cohesion one with another easily breaking; but a congeries of them, which at first view would resemble a pretty large fibre, would without much difficulty rise together; the very small constituting fibres of which clusters yet, if the boiling had been very long continued, whereby the compages were very much relaxed, would in the raising be very apt to separate from each other, and appear distinct, by reason of their insertions, mentioned below. Secondly, that when beginning at the top, I attempted the separation of one of these supposed annular clusters of fibres towards my right hand, a whole ring would come off together, which at first staggered me as to my forementioned conjecture; but trying it towards my left, I found, for the most part, I could easily enough unravel that cluster to a considerable length, viz. that of sometimes more than two or three spans, before breaking, of the whole cluster I mean, which yet at last it would be subject to. For, thirdly, though those convolutions mostly appeared distinct, yet I found that from every one of them at short distances some fibres did obliquely, according to the course of those I have mentioned, insert themselves into the next convolution, and become a part of it; though some I observed to have a contrary tendency, or rather seemed to ascend from the lower to the upper convolution, and help to constitute it, and so to observe the course mentioned; nay, sometimes would go farther than the next convolution, and, running under it, apply themselves obliquely to some higher, which yet being in a smaller number than the rest that lay in the order contrary to them, did not very much hinder the dissociation of the main ones: which fibres breaking off, and that in some places in greater numbers than in others, would at last cause the whole cluster to break off.—Fourthly, I observed, that as most of these fibres would by degrees, according to the order of the convolutions, insert themselves into the next, so some of them would pass over it, and more would run under it, and either adjoin themselves to some more remote, or elude my searching by hiding themselves under them. This insertion of these fibres seems to be the reason of the annular phasis before mentioned, in the contrary way of separation. Fifthly, If I began at a lower part of the intestine, and tried to unravel upwards, there was not much more difficulty than when, beginning above, I attempted it downwards.—Sixthly, when before boiling I caused the inside of the intestine to be turned outward, as I did in two trials, and afterward by taking off the glandular and vascular coats endeavoured to unravel the fibres, I found they would come off in the contrary order, viz. from my left hand toward my right; which, I conceive, confirms the observation above delivered, in regard the intestine being inverted, the order of separation must be so too.—Seventhly, in one of these attempts of unravelling the fibres of the intestine of an ox, so inverted, I found that, though the fibres taken up came off in the order just now mentioned, yet running over some others, they made a more oblique excursion, and for two or three convolutions left between them a considerable area of fibres, amounting to five or six times or more the breadth of those that so came off, till going deeper and deeper among the other fibres, and at last running under them, they could be no longer traced, but brake off.—Eighthly, I found it much more difficult to unravel the fibres of the cœcum, than the other intestine, which seemed more interwoven than those of the rest, and to have contrary tendencies one among another.

This is the sum of my observations hitherto concerning this coat, which I take leave to think one concave and helical muscle, if I may so stile it: and that it might be supposed such, the forementioned insertions seemed to evidence, appearing in the separating appositely enough to represent the fabric of a muscle delivered by the accurate Steno. Where the tendons of it are fixed, is not evident; but, if I may have the liberty to conjecture, I should think the upper of them to be radicated at the pylorus, (since the carneous coat of the stomach being by the learned Dr. Willis found to be a muscular contexture, and there being a continuation of motion between that part and the intestines, it seems to me not altogether improbable they may be but one muscle;) and the other at the anus.*

* Notwithstanding Dr. Cole's observations, the fact remains as before; viz. the "supposed annular" fibres of the intestines are annular, (or rather falciform) and not spiral.

Further Observations of the Lunar Eclipse, of Jan. 1, 1676, N. S. By M. Bulliald, Richelt, Hecker, &c.* N° 125, p. 610.

Phases.	Observed at Paris, by Bulliald.			Paris, by Hecker.			Strasburg, by Richelt.			London.		
	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.
Beginning of eclipse....	2	22	30	2	8	0	2	48	48	2	16	0
Greatest obscuration....	3	18	14	3	20	18	3	45	16	3	11	37
End of eclipse	4	13	56	4	32	36	4	41	44	4	7	15
Total duration	1	51	26	2	24	36	1	52	56	1	51	15
Digits eclipsed	3°	21′	—	4°	24′	31″						

It appears, from these observations, compared with calculations from the then existing tables, that the Rudolphine tables differ considerably from the truth, both for duration and magnitude; but the Philolaic tables less.

An Account of some Books. N° 125, p. 611.

I. Nicolai Mercatoris Holsati, è Soc. Regia, Institutionum Astronomicarum Libri duo, de Motu Astrorum Communi et Proprio, secundùm Hypotheses Veterum et Recentiorum præcipuas; deque Hypotheseωn ex Observatis Constructione: cum Tabulis Tychonianis Solaribus, Lunaribus, Lunæ-Solaribus, et Rudolphinis, Solis, Fixarum, et quinque Errantium, earumque Usu, Præceptis et Exemplis commonstrato: subnexâ Appendice eorum, quæ novissimis temporibus cælitus innotuerunt. Lond. 1676, in 8vo.

This learned and industrious mathematician comprehends in these institutions the sum and substance of astronomy. And though many authors before him have treated this science well, particularly Mæstlin, Kepler, Riccioli, and Gassendi, yet he pursues several things differently from others, and insists on such particulars as he thought most pertinent to his purpose. For besides the chief use of both the globes in some considerable problems, and the trigonometrical calculation employed in the doctrine of the sphere; he has fully explained the equation of time, in both the Ptolomæan and Copernican system, as also the lunar hypothesis of Tycho, and the eliptical of the planets: nor has he been less solicitous in teaching how to raise hypotheses from observations, and in delivering the calculus of the celestial motions from the most approved tables:

* See several other observations of this eclipse, in some of the preceding Numbers.

explaining also with exactness the Keplerian hypothesis of the planets, and subjoining the astronomical hypotheses of Ward, Bulliald, and his own, which last he esteems new, and according to which he teaches how to make a calculus à priori, comparing the same with good observations. The whole he concludes with the exposition of the late discoveries made in the heavens.

II. Observations sur les Eaux Minerales de plusieurs Provinces de France, faites en l'Academie Royale des Sciences, en l'annee 1670, et 1671, par le Sieur du Clos, Conseiller et Medecin ordinaire du Roy, de la dite Academie. A Paris, 1675, in 12mo.

This account of mineral waters, like all other treatises thereon published at this period of time, is filled either with vague conjectures or erroneous assertions. Among the modern descriptions and analyses of the mineral waters of France, those by Cadet, Bayen, Venel, Deyeux, Carrere, Fourcroy, &c. deserve to be particularly consulted.

III. Cochlearia Curiosa, or the Curiosities of Scurvy-grass, written in Latin by Dr. Andr. Molimbrochius of Leipsig, and translated by Dr. Th. Sherley, Physician in ordinary to his Majesty. Lond. in 8vo. 1676.

Scurvy-grass is not now in that high estimation in which it formerly stood.

IV. Two Treatises, the one Medical, of the Gout; by Herman Busschof, Senior, of Utrecht, residing at Batavia in the East Indies; the other partly Chirurgical, partly Medical, containing some Observations and Practices relating to some Extraordinary Cases of Women in Travail, and to some other uncommon Cases of Diseases in both Sexes; by Hen. van Roonhuyse, Physician in ordinary at Amsterdam. Translated from the Dutch. London, in 8vo. 1676.

In the 1st of these treatises, the author after giving it as his opinion, that the seat of the gout is within the periosteum, proceeds to describe from experience, both made upon himself and others, the cure of the gout, by burning with a soft and woolly substance, called moxa, made by a skilful preparation of a certain dried herb, highly valued by the Chinese and Japonese; of which he sent over a quantity to his brother at Utrecht, from whence Mr. Pitt, in St. Paul's Church-yard, has procured a parcel for the use of those that are desirous to employ it, not only for this purpose of curing the gout, but also for that of removing epilepsy, madness, and catalepsy.

The other treatise contains several happy cures of strange ruptures and other remarkable accidents of the womb; the manner of performing the Cæsarean section, of curing the falling down of the womb, of curing wombs closed, and several closures of the vagina uteri, of a happy cure of a child's fundament closed, and of the rupture of a bladder; of the firm union of the dura mater to the skull; of the modern use and abuse of trepanning, which is here showed not to be so often necessary nor useful as is commonly pretended; of grievous wounds in the head well cured without the trepan; of the manner of cutting hare-lips, and several successful operations thereof; of the happy cure of a wounded nerve, &c.

V. New and Curious Observations of the Art of Curing the Venereal Disease, &c. Written in French by M. de Blegny, Surgeon to the French Queen; translated by Walter Harrys, M. D. late Fellow of New College in Oxford. Lond. 1676, in 8vo.

An Account of Virginia, its Situation, Temperature, Productions, Inhabitants, and their manner of planting and ordering Tobacco, &c. Communicated by Mr. Thomas Glover, an ingenious Surgeon that has lived some Years in that Country. N° 126, p. 623.

There is little in this paper worth preserving, except the account of a certain marine animal, which is as follows: As I was coming down the Rapahannock river in a sloop bound for the bay, it happened to prove calm; at which time we were three leagues short of the river's mouth; the tide of ebb being then done, the sloop man dropped his graplin, and he and his boy took a little boat belonging to the sloop, in which they went ashore for water, leaving me aboard alone, in which time I took a small book out of my pocket and sat down at the stern of the vessel to read. But I had not read long before I heard a great rushing and flashing of the water, which caused me suddenly to look up, and about half a stone's cast from me appeared a most prodigious creature,* much resembling a man, only somewhat larger, standing right up in the water, with his head, neck, shoulders, breast, and waste, to the cubits of his arms, above water; his skin was tawny, much like that of an Indian; the figure of his head was pyramidal, and smooth, without hair; his eyes large and black, as were also his eye-brows; his mouth very wide, with a broad black streak on the upper lip, which turned upwards at each end like mustachios; his countenance was grim and terrible; his neck, shoulders, arms, breast, and waste, were like those of a man; his hands, if he had any, were under water; he seemed to stand with his eyes fixed on me for some time, and afterwards dived down; but a little after rose at somewhat a farther distance, and turned his head towards me again, and then immediately fell a little under water, and swam away so near the top of the water, that I could discern him throw out his arms and gather

* In all probability some species either of phoca or trichechus.

them in, as a man does when he swims. At last he shot with his head downwards, by which means he cast his tail above the water, which exactly resembled the tail of a fish, with a broad fan at the end of it.

As to the timber of this country there are divers kinds, four several sorts of oak, very tall and smooth. There is also another sort of timber called hickery, harder than any oak. There are also very large and tall poplars, and in some parts of the country great store of pines, fit for masts of ships; there are likewise black walnut, cypress, cedar, dogwood, ash, elm, gum-tree, locust, chesnut, hasel, sassafras, holly, elder, with several others.

As to the fruit trees of the country it affords great plenty: for there are few planters but that have fair and large orchards, some whereof have twelve hundred trees and upward, bearing all sorts of English apples, as pear-mains, pippins, russetens, costards, marigolds, kings-apples, magitens, batchelors, and many others, of which they make great store of cider. Here are likewise great peach orchards, yielding such an infinite quantity of peaches, that at some plantations they beat down to the hogs 40 bushels in a year. Here are also great store of quinces, which are larger and fairer than those of England, and not so harsh in taste; of the juice of these they also make quince drink. Here are likewise apricots, and some sorts of English plums, but these do not ripen so kindly as they do in England. There are some sorts of pears, but at very few plantations; I have seen the bergamy, warden, and two or three other sorts, and these are as fair, large, and pleasant, as they are in England. Here grow as good figs as in Spain, but there are few planted as yet. Those that take the pains to plant gooseberries have them; but I never saw any of our English currants (riberries) there, and it is observed that oranges and lemons will not grow there; though they do in more northern countries. There are also plenty of mulberry-trees about their houses; these were planted at first to feed silk-worms, but that design failing, they are now of little use amongst them. The meanest planter has store of cherries, and they are all over Virginia as plentiful as they are in Kent. The cherry-trees grow more large generally than they do in England, and bear more plentifully, without any pains taking of digging about them, or pruning them. There grows wild in some places of the woods a plum somewhat like our wheat-plum: but it exceeds it, being much more succulent. In the woods there are abundance of vines, which twine about the oaks and poplars, running up to the top of them; these bear a kind of claret-grapes, of which some few of the planters make wine. It is somewhat smaller than French claret; probably through want of cultivation. There is also in the woods a little shrub, bearing a berry like our elderberry, and is very pleasant to eat.

Their gardens have all sorts of English pot-herbs, greens, and sallads. Also roses and other flowers. There grow wild in the woods plantane of all sorts, yellow-dock, bur-dock, Solomon's-seal, agrimony, centory, scabious, groundsel, dwarf-elder, yarrow, purslane, and white maiden-hair, the best that ever I saw. Upon the sides of the hills, asarum, and on the bayside, soldanella, or sea-scurvy-grass in great plenty.

Here grows the serpentaria nigra, the root of which was so much used in the last great pestilence, that the price of it advanced from ten shillings to three pounds sterling a pound; here is also a herb which some call dittany, others pepper-wort; it is not dittany of Candia, nor English dittander; it grows a foot or a foot and a half high; the leaves are about the breadth of a groat, and figured like a heart, and shot out of the stalk and branches one of a side directly opposite to each other; it smells hot like pepper, and bites on the tongue. The water of this herb distilled out of an alembick, is one of the best things I know to drive worms out of the body, and an ounce of this water taken causes perspiration plentifully. There are great numbers of other herbs, whose names, nature, virtues, and operations, are altogether unknown to us in Europe, neither have there been any physicians in those parts that have made it their business to understand much of them; but if the use of them were well known, it might prove a great and beneficial addition to the materia medica.

There are also divers kinds of small birds, of which the mocking-bird, the red-bird, and humming-bird, are the most remarkable; the first for variety and sweetness of notes, the second for his colour, and the last for the smallness of his body. As to the mocking-bird, besides his own natural notes, which are many and pleasant, he imitates all the birds in the woods, from whence he takes his name; he sings not only in the day but also at all hours in the night, on the tops of the chimneys; he has strange motions in his flying, sometimes fluttering in the air with his head right down and tail up, other times with his tail down and head up. Being kept tame he is very docile. The red-bird takes his name from his colour, being all over of a pure blood-red. The humming-bird takes his name from the noise he makes in flying; this is of divers colours, and not much larger than a hornet, and yet has all the parts of a bird entire.

There are five or six sorts of snakes, amongst which the rattle-snake is most remarkable, being about the size of a man's leg, and for the most part a yard and a half long; he has a rattle at the end of his tail making a noise when any one approaches him; which seems to be a peculiar providence of God to warn people to avoid the danger.

The Indians are generally well proportioned as to their stature, being somewhat tall, but no ways corpulent; their hair black, usually hanging right down;

their eyes also black; their skin tawny, inclining to blackishness; they live together in towns, and every town is under a several king. At the first coming of the English, divers towns had two or three thousand bowmen in them; but now, in the southern parts of Virginia, the largest Indian town has not above 500 inhabitants; many towns have scarcely 60 bowmen in them, and in one town there are not above 20; and they are so universally thinned in the forementioued southern part, that I verily believe there are not above 3000 left under the whole government of Sir Will. Bartlet; but in my Lord Baltimore's territories, at the head of the bay, where the English were later seated, they are more numerous, there being still in some towns about 3000 Indians. But these being in continual wars with each other, are likely soon to be reduced to as small numbers as the former. Instead of clothes they wear a deer-skin tacked about their middle, and another about their shoulders; and for shoes they have pieces of deer-skin tied about their feet. Their habitations are cabins about nine or ten feet high, which are made after this manner: they fix poles into the ground, and bring the tops of them one within another, and so tie them together: the outside of these poles they line with bark to defend them from the injuries of the weather, but they leave a hole on the top, right in the middle of the cabin, for the smoke to go out; round the inside of their cabins they have banks of earth cast up, which serve instead of stools and beds; they have no kind of houshold stuff, but earthen pots, wooden bowls, and thin mats to lie on; all which they make themselves. Their diet is Indian corn, venison, wild turkeys, oysters, and all kind of fish the rivers afford, and all kind of wild beasts of the woods. They are prohibited the keeping either cows, sheep, or hogs, by the English, lest they should make bold with more than their own. They formerly took their fish after an odd manner, before the English came amongst them, which was thus: at the head of their canoes they fixed a hearth, on which in a dark night they would make a blaze with fire put to the shivers of pine-tree, then they would paddle their canoes along the shore in shoal-water; the fish seeing the light would come as thick as they could swim by each other about the head of the canoes; then with sticks that were pointed very sharp at the ends, they would strike through them and lift them up into the canoe. But now they have learned of the English to catch fish with hook and line, and sometimes the English use their way in dark nights, only they strike with an instrument of iron somewhat like mole-tines.

As to their worship I know little of it; only they have priests which are generally thought to be conjurers; for, when they have great want of rain, one of their priests will go into a private cabin, and by his invocations will cause abundance to fall immediately, which they call making of rain. They offer the first fruits of all things; the first deer they kill after they are in season, they lay privately on the head of a tree near the place where they killed it, and they say, no good luck will befal them that year if they do not offer the first of every thing. They burn the bodies of the dead, and sow up the ashes in mats, which they place near the cabins of their relations.

Advertisement concerning the Quantity of a Degree of a great Circle, in English Measures. N° 126, p. 636.

Some time since an account was given* concerning the quantity of a degree of a great circle, according to the tenor of a printed French discourse, entitled De la Mesure de la Terre. The editor not then knowing what had been done of that nature here in England, but having been since directed to the perusal of a book, composed and published by that known mathematician Richard Norwood in the year 1636, entitled The Seaman's Practice, wherein, among other particulars, the compass of the terraqueous globe, and the quantity of a degree in English measures are delivered, approaching very near to that which has been lately observed in France; he thought it would much conduce to mutual confirmation, in a summary narrative to take public notice here of the method used by the said English mathematician, and of the result of the same; which, in short, is as follows:

An. 1635, Mr. Norwood, reader of the mathematics in London, observed as exactly as he could, the summer solstitial meridian altitude of the sun in the middle of the city of York, by an arch of a sextant of more than five feet radius, and found it to be 59° 33′; and formerly, viz. An. 1633, he had observed the like altitude in the city of London, near the Tower, to be 62° 1′; the difference between which numbers gives 2° 28′ for the difference of latitude between those two cities. Whereupon he actually measured for the most part the way from York to London with chains, and where he measured not, he paced it, saying, that through custom he usually came very near the truth; observing all the way he came, with a circumferentor, all the principal angles of position or windings of the way, with a competent allowance for other lesser windings, ascents, and descents; not laying these down by a protractor after the usual manner, but framing a table, much exacter and fitter for this purpose. And by this method and measure he found the parallel of York from that of London to be 9149 chains, every chain being 6 poles or 99 feet, 16½ English feet to a pole. Now these 9149 chains being equal to 2° 28′, the aforesaid difference of

* See No. 112 and No. 124 of these Tracts.

latitude between those two cities, a little calculation makes it appear, that one degree of a great circle, measured on the earth, is 367196 of our feet, in round numbers 367200, or 22254 poles, which make 556 furlongs and 14 poles, or 69½ English miles and 14 poles, 8 furlongs to a mile, and 40 poles to a furlong. Which being compared to that measure of a degree, which is delivered in the above-mentioned French discourse, will be found to come very near it, they finding 73 miles nearly, at 5000 feet to an English mile, which make 365000 feet; whereas the 69½ English miles and 14 poles, found by Mr. Norwood, amount to 367200 feet, reckoning 5280 feet to an English mile, as the true measure of it is; whence the difference between these two measures appears to be no more than 2200 feet, which is not half an English mile by 440 feet.

From hence the whole circumference, as also the diameter and semidiameter of the said terraqueous globe, according to this measure may be easily found, viz.

The circumference............	25056 miles.
The diameter.................	7966.*

Observations made of the late Solar Eclipse on the 1st of June, 1676, *O. S.* N° 126, p. 637.

1st. *By Francis Smethwick, Esquire,* as follows,

	h.	m.
The beginning of the eclipse at Westminster....	7	50
The end was at	9	54¾
Therefore the whole duration was.............	2	4¾

The times taken by a pendulum clock, vibrating seconds, and corrected by observations.

The telescope a good one, of 7½ feet.

* This measurement of the earth at so early a date, by our countryman Norwood, is very ingeniously devised, and simply executed, reflecting on him considerable credit as an individual, whose means and convenience for the performance were small and humble. The deviation from an accurate result is also not so considerable as might be expected from the rude manner of his measuring with a chain, in all directions along the high road, to the right and left, as well as up and down hill, and sometimes only by pacing or stepping the distances. It seems however he did not make a sufficient allowance for those zigzag directions and estimations, as his conclusion gives the mean length of a degree of latitude too much by almost half a mile, viz. $69\frac{8}{15}$ miles to a degree, instead of $69\frac{1}{15}$, as deduced from many later and more accurate measurements. Of these a full account is given in my [Dr. *Hutton's*] Philosophical Dictionary, under the articles *degree* and *earth,* where the particulars relating to them may be seen.

2d. *By Mr. Colson, at Wapping, near London.*

	h.	m.	s.
$\frac{1}{4}$ Digit was observed eclipsed at..........	7	51	51
$3\frac{1}{10}$....................................	8	23	21
$\frac{3}{4}$....................................	9	49	0
The eclipse was not ended at..............	9	55	36
It was observed as ended at	9	57	6

The times corrected by observed altitudes.

An Account of some Books. N° 126, p. 638.

I. Elemens des Mathematiques, ou Principes Generaux de toutes les Sciences qui ont les Grandeurs pour Object: par J. P. A Paris, 1675, in quarto.

The author of this work, Jean Prestet,* delivers a short and easy method to compare quantities, and to discover their proportions and relations to each other, by characters of numbers and letters of the alphabet; affirming to have here demonstrated things in a geometrical order, and rendered the algebraical analysis much easier, and treated the same more fundamentally than has been done hitherto.

By quantity he understands here not only the extension in length, breadth, and depth, but whatsoever we conceive to be capable of more or less, and that can be exactly measured, whether it be exactly known, or supposed such. He considers, that though arithmetic be a science on which all others depend, yet it is this algebra which serves to elucidate, extend, and perfect, as much as is possible, arithmetic, and generally all the sciences that relate to the mathematics: it being so general that it considers all quantities, and what it demonstrates being capable to be applied, not only to numbers, lines, and figures, weights and velocities, but also to all such numbers, lines, velocities, and particular quantities, as you can conceive in each species of quantities.

But it is not only the extent and universality of algebra for which he commends it, but also the facility it affords to the mind of discovering the most hidden truths.

* John Prestet, a priest of the oratory, was born at Chalons-sur-Saone, in 1658. He went to Paris early in life, where having finished his studies, he was entertained by father Malbranche, who, finding he had a genius for the sciences, taught him mathematics, in which his young pupil made so rapid a progress, that at 17 years of age he published the first edition of his Elemens de Mathematiques. In the same year, 1675, he entered the congregation of the oratory, and taught mathematics with distinguished reputation, particularly at Angers and at Nantes. He died June 8, 1690, at Malines. His Elemens, above noticed, contain many curious problems. The best edition is that of 1689, in 2 volumes 4to.

As to the order which our author has observed in those elements, they are divided into two parts. The first, containing five books, explains and demonstrates both the supputation with numbers, called arithmetic, and that of symbols or letters, called algebra. The other, in four books, explains and treats fundamentally of the analytical part, teaching to resolve questions, and to discover the general truths of the mathematics.

In the last four books he settles first the grounds of analysis. Next, after giving some idea of the method of Diophantus, and of that of Vieta, he is particular in explaining the method of Descartes, which he esteems to be the most general, fertile, and easiest of all. Yet seeing that this famous man has not demonstrated, nor so much as explained, all the principles which he has made use of, our author intimates, that the reader will not find in his writings the same advantages for understanding his analysis, as may be had from these elements. For, after he has clearly explained and demonstrated all those principles, he thence deduces in order not only all the discoveries made by Descartes, but also other new and more useful ones.

II. De l'Art de Parler; à Paris, 1675, in 12mo.

To make the reader comprehend the true reasons of the principles of rhetoric, the author begins with explaining how discourse comes to be formed; and there being nothing better than nature herself to teach us the form that our words ought to have for expressing our thoughts and the motions of our will, he represents to himself a troop of men newly born, and that never have spoken before. He considers what these men would do: he shows, that being soon tired with expressing their mind to each other by gestures, they would quickly find the advantage of speaking, and form a language to themselves; he inquires, what form they would give to that language; and in this inquiry he lays the foundations of all languages, and shows the reason of all the rules prescribed by grammarians, showing that this research is very useful to learn languages with more ease, and to speak them with more exactness. And having made these new men act their part, he declares what has been the true origin of tongues, and that it is not hazard that has made men find out the use of speech; yet showing that speech is subject to men's will, and that custom, or the common consent of men, exercises an absolute power over words; whereupon he gives rules to know which are the laws of custom, and to observe them after he has instructed the reader which are the laws prescribed by reason.

III. The Manner of Raising, Ordering, and Improving Forest Trees; also how to plant, make, and keep Woods, Walks, Avenues, Lawns, Hedges, &c. with several Figures proper for Avenues and Walks to end in; and convenient Figures for Lawns; also Rules and Tables, showing how the ingenious Planter may measure superficial Figures; with Rules, how to divide Woods and Land; and how to measure Timber and other Solid Bodies, either by Arithmetic or Geometry, &c. By M. Cooke, in 4to.

IV and V. The French Gardener reprinted; to which is annexed the English Vineyard vindicated, and the Way of making and ordering Wines in France, &c.

The French Gardener, which gives proper instructions for the culture and propagation of the best esculent plants, which are yet much wanting in England, is reprinted in a third edition, illustrated with sculptures: To which is annexed, The English Vineyard vindicated; and the Way of making and ordering Wines in France, in 8vo.

On the Effects of Thunder and Lightning on Sea Compasses; also on the Gradual Alteration of the Temperature of the Air in different Countries; of an uncommon Hygroscope; and on the Scent of the Musk-quash, &c.; in a Letter from Dublin. N° 127, p. 647.

Mr. Haward, a very credible person, tells me, that being once master of a ship in a voyage to Barbadoes, in company with another commanded by one Grofton, of New-England, in the latitude of Bermudas, they were suddenly alarmed with a terrible clap of thunder, which broke Mr. Grofton's foremast, tore his sails, and damaged his rigging. But that after the noise and confusion were past, Mr. Haward, to whom the thunder had been more favourable, was however no less surprised to see his companion's ship steer directly homeward again. At first he thought that they had mistook their course, and that they would soon perceive their error; but seeing them persist in it, and being by this time almost out of call, he tacked and stood after them; and as soon as he got near enough to be well understood, asked where they were going: but by their answer, which imported that they had no other design than the prosecution of their former intended voyage, and by the sequel of their discourse, it at last appeared that Mr. Grofton did indeed steer by the right point of his compass, but that the card was turned round, the north and south points having changed positions; and though with his finger he brought the fleur-de-lys to point directly north, it would immediately, as soon as at liberty, return to this new unusual posture; and on examination he found every compass in the ship altered in the same manner: which strange and sudden accident he could impute to nothing else but the operation of the lightning or thunder just mentioned. He adds, that those compasses never to his knowledge recovered their right positions again.

That in America, at least as far as the English plantations are extended, there is an extraordinary alteration, as to temperature of the country, since the Europeans began to settle there. Which change is generally attributed to the cutting down of vast woods, with the clearing and cultivating of the country. But that Ireland should also considerably alter without any such manifest cause either invalidates that reason, or else evinces that quite different causes may produce the same effect. For if it be true, as some compute, that this kingdom was better inhabited and cultivated before the late civil wars, than at present, viz. 1676, it should, according to the reasons alleged for the change of temperature in America, be rather grown more intemperate, viz. for want of cultivation: but the contrary is observable here, and almost every one begins to take notice, that this country becomes every year more and more temperate. Formerly it was not unusual to have frost and deep snows of a fortnight or three weeks continuance; and that twice or thrice, sometimes oftner, in a winter; nay we have had great rivers and lakes frozen all over; whereas of late, especially these two or three years last past, we have had scarcely any frost or snow at all. Neither can I impute this extraordinary alteration to any fortuitous concourse of ordinary circumstances requisite to the production of fair weather; because it is manifest, that it has proceeded gradually, every year becoming more temperate than the preceding. Though it be observed that frosty and snowy winters make early springs, and for as little as we have had of either this winter, yet there has not within the memory of any now living happened a forwarder spring in Ireland; since this place could produce some store of ripe cherries in the midst of April. The wind keeps for the most part here between the north-west and the south, seldom at east, and yet less frequent at north or north-east, insomuch that many here do not scruple to affirm, that for at least $\frac{3}{4}$ of the year the wind is westerly; and we have sometimes known passengers wait at Chester and Holyhead no less than three months for a fair wind to come hither.

The hygroscope I make use of, I thus contrived. I took two pieces of deal board, though poplar would do better, each about two feet long, and a foot or more in breadth, as AB, fig. 6, pl. 9. These being well planed and shotten, that their edges might meet even together, in that position they were fastened at each end between two ledges of oak, CC, of two inches broad; fixing both boards in, as pannels are set in wainscoat. This done, supposing $\frac{1}{4}$ of an inch to be the utmost distance that these two boards would shrink asunder in the driest weather, I took a thin piece of brass, D, two or three inches long, and $\frac{1}{4}$ inch broad, and on one edge towards the end I measured $\frac{1}{4}$ of an inch, dd, this I divided into five equal parts, and with a small file made them into so many fine teeth, like those of a watch-wheel. This piece of brass I placed flat, across the juncture of the two boards, nailing one end, by means of two small holes bb, to the board A only, leaving the other end, which is the toothed one, free, and reaching to a competent distance over the board B, to which it had no coherence. Next I made a pinion e, consisting of as many teeth as the piece of brass had, on the end of a piece of thick iron wire: the axis F, with its pinion e, I so fastened to the other board B, by means of the small arm E, and so adapted to the teeth of the brass plate, that when the boards shrink asunder, the brass being drawn a little away, must needs turn this axis, by means of its toothed pinion e, more or less; and thus, if ever it happens that the boards gape but a quarter of an inch asunder, this axis will have made one entire revolution: wherefore putting a long index GG on the extremity of this axis, and making a circle round it with the usual graduations, numbered from any point at pleasure, the motion of the index backward or forward, shows the degrees of the drought or moisture of the air. Now this axis may be made to pass through a round plate of wood or metal, that hides the contrivance, all but the hand and figures, as in a clock or watch. It is to be noted, that the boards must be fastened to the ledges, only at the outer edges, as at aaaa, that they may have the more liberty of swelling and shrinking asunder. The commodiousness of this kind of hygroscope in comparison of those made of wild oat-beards, may best be observed by those that may try them both.

I have here also sent you the figure of an admirable instance of nature's luxuriancy, in her contrivance even of insects. Fig. 7, pl. 9, is a kind of large flying beetle, of a dark shining brown, with a huge pair of horns, in proportion to the body, shaped and branched exactly like a stag's or hart's, from which last it is denominated, in Virginia and New-England, the flying hart. It is of the shape and size of the figures above referred to. It flies high and swift, and rests most commonly on branches or trunks of standing trees; where, as soon as it has taken up its station, it begins with a shrill chirping voice, which it raises by little and little till it makes the whole woods ring again; and then lessens gradually, till it ceases with a kind of silent murmur, as if the little creature had rung itself asleep: then it flies to some other place, and begins the same tune again. The horns are of a shining hard substance, and the tips of them touch the same plane with the belly.

Though the author in N° 27, of your Transactions, seems inclinable to believe, that it is peculiar to the Thames water alone, upon stinking, to be recoverable or potable again; I can affirm upon my own knowledge, that water taken aboard at New London in New-England, though in eight days time it stunk intolerably, yet when we came to Virginia, it recovered so perfectly, that I made no scruple to drink of it in harbour, even when we had fresh water newly brought from shore, nor could I easily perceive it had any relics of its late corruption.

That the testicles of the animal called musk-quash do smell strong of musk, as Mr. Josselin says, in N° 85, is most certain: for, I have known some of them kept a long time in a pocket, till they were become hard and black, and yet smelt as strongly as at first; which, in my opinion, was nothing inferior to the scent of that which is commonly sold for musk in the shops.

On the Texture of Trees; and on Animals in Wine. In a Letter from Mr. Leewenhoeck, Delft, April 21, 1676. N° 127, p. 653.

M. Huygens was pleased to show me the comparative anatomy of the trunks of plants, written by Dr. Grew; and told me that he had very ingeniously and learnedly discoursed on that subject; though I, by reason of my unskilfulness in the English tongue, could have little more than the pleasure of viewing the elegant cuts.

I have formerly written to you, that I had discovered in several trees two sorts of vessels[1] or pores, and conceived, that the matter which serves for the increase of trees was in the greater vessels sent upwards,[2] and that some small particles again descended in the smaller vessels, to the roots, whereby was maintained a circulation[3] also in trees.

But not finding by the figures of Dr. Grew, that he has discovered those two sorts of vessels in the woody part,[4] I here take the liberty of sending you the eighth part of the transverse slice of an ash-sprig of a year's growth; and now acquaint you, that besides those two sorts of vessels in wood, I have discovered a third sort; these two going directly upward, and this third issuing out of the

[1] These two sorts of vessels are described by Dr. Grew in his first and general Anatomy of Plants, in his Anatomy of Roots, and in his Anatomy of Trunks.—[2] The chief use to which Dr. Grew, in his said three books, assigns these vessels in all parts, is not the conveyance of sap but of air. And herein Signor Malpighi agrees with him; see his Anatome Plantarum de part. Caulem componentibus. Yet in some few plants, and at some certain times of the year only, Dr. Grew shows, that the said air-vessels do contain an aqueous sap, and how it comes to pass; see his Anatomy of Trunks, p. 2, ch. 1, and pag. 26.—[3] Dr. Grew, in his aforesaid first book, speaks conjecturally of a circulation, not in the trunk but in the root only; and that not by vessels of a different but the same species, viz. sap-vessels, some of them running through the pith, by which chiefly the sap may ascend, and some through the bark, by which part of the sap may descend; see ch. 2, of that book.—[4] These two sorts of vessels are, as was said, distinctly and largely described by Dr. Grew, particularly in his Anatomy of Trunks, p. 22 to 30. And the explications of all the figures plainly distinguish the air-vessels from the sap-vessels. The pores or mouths of which sap-vessels are from their incomparable smallness represented only in fig. 18, where they are very much wider than ordinary; see also p. 25, of that book.

middle or the pith, going horizontally[5] to the circumference: so that the whole body of wood consists of nothing but small hollow pipes.[6]

These pipes, out of which the firm wood is made up, are in many places as clear as crystal,[7] and in other places they seem to consist in part, of small globules.[8] The great vessels observed and expressed by Dr. Grew, I saw very manifestly to consist of small globules. These great vessels are generally furnished with small membranes, which being cut through, may be seen to lie obliquely in the vessels; and these I conceive to be valves.[9]

[10] These three sorts of vessels then I have observed not only in ash wood, but also in elm, oak, willow, shumach, lime tree, apple, pear, plum, walnut, hasel tree, &c. And all the vessels which Dr. Grew has represented in ash and other wood, though they differ from each other in size, yet under favour, I take them to be of one sort.[11] I have also made it out, as well as I could, how

—[5] These parts, which Mr. Leewenhoeck calls a third sort of vessels, Dr. Grew calls the insertions, and has largely described them in all his three books, particularly in his Anatomy of Trunks, p. 20, 21, 22; and has clearly expressed them in almost every figure of that book, sc. by white diametrical lines, more agreeable, as he conceives, to nature, which Mr. Leewenhoeck (fig. 10, GH) has expressed by black. These parts he demonstrates, especially from herby plants, to be of the very same substance with the pith. Wherein Signor Malpighi also most clearly agrees with him; see his Idea Anat. Plant. p. 3, l. 3. Of these insertions it is by Dr. Grew further remarked, that they consist of a number of most exquisitely small fibres; which in all less woody, softer, and younger plants, are woven up together into extreme small bladders, which bladders Signor Malpighi has likewise observed, calling them utriculous; see the forecited place; but not their being composed of such fibres. These bladders being in cleaving a branch many of them cut open, Dr. Grew tells me, he conceives may be taken by Mr. Leewenhoeck for the mouths of vessels. But in most hard woods the bladders, he says, are scarcely to be seen, the said fibres being so closely couched and drawn up together, as to lie rather after the manner of the vessels in the liver, testicles, glands, and other viscera in animals. —[6] Dr, Grew has formerly gathered on probable grounds, that not only the wood, but the whole of a plant consists of pipes; see his Anatomy of Roots, part 2, ch. ult. and Anatomy of trunks, p. 18, and p. 34, 35; see also the latter paragraph of note 5.—[7] The same Dr. Grew has said in his Anatomy of Roots, p. — 114.—[8] Dr. Grew has given a further and more particular description of the structure of these vessels; Anatomy of Roots, p. 89, and Anatomy of Trunks, p. 30, and fig. 24. Which, if well minded, will give the reason why they seem, especially in vines, oak, and some other plants, to consist of globules.—[9] Of the same appearance of pithy valves, Dr. Grew makes mention in his first book of the Anatomy of Plants, p. 71, at the beginning. But that in the sap-vessels there are no valves, he proves by divers arguments; see his Anatomy of Trunks, p. 45, 46. He also says, that he has made some experiments, proving that there are no valves neither in the air-vessels.— [10] These three general parts Dr. Grew has described, and represented in several figures, showing the different texture of so many several sorts of wood; see Anatomy of Trunks, p. 20 to 30, compared with the figures and the explication of the same. But for what he says of one of the said three parts, which Mr. Leewenhoeck calls a third sort of vessels; see the note 5.—[11] Dr. Grew has both described, and by his figures (Anatomy of Trunks) represented, two sorts of vessels in the wood of ash, and divers other trees. But all these vessels, whose pores or mouths are represented, are indeed of one sort only, excepting in the 18th figure; which made Mr. Leewenhoeck (for want of skill in the trees and other plants grow in height and thickness; of which[12] I doubt not but Dr. Grew has written so learnedly that I shall not need to notice it here.

In fig. 8, pl. 9, AB is one of the great pores or vessels of an ash twig of one year's growth, cut lengthwise through the middle of the pores, which vessels consist of tranparent globules,[13] wherein might be plainly seen the small oblique membranes called valves,[14] which membranes do not lie with their upper part extended one and the same way, but so as that two sides of them, with their upper end, reach towards one another, as CC and DD. And if we suppose that the hollowness of these greater vessels is as large as a hair, we may then very well say, that the hollowness of the small ones[15] is at least 25 times smaller than such a hair. That these vessels consist of globules,[16] I have not only seen in ash-wood, but also in walnut, hasel, apple, pear, and plum, &c. trees.

In fig. 9, AB exhibits some of the small vessels that make up the firm wood,[17] cut off close to the bark lengthwise. Of these vessels[18] there lie 8, 10, or 12 together, as at C and D, in the manner of a weaver's shuttle, lying in some places irregularly, the one close by the other, and in other places somewhat more dispersed.

In fig. 10, ABCD is the bark[19] of the twig, which is only represented with bare lines, because that now the plant is growing, by which the bark is changed from what it is in winter. AHHDEGF is the eighth part of the wood of an ash twig one year old, cut transversely, not made up wholly of firm or close parts, but partly too of large vessels,[20] which yet differ much among themselves in size, and which are seldom or never perfectly round, standing also near the

English tongue to have recourse to the explications) to conceive there were no other represented at all. And for fig. 18, that being but one, which the author thought sufficient for example's sake, amongst so many more figures, Mr. Leewenhoeck it seems overlooked it; see the latter end of the note 4.—[12] The causes of which are assigned and explained in Dr. Grew's Anatomy of Trunks, part 2, ch. 5. And of a great many more particulars throughout the whole economy of vegetation in all the aforesaid three books—[13] See the note 8.—[14] See the note 9.—[15] Of the size of these valves; see Dr. Grew's computation, Anatomy of Trunks, p. 18, 19.—[16] See the note 8.—[17] Dr. Grew's description of this; see Anatomy of Trunks, p. 22 to 26.—[18] See the note 5.—[19] See Dr. Grew's description and representation of the bark in his Anatomy of Trunks. And of this very bark, fig. 15, with the explication. And it is further to be noted, that the same author in his Anatomy of Trunks, informs us that there are two sorts of vessels visibly distinct in the bark of most, if not of all, sorts of trees, and other plants, as well as in the wood. Wherein Signor Malpighi also agrees with him, at least that they are to be found in many trees of two distinct species; see his Idea, p. 2, towards the end. And Dr. Grew also both observes and shows three distinct species of vessels, even in the bark of some plants; see Anatomy of Trunks, p. 14 to 17, and fig. 19, 20, 21.—[20] Which Dr. Grew calls the air-vessels, (Malpighi Fistulas spirales) and describes, Anatomy of Roots and Trunks, p. 26

pith in some places irregular by each other; and the rest of the wood being an infinite number of little vessels[21] or pores. GH are vessels[22] having their origin from the pith, and terminating in the circumference of the woody part, viz. when the tree is not growing. These vessels may not always be seen, in a transverse cut, to have their rise out of G and to end in the circumference H,[23] because that in the dissection the knife does not throughout keep just the middle of the body that takes hold of these vessels, from the place of the very beginning of them, but in one place, as about C in fig. 9, it will cut through with its sharp point, and in another place the same will pass with its middle, as at D, where it is thickest; and thus the eye sees these vessels to have their beginning out of G, and run between G and H into nothing, and again, that the same seem to have their beginning in the middle, and become still broader and broader till they end in H.

II are the very small vessels,[24] that are accounted to be the firm wood. EKF is the pith of the twig, which likewise cannot be imitated by art, as it consists of vesicles or bladders[25] that have six, seven, or eight sides, and lie most curiously with their sides to each other. In some of which bladders I have seen small darkish globules;[26] and if I had not in some other wood more plainly discovered these globules, it would have been impossible to have observed them in this pith by reason of their extraordinary smallness.[27]

I beg you, sir, to communicate this to Dr. Grew, with my service to him, and to inquire of him, whether he has seen, as well as I, that the great vessels or pores, which are expressed in his figures, do not consist of globules, as in fig. 8, AB; as also that in the same do lie oblique membranes or films, which I call valves, as CC, DD; again, whether the particles of the wood, which encompass the great vessels, be not all of them very small vessels or pores; lastly, whether the strokes which in fig. 10 are denoted by GH coming out of the pith, and running horizontally to the circumference, do not also all of them

to 30.—[21] Which Dr. Grew calls the true wood, or old sap-vessels, described in his Anatomy of Trunks, p. 22 to 26.—[22] See the note 5.—[23] See the same thing observed in Dr. Grew's general Anatomy of Plants. And an example of the same in the wood of Sumach, Anatomy of Trunks, fig. 20; that being of a branch of the first year's growth, as is Mr. Leewenhoeck's, wherein it is much more observable than in older branches. The cause of which is that which Dr. Grew calls the braces, and Signor Malpighi, the superequitations of the vessels.—[24] The same with those mentioned note 21.—[25] See Dr. Grew's description of the pith, and therein of these bladders, Anatomy of Roots, part 2, and Anatomy of Trunks, part 2, ch. 4.—[26] See the same ch. p. 34.—[27] See the same ch. 32, 33. Note, that these bladders, whereof the pith consists, Signor Malpighi also observes, but not the fibres, of which fibres (most admirably woven up together) Dr. Grew has discovered the said bladders to be composed; see the same ch. p. 35.—Orig.

consist of vessels or pores; as these also which in fig. 9 are cut off along the wood, and run through the said vessels, as CD.

I have now some French wine of the growth of the year past, which has a very delicate taste. The vessel wherein this wine is, was very good and sweet when the wine was put in, and a coarse linen cloth dipped in melted brimstone and kindled had been hung over the vessel before it was filled. In this wine I have often observed small living creatures, shaped like little eels, as appear in AB, fig. 11, having on their forehead a round convexity like a crescent without having any thing else that I could see on the forepart of their body, and that part looked no otherwise than crystal; but towards its middle it was made up of nothing but globules, which I could very plainly discern; and the hinderpart of the body of these little animals appeared as clear and transparent as the forepart, and running to a very sharp tail.

Eclipse of the Sun, Anno 1675, *June* 23, *in the Morning, New Style. Observed at Dantzic. By Hevelius.* N° 127, p. 660.

	Calcul. by the Rudolphine Table.			Observed at Dantzic.			Differences.
	h.	m.	s.	h.	m.	s.	min.
Beginning at Dantzic ..	4	31	42	4	44	0	12
Greatest obscuration ..	5	28	20	5	39	0	11
End of the eclipse	6	24	58	6	33	30	9
The duration	1	53	16	1	50	0	3
The quantity	6°	5′		6°	42′		37

M. Hevelius also communicated his observation of the figure of Saturn, as it appeared to him in August 1675, as exhibited in fig. 1, pl. 10.

Mr. Flamsteed's Letter, concerning his Observations, and those of Mr. Townly, and Mr. Halton, of the late Eclipse of the Sun, June 1, 1676. N° 127, p. 662.

In conducting these observations, I was assisted by my friend Mr. Halley. We had prepared two tubes; the one 196½ inches long, having one of Townly's micrometers, with which I took the measures of the first eight phases. The other was only 103½ inches long, with my own micrometer, and with which Mr. Halley took the observations. But in the last two observations, with this tube, (the micrometer of which is fitter for this use than the other) I took the distance of the azimuths falling by the sun's lucid limb and the nearest cusp of the eclipse, while Mr. Halley in the mean time measured the lucid parts and the distance of the cusps, with the longer tube. A little before the beginning came Lord Viscount Brouncker, president of the Royal Society, who proved by his own judgment the measure of the sun's diameter, taken with the longer tube. At 7^h 45^m the sun first appeared through the clouds. The observations were as below. See the type at fig. 2, pl. 10.

Order of the Phases.	Time by the Pend. Clock.			Corrected Time.			Observed with the longer Telescope.	Observed with the Shorter.
	h.	m.	s.	h.	m.	s.		
	7	50	0	7	49	0	No eclipse yet. Clouds,	
1	7	54	50	7	53	50	The sun emerging from the clouds, the right side appeared eclipsed.	
2	7	58	24	7	57	24	IC.... 2040 = 10′ 10″	
3	8	4	12	8	3	12	IC.... 2773 = 13 56	
4	8	13	40	8	12	40	IC.... 3580 = 17 52	PL.. 3198 = 26′ 18″
5	8	18	37	8	17	37	PL.... 4975 = 24 50	IC .. 2334 = 19 13
6	8	21	6	8	20	6		PL.. 2989 = 24 35
							Sun's dia. 6360 = 31 43	3850 = 31 40
7	8	28	1	8	27	1		PL.. 2888 = 23 57
8	8	29	1	8	28	1	PL.... 4565 = 21 46	
9	8	35	12	8	34	12	PL.... 4478 = 22 18	AZ.. 2310 = 19 0
10	8	40	20	8	39	20	IC.... 4417 = 22 0	AZ.. 2070 = 17 2
							The sun then hid by clouds, till	
	10	2	0	10	1	0	The limb seen through the clouds, appeared free from the eclipse.	
	10	4	0	10	3	0	Shining clearer, nothing of the limb was wanting.	

The same Eclipse observed by Mr. Richard Townly, at Townly, in Lat. 53° 44′, *and Long.* 9 *min. West of the Meridian of London, page* 664. *See Fig.* 3, *Pl.* 10. N° 127, p. 663.

Time by the Pend. Clock.	Corrected by a merid. line.	Measures of the Phases.
h. m. s.	h. m. s.	
8 6 45	8 8 27	AB.... 1190 = 16′ 9″
		or . 1109 = 14 50
8 11 0	8 12 42	CD.... 1935 = 26 15
18 0	19 42	AB.... 1405 = 19 4
21 0	22 42	CD.... 1805 = 24 30
26 14	27 56	AB.... 1504 = 20 47
34 0	35 42	CD.... 1711 = 23 13
42 15	43 57	AB.... 1551 = 21 3 accurately.
46 30	48 12	CD.... 1720 = 23 20
		or.. 1702 = 23 15
51 45	53 27	AB.... 1553 = 21 4 accurately.
9 0 0	9 1 42	CD.... 1809 = 24 33
9 12 34	9 14 16	AB.... 1357 = 18 25
9 30 55	9 32 37	AB.... 872 = 11 50
9 41 15	9 42 57	Eclipse ended as near as could be perceived.

The place of the exit was so near the vertex, that I could not well determine which way it inclined from it. Though at 9h. 29m. by the clock, the cusps appeared parallel to the horizon.—At 9h. 10m. the sun's diameter measured 2334.—When the sun came to the meridian, by a long meridian line, the clock was found too slow by 1m. 42s. But by a large equinoctial dial, by which I could distinguish to half minutes or less, the clock was too slow all this morning only 45 seconds. Yet I rather trust to the former correction by the meridian line, than by the sun-dial.

The same Eclipse observed by Mr. Immanuel Halton, at Wingfield, about 10 *miles North of Derby, in Lat.* 53° 8′.

	h.	m.		h.	m.	
At	7	50	Nothing over the sun.	9	11	Digits $3\frac{1}{10}$.
	7	$50\frac{1}{2}$	The beginning exactly.	9	21	Digits $2\frac{15}{100}$.
	7	52	The defect observable.	9	$47\frac{1}{2}$	Not finished, but the end just approaching.
	9	0	The digits $3\frac{1}{2}$.			

The same observed at Paris, by M. Cassini. Page 664.

When the sun emerged out of the clouds, approaching to an altitude of 48°, I directed my quadrant to him, and kept it fixed at that altitude. From the time that the upper limb of the sun *a* touched the horizontal thread *cd*, fig. 4, pl. 10, in the focus of the telescope, to the coming of the centre *b*, there passed 104 seconds = *ab* or *br*. From the passage of the centre *b*, to that of the upper limb of the moon *o*, were 11 sec. = *bs*. From the transit of the centre *b*, to that of the upper western horn *e*, were $25\frac{1}{2}$ sec. = *eh*. From the transit of the centre *b*, to that of the lower eastern horn *i*, were 93 sec. = *ik*. Hence is determined the line of the horns *ie*, (independent of the variation), and its inclination to the horizon *lk*; also the point of concourse *p* of a tangent to the moon with the secant *iep*, and the tangent itself *po*, being a mean proportional between *pi* and *pe*. Also the angles *noe*, *toi*; and hence the angle *ioe*, with the triangle *ioe*, inscribed in the circumference of the moon.—From these and other astronomical principles I have deduced, that the beginning of the eclipse at Paris ought to be at 7h. 55m. and the end at about 10h. 12m.

The same Eclipse observed by M. Hevelius, at Dantzic.

	Times by the Rudolphine Tables.			Observed Times Corrected.			Measures.	
	h.	m.	s.	h.	m.	s.		
Beginning	9	22	26	9	22	0		
Greatest obscuration ..	10	17	57	10	31	0		
The end............	11	13	26	11	39	40		
Duration	1	50	58	2	17	40		
Sun's semidiameter ...							15′	0″
Moon's semidiameter..				10	0	0	13	53
Ditto				10	24	0	14	0
Ditto				11	0	0	14	50
Digits eclipsed.......	4°	21′	30″	4°	22′	0″		

An Extract of a Letter of the learned Dr. Matthias Mangold, of Basil, concerning a Mathematico Historical Table, designed in that University; together with a Description of the import of the same. N° 127, p. 667.

This idea of a mathematico historical table is by M. P. Megerlin, professor of mathematics at Basil, and which he proposes to regulate according to the revolutions of the great conjunctions of Saturn and Jupiter. But it contains nothing worth extracting here.

An Account of some Books. N° 127, p. 669.

I. Experiments, Notes, &c. about the Mechanical Origin of divers particular Qualities; among which is inserted a Discourse of the Imperfection of the Chemist's Doctrine of Qualities; together with some Reflections upon the Hypothesis of Alcali and Acidum. By the Hon. Robert Boyle, Fellow of the Royal Society. London, 1675, in 8vo.

These tracts are a fresh proof both of the noble author's constancy in his kindness to experimental philosophy, and of his sagacity in giving a more intelligible account of philosophical subjects, than is commonly received in schools. The matters here presented, by way of specimen, comprehend in a small compass a great variety; there being scarcely any one sort of qualities, of which there is not an instance given in this small volume; since experiments and considerations are there delivered about heat and cold, which are the chief of the first four qualities; about tastes and odours, which are of those that, being immediate objects of sense, are usually called sensible qualities; about volatility and fixity, corrosiveness and corrosibility, which, as they are found in bodies purely natural, are referrible to those qualities, that many physical writers call second qualities, and which yet as they may be produced and destroyed by the chemist's art, may be styled chemical qualities, and the spagyrical ways of introducing or expelling them may be referred to chemical operations, of which here is given a more ample specimen in the mechanical account of chemical precipitations. To all which are added some notes about magnetism and electricity, which are known to belong to the tribe called occult qualities, by dark philosophers.

Concerning these particular qualities the present design of the excellent author is chiefly, to give an intelligent and historical account of the possible mechanical origin of them; though his secondary end is to become a benefactor to the history of qualities, by providing materials for himself or others. And he endeavours to prove that all the qualities of bodies may be derived from and explained by mechanical principles.

II. Th. Bartholinus de Peregrinatione Medica, &c. Hafniæ, 1674, in fol.

A treatise showing the advantages of foreign travel to the physician and naturalist.

III. Georgii Hieronymi Velschii Hecatosteæ II. Observationum Physico Medicarum. Augustæ Vindelicorum. 1675.

IV. Joh. Nicolai Pechlinii,* M.D, &c. de Aëris et Alimenti Defectu, et Vita sub Aquis Meditatio. Kiloni, 1676, in 8vo.

This author having received out of Sweden a very extraordinary relation about a man drowned under ice, and revived after 16 hours time, takes thence occasion to discourse, in this tract in general, how far air and aliment are necessary to the life of vegetables and animals. He begins with vegetables, and examines the necessity of air and water to preserve them alive. Where he observes the obscure degree of life in bulbs and roots during winter; as also the cause of the distinction of life in annual and perennial plants; with the hasty growth of some vegetables. Proceeding to animals, he inquires first into the life of insects, and their apparent death in winter, which he esteems not to be without a remainder of the principle of life, as also into the changes of some of them into aurelias and butterflies. Here he takes notice, after Malpighi, of those exceedingly minute tubes in silk-worms, through which the air passes and carries on the motion of the liquor in their annular fibres. Next he explains, how the same alteration of life and death holds in birds, particularly in swallows and storks, that is found in insects; and takes notice of the swallows immerging themselves under the water on the sides of the Baltic sea, and remaining there all winter, and reviving again in the spring, flying about upon their being taken up in winter, and brought into a hot stove.

Thirdly, he attempts to show, why fishes cannot live long in the open air, partly because the current of the air is more impetuous than the nature of fishes will bear; partly, because the motion of the air carries off that viscous moisture which overlays their outside: partly also because the motion of their fins, by which the blood is made to circulate in them, having no place in the free air, the blood must needs stagnate in that element; though some fishes, especially

* This expert anatomist and distinguished medical writer was born at Leyden, in 1646. He held the professorship of physic at Kiel, and was chief physician to the Duke of Holstein. He died in 1706; aged 60. In his treatise de Purgant. Med. Facultatibus, (which contains many excellent observations) he has described more accurately than had been done by preceding authors, the villous coat of the intestines, the intestinal glands, &c. This, with his Dissertations de Fabrica et Us Cordis, and his Observat. Physico-Medicarum Libri III. are among his most valuable works. Besides the above-mentioned tract, he also wrote de Habitu et Colore Æthiopum; and he is moreover supposed to have been the author of a satirical piece against Sylvius and de Graaf, entitled Metamorphosis Æsculapii et Apollinis Pancreatici.

those that emit, and are covered with a very viscous moisture, as tenches, skates, eels, will live longer in air than others. Here he notes that fish under conglaciated water die not so much for want of air, as from the plenty of the vapours that issue from the warm bottom.* To all which he adds the reason, why oysters, lobsters, shrimps, and the like, survive longer in the air than other inhabitants of the water. Concluding this chapter with an account why the serpentine kind grow torpid of themselves in winter, and after revival cast their skins every year.

Fourthly, he discourses of some quadrupeds hiding themselves in caves during winter, as bears, hedge-hogs, &c. observing that whatever the tradition be of bears sleeping all winter, and sucking now and then their paws, it will be found, that they sleep soundly at first for a good while, but afterwards awaken and live upon some provision they have stored up for that dead time of winter: and as to the oleous moisture sweating out of the tubulous channels of their feet, that that has no other use than to soften and smooth, by being licked up, the sinuosities of the stomach and bowels that had by long abstinence been much corrugated, and so prepare them again for the new food to be taken in by the animal.

Fifthly, he inquires how far it is possible for men to live without air. Where he relates first an example, upon his own knowledge, of a woman strangled, who was recovered to life by a good dose of spirit of sal ammoniac; adding, that doubtless many such might be recovered if the like brisk spirits, together with bleeding and friction, were employed. Then he inquires into the possibility of the living of men under water; where he begins with the consideration of the difference there is between the life of embryos and urinators or divers, representing that the former need no other air than what is conveyed into them by the mother's rarified blood, being imbued with an aërial ferment; but that the latter, the divers, that is, such as use no art, are of that temper and constitution, that their blood being colder than that of others, and there arising but a slender effervescence of the blood in the heart, there is no quick circulation, nor a necessity of expiring any great plenty of sharp and offensive fumes; which kind of blood the author compares to that of fishes, or rather to that of amphibious animals, as frogs, tortoises, &c.†

On this occasion he relates that extraordinary example of a Swedish gardener, lately alive, who some years ago endeavouring to help another who was fallen

* In shallow waters, may not the freezing cold itself prove fatal to them?

† Even on the supposition that the foramen ovale remains unclosed in the case of some divers, yet can the fact of their continuing for some minutes under water, without a supply of fresh air, only be referred to the power of habit.

into the water under the ice, fell into it himself to the depth of 18 Swedish ells; where afterwards he was found standing upright with his feet on the ground, and whence they drew him up, after he had remained there for the space of 16 hours, wrapping him about close with linen and woollen clothes, to keep the air from too suddenly rushing upon him, and then laying him in some warm place, and rubbing and rolling him, and at length giving him some very spirituous liquor to drink; by all which he was at length restored to life, and brought to the queen-mother of Sweden, who gave him a yearly pension, and showed him as a prodigy to divers persons of quality; the same thing being also confirmed by the famous Dr. Langelot, who himself received the relation in Sweden so well attested, that nothing, says our author, can be required more to prove a historical truth. To which narrative are here subjoined some others, so much more prodigious, that we want confidence to insert them here.

To solve these strange phænomena, Dr. Pechlinius pretends, that there remained in these persons some, though very languid and obscure, motion of the blood and spirits, and that that motion was reduced ad interiora, and there confined to a small compass, without circulation. And that it may not be thought impossible that the blood should get into the lungs destitute of motion, our author alleges the life of urinators, in whom it is manifest that there is a motion of the heart and blood, and yet the respiration suppressed, &c.*

Description of a Hydraulic Engine, from the Register of the Royal Academy of Sciences of Paris, and inserted in the Journal des Sçavans, 1675. N° 128, p. 679.

The effect of this engine is to throw out water to a great distance by the compression of the water forced out through a tube, which turning every way, is thereby fitted to direct the jet of water to the places where the fire is to be extinguished. What is most peculiar in this engine is, that the course of the water, issuing out of the tube that darts it, is continued, not being interrupted, even when the compression of the pump's sucker ceases to act.

The engine is a chest of copper, A, (fig. 6, pl. 10), transportable by means of wooden bars like a sedan or chair. This chest is pierced above with many holes BB, holding within it the body of a pump EFM, whose sucker DE is raised and lowered by two levers CC, having each of them two arms, so as to be worked by the hands of a man. Each lever is pierced in the middle by a

* The experiments of modern physiologists have placed it beyond a doubt, that in the case of drowning, man and other breathing animals are destroyed by the suppression of respiration; in consequence of which the blood ceases to be furnished with a principle (oxygen) essential to life.

mortise *aa*, in which an iron nail, which passes through the handle of the sucker, turns round when that sucker is raised or lowered. Near the body of the pump is a copper vessel IHK, communicating with it by the tube G, and having another tube KNL, which at N may be turned every way.

To make this engine play, water is poured upon the chest to enter in at the holes that are in the cover. This water is drawn into the body of the pump at the hole F, when the sucker is raised; and when the same is let down, the valve of the hole F shuts, and forces the water through the hole M into the tube G, of which the valve H being lifted up, the water enters into the pot, and filling the bottom, it enters through the hole K into the tube KNL, in such a manner, that when the water is higher than the tube KNL, and the hole of the tube G is shut by the valve H, the air inclosed in the vessel has no vent; and when you continue to make the water enter into the vessel by the tube G, which is much thicker than the aperture of the end L, at which it issues, it must needs be, that the surplus of the water that enters into the vessel, and exceeds that which at the same time issues through the small end of the jet, compresses the air in the vessel: by which means, whilst the sucker is raised again to let new water enter into the body of the pump, the air compressed in the vessel drives the surplus of the water by the force of its spring, in the mean time that a new depression of the sucker makes new water to enter, and causes also a new compression of air. And thus the stream of the water, which issues by the jet, is made to play continually.

Extract of a Letter from Signor Cassini to the Author of the Journal des Sçavans, containing some Advertisements to Astronomers about the Configurations of the Satellites of Jupiter, for the Years 1676 and 1677. N° 128, p. 681.

The configurations of Jupiter's satellites, which are observed this year 1676, and which may be observed the next year, are of so great importance to the verifying of their hypothesis, that Signor Cassini thought fit to advise astronomers, not to let this occasion slip, which presents itself only twice in 12 years, of observing them with care and attention. For, by comparing the observations of this year with those of the next, they will find an apparent inversion of the whole system of the satellites, which will happen towards the end of March next, according to his particular hypothesis, which he proposes to verify by comparing these observations with those of Galilæus, Marius, and Hodierna, who undertook to give tables of their motions.

Since the satellites have the centre of Jupiter for the centre of their particular motions, and that the circles they describe are not directly opposite to the earth nor the sun, there is always a part of each of those circles inferior to Jupiter, and another superior to him; and this being compared to the centre of the apparent disk of Jupiter, is sometimes turned to the south, sometimes to the north, by a perpetual change of inclination to our visual ray. Galilæo believed formerly, that he had found rules for this phenomenon, or perpetual change of inclination, by supposing the planes of those circles to be always parallel to the ecliptic; for by Galilæo's supposition, the satellites in the superior part of their circles should have their latitude, in respect of the centre of Jupiter, always contrary to the latitude of Jupiter in respect of the ecliptic; which the observations of this year contradict, forasmuch as the satellites, being in the superior part of their circles, near to their conjunction with Jupiter, have also the meridional latitude in respect of his centre, as Jupiter has since the month of March, in respect of the ecliptic.—The contrariety of latitude between one satellite being in the superior part of his circle, and another being in the inferior part of his, is more sensible in the encounter of a direct, which is always superior, with a retrograde, which is always inferior, and particularly near to Jupiter.

Signor Cassini foresees, 1. That at the end of March next, the satellites will no more have any latitude in respect of Jupiter's centre, and that they will appear in a straight line in all their configurations between themselves and with Jupiter, and will eclipse each other: which, according to Galilæo, should have come to pass ever since the first months of this present year, when Jupiter passed from the north-side to that of the south, and not the next year, when Jupiter will have a great southern latitude. 2. That the straight line of the satellites will be inclined to the ecliptic, contrary to the Galilæan hypothesis. 3. That this disposition of the satellites, in a straight line in their encounter, will last but a few days, though Galilæo assures us that it lasts many months. 4. That the next summer the situation of the circles of the satellites will be found inverted, in respect of that which they have now; for, the superior semicircles which at present are turned to the south, will then be turned to the north: which will overthrow the hypotheses of Marius and Hodierna, who suppose them always turned the same way.

These observations will serve to verify the nodes of the orbs of the satellites with the orbit of Jupiter, and the obliquity of the one to the others; which are the two keys to the theory of the satellites. Signor Cassini settles these nodes towards the 13th degree of Leo and Aquarius; but Galilæo supposed them always to be with the nodes of Jupiter, which are towards the beginning of Cancer and Capricorn. He finds the obliquity of their circles to the orbit of Jupiter almost double to the obliquity of this orbit to the ecliptic; whereas Galilæo supposes it equal.—Lastly, Cassini retracts the motion, which he introduced to the nodes of the satellites, such as is described at the end of his first tables, only to reconcile the observations of Galilæo with his, and he acknowledges that the obliquity of their circles is permanent.

The goodness of Signor Cassini's system, and the imperfection of the hypotheses of Galilæo are demonstrated by the eclipses of the satellites, which happen conformable to the calculus of Cassini, and differ by days and hours from the calculus and predictions made upon the hypotheses of Galilæo: besides that there should happen a great many which do not happen according to the system of Cassini. E. g. according to the hypothesis of Galilæo, the fourth of the satellites should have more than 90 eclipses in a year, of the duration of three or four hours; but according to the system of Cassini, the same satellite will be three or four years without suffering any eclipse. Which proceeds from nothing but the false situation of the orbs supposed by Galilæo; as the great difference of the time of the eclipses that happen depends on this, that neither Galilæo nor the other astronomers do separate from the proper motion of the satellites, the appearances which befal it by that of Jupiter about the sun. And therefore it is, that they have taken for a simple and equal motion, a motion compounded of an equal and unequal; whence they have fallen into an error about the mean motions, which in progress of time has so increased, that the configurations drawn from their hypotheses for that time, have hardly any likeness at all with those that are observed.

These old hypotheses were therefore far from serving to find out the longitudes, as their authors intended them; since it was impossible for them not only to observe the eclipses of the satellites for some years to the nearness of an hour, but even to make us know and distinguish at this time one satellite from another, whereas, by the system of Signor Cassini, we may predict for many years to come the eclipses of the satellites with as much preciseness as those of the sun and moon by the astronomical tables.

A Direct and Geometrical Method of investigating the Aphelia, the Eccentricities, and the Proportions of the Orbits of the primary Planets, without supposing the Equality of the Angle of Motion at the other Focus of the Planets Ellipsis. By Mr. Edmund Halley. Translated from the Latin.* N° 128, p. 683.

The annual motion of the earth in the ecliptic, is the cause of an optical inequality in the motions of the other planets, well known to the Copernican

* Dr. Edmund Halley, a celebrated mathematician and astronomer, as well as one of the most eminent and useful members of the Royal Society, was born in London, An. 1656, and educated at St. Paul's School there; from whence he was sent 1673 to Queen's College, Oxford, where he

astronomers by the name of the parallax of the orbit; this inequality, which is easily discovered by observations, I make the solid basis of the following

chiefly applied himself to mathematics and astronomy, in which he soon distinguished himself in a remarkable manner, being only in his 19th year when he produced the above paper in the Philosophical Transactions, on the aphelia and eccentricity of the planets. He made a great number of accurate observations in astronomy; and the forming in that way an entire new catalogue of all the stars was a favourite object; but finding that project already occupied by Hevelius and Flamsteed, he formed the design of completing their scheme, by the addition of the stars about the south pole, which could not be seen by those astronomers in the latitude of Dantzic or Greenwich. For this purpose, he left the university before he had taken any degree, and sailed for the island of St. Helena in 1676, when he was only 20 years of age. Here, with great diligence he soon completed his catalogue of those stars, with which he returned to England the latter end of 1678, when the Royal Society immediately elected him one of their members, and the king (Charles 2d) gave him a mandamus to the university of Oxford, for the degree of A. M. In 1679 he went to Dantzic, at the request of the Royal Society to endeavour to adjust a dispute between M. Hevelius and Mr. Hook, concerning the preference between plain and telescopic sights in astronomical instruments; from whence he returned in about two months.

In 1680 he set out on a tour through France and Italy, to establish a friendly communication among the astronomers of Europe. In Paris he completed his observations on the great comet of that year, which he had before seen in England. He returned to England in 1681, and married a lady, with whom he lived happily for 55 years after. In 1683 he published his "Theory of the Variation of the Magnetical Compass;" in which he supposes the whole globe of the earth to be one great magnet, having four magnetical poles or points of attraction, &c. The same year also he entered on a new method of finding the longitude, by an accurate observation of the moon's motion. In the beginning of 1684, contemplating Kepler's laws of the periods and distances of the planets, he concluded that the centripetal force must decrease in proportion to the square of the distance reciprocally. He found himself however unable to make it out in any geometrical way; and therefore, after applying in vain for assistance to Mr. Hook and Sir Christopher Wren, he repaired to Cambridge to Mr. Newton, who fully supplied him with what he so ardently sought. But Halley having now found an immense treasure in Newton, could not rest till he had prevailed with the owner to enrich the public with it; and to this interview the world is in some measure indebted for the immortal Principia of Newton. That great work was published in 1686; and Halley, who had the whole care of the impression, prefixed to it a discourse of his own, giving a general account of the astronomical part of the work; and also an elegant copy of verses in Latin. In 1687 he undertook to explain the reason why the Mediterranean Sea never rises higher, though there is no visible discharge of the prodigious quantity of water that runs into it from nine large rivers, besides many small ones, and the constant setting in of the current at the mouth of the Straits; which he accounted for by the great quantity of waters raised from its surface by evaporation, which he showed by a calculation was fully adequate to the purpose. Halley's active and elevated mind next ranged through various other fields of science; hence resulted his tracts on the construction of solid problems, or equations of the 3d and 4th powers, with a new method for the number and the limits of their roots; exact tables of the conjunctions of Venus and Mercury, with their use in discovering the parallax and distance of the sun; new tables for showing the values of annuities on lives, calculated from bills of mortality; the universal theorem for finding the foci of optic glasses. But it would be endless to enumerate all his valuable discoveries, then communicated to the Royal Society, and published in the Philosophical Transactions, of which for many years his pieces were the chief ornament and support, in all the method; where, besides observations, nothing more is supposed, than that the orbits of the planets be elliptical, that the sun be in their common focus, and in fine, that their periods be discovered in such a manner as that no sensible error can arise, at least in two or three revolutions.

These things being granted, let S be the sun, fig. 5, pl. 10, ABCDE, the orbit of the earth; P the planet Mars, which is to be preferred on many accounts; and in the first place let the true time and place wherein Mars is in

sciences, astronomy, geometry, and algebra, optics and dioptrics, ballistics and artillery, speculative and experimental philosophy, natural history, antiquities, philology, and criticism; all abounding with ideas new, singular and useful.

In 1691, the Savilian professorship of astronomy at Oxford being vacant, Mr. Halley applied for that office, but without success: refusing to deny or conceal his sceptical turn of mind, though his own extraordinary merits were supported by the interest of Newton, he was rejected, and the office bestowed on Dr. Gregory. In 1698 he procured from King William the appointment of captain of a ship, sent out for the express purpose of establishing his theory of the variation of the compass, which he had advanced in 1683. He made another voyage on the same design the year following, and returned to England in September 1700, with numerous observations; from whence he soon after published his general chart, exhibiting at one view the variation of the compass in all these seas where the English navigators were acquainted. He was also soon after sent out again on a third voyage, to ascertain the course of the tides in every part of the British channel, of which, in 1702, he published a large chart. Soon after, at the request of the Emperor of Germany, he made two journeys, to inspect the coasts of the Adriatic Sea, and to examine certain ports, which the emperor intended to construct or improve. He returned in 1703, when he was appointed to succeed Dr. Wallis as professor of geometry at Oxford, and was at the same time honoured with the degree of doctor of laws. Here he soon employed himself in translating into Latin, from the Arabic, Apollonius's Section of a Ratio, and in restoring the same author's two last books on the Section of a Space, from the account given of them by Pappus; which were published in 1706. He next prepared an edition of the whole works of Apollonius, and ventured to supply the whole 8th book of the Conics, the original of which was lost. To this he added, Serenus on the Sections of the Cylinder and Cone, in Greek, with a Latin translation; and published the whole in 1710. Besides these, the Miscellanea Curiosa, in 3 volumes 8vo, had come out under his direction, in 1708, consisting chiefly of pieces of his own, extracted from the Philosophical Transactions.

In 1713, Dr. Halley succeeded Sir Hans Sloane, in the office of Secretary to the Royal Society; which he resigned in 1721, having been appointed Astronomer Royal on the decease of Mr. Flamsteed in 1719. And although he was now 63 or 64 years of age, yet here for the space of 18 years he watched the heavens with the closest attention, hardly ever missing an observation, and, without any assistant, performed the whole business of the observatory himself.

About 1737 he was seized with a paralytic disorder in one of his hands. However, he still continued to come regularly once a week, to meet his friends in town on Thursdays, before the meeting of the Royal Society, at what is still called Dr. Halley's club. But his paralytic disorder increasing, his strength gradually decreased, till he expired Jan. 14, 1742, in the 86th year of his age; and his corpse was interred in the church-yard of Lee, near Blackheath.—Beside the works beforementioned, Dr. Halley's principal publications are, 1. Catalogus Stellarum Australium. 2. Tabulæ Astronomicæ. 3. The Astronomy of Comets. With a multitude of papers in the Philosophical Transactions, from volume xi to volume lx.

opposition to the sun, be observed; for then the sun and the earth are in the same right line with Mars, or, which always is the case when it has latitude, with that point where a perpendicular let fall from Mars meets the plane of the ecliptic. Thus, in the scheme, S, A and P are in a right line; then after 687 days Mars returns to the same point P, where it was in opposition to the sun in the former observation; but since the earth does not return to A till after 730½ days, in B it respects the sun in the line SB, and Mars in the line BP; and observing the longitudes of the sun and of Mars, all the angles of the triangle PBS are given; and supposing PS 100000 parts, the length of the line SB is found in the same parts; in the same manner, after a second period of Mars, the earth being in C, the line SC is found, and in like manner the lines SD, SE, SF; and the differences of the observed places of the sun, are the angles at the sun ASB, BSC, CSD, DSE: and thus at length we come to this geometrical problem, viz. Three lines meeting in one of the foci of the ellipsis being given, both in length and position, it is required to find the length of the transverse diameter, together with the distance of the foci: the resolution of which is also extended to the other planets, if, after knowing the theory of the earth's motion, we investigate, according to the method proposed by Dr. Ward, Bishop of Sarum, in his Astronomiæ Geometrica, lib. 2, part 2, cap. 5, three distances of any planet from the sun in their positions. But because the doctor supposes a planet to move in its orbit in such a manner, as in equal times to describe equal angles about the other focus, and upon this builds his calculation; it does not seem improper to show how the same thing may be done without supposition, that which is inconsistent with observation.

Let S, fig. 7, be the sun; ALBK the orbit of the earth; and let P be the planet, or the point in the plane of the ecliptic, where a perpendicular from the planet meets it; let AB be the line of the apses of the earth's orbit. In the first place, let the longitude and latitude of the planet in P be observed, together with the longitude of the sun from the earth in K; and after a revolution of the same planet, the earth being in L, let again the positions of the planet and of the sun be observed as before. Now from the observed longitudes of the sun, and aphelion of the earth, the angle ASK, ASL are given, and consequently the sides SK, and SL: for if the angle of the co-equated anomaly be acute, the proportion is, as the difference of the mean distance and of the cosine of the angle multiplied into the eccentricity, is to the aphelion distance, so is the parhelion distance, to the distance of the planet from the sun in the given anomaly; but if the angle be obtuse, the first term of the proportion is the sum of the two parts, as it was their difference in the former analogy: now in the triangle KSL, the sides KS, LS, and the angle KSL are given; to find the side

KL, and the angles SKL, SLK. Again, in the triangle KLP, are given the side KL, the angle KPL, the difference of the observed longitudes of the planet, and PKL, the difference of the angles SKL last found, and SKP the elongation of the planet from the sun in the first observation, to find LP: then, in the triangle LSP, the sides LS, LP, and the angle PLS, the elongation of the planet from the sun in the second observation, are given, to find the side SP, and the angle LSP; which being found, then as SP is to LP, so is the tangent of the latitude observed from L, to the tangent of the inclination or latitude at the sun; and as the cosine of the inclination is to the radius, so is SP the curtate distance, to the planet's true distance from the sun. And thus we find the position and length sought. Now it remains to show, how from three given distances from the sun, together with the intercepted angles, the mean distance may be found, with the eccentricity of the ellipsis.

Let S, fig. 8, be the sun, and SA, SB, SC three distances in due position; and drawing AB, BC, let AB be the distance of the foci of an hyperbola, and SA − SB = EH the transverse diameter: which being supposed, let the hyperbolic line be described, whose internal focus is the point A, the extremity of the longer line SA: after the same manner, let B and C be the foci of the other hyperbola, whose diameter SB − SC = KL; from which let the hyperbolic line be described, having its internal focus in the point B. I say these two hyperbolas, thus described, intersect each other in the point F, which is the other focus of the ellipsis; and drawing the line FA, FB, or FC; SA + FA, SB + FB, or SC + FC will be equal to the transverse diameter, and SF is the distance of the foci; which being supposed, the description of the ellipsis is very easy. But since the reason of this construction is not clear to every one, it will not be improper to illustrate it a little; therefore I say, that from the known property of the ellipsis, SB + FB = SA + FA, and transposing the members of the equation, FB − FA = SA − SB; so that though FB and FA be unknown, yet their difference is equal to SA − SB, that is, EH; and since from the nature of the hyperbola, any two lines drawn from the foci to any point of the curve constantly differ by the quantity of the transverse diameter; it is plain that the point F is somewhere in the curve of the hyperbola, whose transverse diameter is equal to SA − SB, and the foci A and B. After the same manner it may be demonstrated that the point F is in the hyperbola, whose diameter is SB−SC, and foci B and C. Therefore it must necessarily be in the intersection of these two hyperbolas, which since they intersect each other in one point only, clearly show where the other focus of the required ellipsis is.

Now to perform the same analytically, suppose it done, and let FB = a, SA − SB = FB − FA = b, AB = c, SB − SC = FC − FB = d, BC = f, and let the sine of the angle ABC = S, and its cosine = s. Then as $c : b :: 2a - b : \frac{2ab - bb}{c}$ and, $\frac{2ab - bb + cc}{2c}$ = BD by 36. 3. Eucl. and as $f : d :: 2a + d : \frac{2ad + dd}{f}$; and $\frac{ff - 2ad - dd}{2f}$ = BG by the same proposition; and to lessen the trouble of calculation, let $\frac{cc - bb}{2c} = g$, and $\frac{b}{c} = h$, likewise let $\frac{ff - dd}{2f} = k$, and $\frac{d}{f} = l$; then BD $= g + ha$, and BG $= h - la$; and because in every $\left\{\begin{matrix}\text{obtuse angled triangle}\\ \text{acute angled triangle}\end{matrix}\right\}$ the square of the base is equal to $\left\{\begin{matrix}\text{the sum}\\ \text{the difference}\end{matrix}\right\}$ of the sum of the squares of the sides and of double the rectangle of the sides multiplied into the cosine of the comprehended angle, it will be $gg + 2gha + hhaa + kk - 2kla + llaa + 2gks - 2glsa + 2khsa - 2hlsaa$ equal to the square of DG; but DG is equal to the sine of the angle DFG or DBG multiplied into a, that is, into FB; for the quadrilateral FBDG is inscribed in a circle whose diameter is FB: therefore SS$aa = gg + 2gha + hhaa + kk - 2klz + llaa + 2gks - 2glsa + 2khsa - 2hlsaa$, which equation is easily resolved, since it does not exceed an adfected quadratic, and always consists of these squares and rectangles; but the signs + and − are to be very cautiously used to the rectangles, on account of the different nature of the three lines. We have fitted our equation to fig. 8; but in any other case it will not be difficult from what is said above to constitute a similar one; and thus it is shown how from three heliocentric places of a planet, and the observed distances from the sun, its orbit may be described; which formerly required five such observations.

An Extract of an account given by Mr. Flamsteed, of his own and Mr. Edmund Halley's Observations concerning the Spots in the Sun, appearing in July and August, 1676. N° 128, p. 687.

The following ephemeris of their daily places, was reduced from careful observations, made with the micrometer, of the distances of the spots from the limb of the sun, and the differences of altitudes and azimuths from the upper and under parts and sides of him. The comparing of the observations made in two distant places, Greenwich and Oxford, evince the diligence of the observers, and the goodness of their instruments; the differences between them being easily excusable; for that the spot had a diameter more considerable than any of the differences, and was broken into several pieces.

Mr. Halley says, that he saw the spot again on the fifth day at 8 h. 30 m. mane, very near the limb of the sun, so that it appeared only as a fine line; but by reason of its fineness, and the too great height of the sun, he could not take any measures to determine its place and latitude by; and that, while the spot continued one, as it was July 25, he measured to the middle of it; as also when the pieces were divided, but not far disjoined: afterwards, when they were separated considerably, he observed the middle of the larger spot, which was to the south, apparently, I suppose; but really north: for so only his observations will agree with those of Mr. Flamsteed exactly.

Hence it seems very evident, says Mr. Flamsteed, that the spot's way was not inclined to the ecliptic six or seven degrees, as Scheiner and some others make it, but much less, by the joint consent of the observations of both our observers. Mr. Halley adds, that considering the motion of the spot across the sun's disk, as both their observations give it, it appears, that the latitude was not so great at its entrance into the sun as in the middle of him. And by Mr. Flamsteed's observation it was greatest on the 1st of August, and then again inclining towards the ecliptic. If you grant this, it will follow, infers Mr. Flamsteed, that the sun's axis was inclined to the plane of the orbis magnus; but the quantity of this inclination must not be very great. The nodes of the sun's equinox and ecliptic he guesses to be not far from the beginning of Cancer and Capricorn; and that from Cancer to Capricorn the earth is north of the sun's equator, from Capricorn to Cancer, south of the same: and the period of the sun's revolution, in respect of the fixed stars, 25 days, 9½ hours sufficiently exact. Of which things, these two observers say, they might have been more certain, had not the spot in its passage broken into so many parts, and those often varied their positions to each other. These conjectures, though probable, yet when another of the like phænomena appears, will still deserve the further consideration of the curious.

An Extract of Signor Cassini's Letter concerning a Spot lately seen in the Sun: together with a remarkable Observation of Saturn, made by the same. N° 128, p. 689. *Translated from the Latin.*

We here observed the same solar spot as Mr. Flamsteed, from August 6 to 14 N. S.; and by comparing our observations, we found that it was in the middle of its course, in the sun's apparent disk, about midnight after the 8th of August, at the apparent distance of 3 minutes from the centre towards the south. It is divided into several parts, which separate from each other daily towards the north and south; so that besides their commensuration about the sun's axis, all the parts have a direct motion of their own among themselves. I think this spot is different from that which we observed in the preceding month of June. For since that had the middle of its course in the apparent disk of the sun, on the 28th day of the same month, it must have returned nearly to the same situation, had it still existed, in the night following the 25th of July; as is deduced both from its velocity, the observed time of its appearance, and also from the course of the other spots, which we have seen to finish their period about the sun in 27$\frac{1}{3}$ or 27$\frac{1}{2}$ days. Besides, its path is different from the preceding; for the former was something more remote from the equator of the spots, than the latter. And this, if it should have consistence enough, should return to the middle of the sun on Sept. 5, in the morning.

From the scheme of the appearance of Saturn, as observed by M. Hevelius a year ago, I perceive that he made use of telescopes much inferior to our's. For at that time, and now also (Aug. 1676), in the globe of Saturn we observed an obscure zone, a little south of the centre, not unlike the zones of Jupiter. Then the breadth of the annulus was divided into parts, by an obscure line, apparently elliptical, but in reality circular, as it were into two concentric rings, the interior of which was brighter than the exterior. This phasis I saw immediately after the emersion of Saturn from the sun's rays, through the whole year, quite to his immersion; first with a telescope of 35 feet, and afterwards with a less, of 20 feet. See fig. 1, pl. xi.

An Intimation given in the Journal des Sçavans, of a sure and easy way to make all sorts of large Telescopical Glasses; with a generous offer of furnishing industrious Astronomers with them gratis. By M. Borelli. N° 128, p. 691.

Mons. Borelli, a member of the Royal Academy of Sciences of Paris, whose love for natural philosophy, and chiefly chemistry, has been long known, has found out a sure and very easy method of working all sorts of large telescopical glasses. He has already carried the experience of his secret to great extent, having made one of them very good of 200 feet, wrought on both sides on the same rule: which shows, that if he had wrought it flat on both sides, the glass would have been of 400 feet.

This easiness of making large glasses, and the desire of procuring some advancement to astronomical discoveries, have induced him to make presents of them in divers places to several persons capable of using them: and the same motive induces him to make the like offer, not only to the astronomers that are dispersed up and down in the kingdom of France, but also to those that are in foreign countries, especially in those parts, where there is some established academy or society for astronomical observations; offering in this case, to every one of such societies, three very good glasses, one of 10 or 12 feet for a chamber: another of 25 or 30 feet for ordinary observations, and a third of 60 or 80 feet for making new discoveries.

Private persons who are not in a condition to make engines for large glasses, may at least make use of glasses of 14 or 20 feet, which he is willing to send them, to observe the eclipses of Jupiter's satellites, which happen almost daily, and afford so fair a way for establishing the longitudes over all the earth. For, besides that these eclipses are very frequent, the emersion and immersion of these satellites, especially in the shadow of Jupiter, is so momentary and so sensible, that they may be observed with the greatest exactness, being altogether exempt from those essential inconveniencies that accompany the eclipses of the sun and moon, which also are rare, and whose beginning and end are always doubtful, by reason of a certain ambiguous light.

Since M. Borelli has found this way of working glasses, he entrusted the secret of it to a member of the academy above-mentioned; and he proposes to publish the same hereafter, with some other considerable observations on the same glasses.

Exceptions against Mr. Newton's Experiments, and Theory of Light and Colours. By Mr. Lucas of Liege. N° 128, p. 692.

Mr. Gascoigne having received your obliging letter of Jan. 18, with fresh directions from Mr. Newton; but wanting convenience to make the experiment, he has requested me to supply his want. In compliance with this request I have made many trials; the issue whereof I here acquaint you with; next, with some exceptions, grounded on experiments, against Mr. Newton's new theory of light and colours.

The verticle angle of my prism was 60 degrees, the distance of the wall, whereon the coloured spectrum appeared, from the window, about 18 feet. The diameter of the hole in the window-shutters about $\frac{1}{6}$ of an inch, which was occasionally contracted to half the said diameter; but still with equal success as to the result of the experiment. The refractions on both sides the prism, were equally as near as I could make them, and consequently about 48° 40′, the refractive power of glass being computed according to the ratio of the sines 2 to 3. The distance of the prism from the hole in the shutter was about 2 inches: the room darkened to that degree as to equal the darkest night, while the hole in the shutter was covered.

Now as to the issue of my trials; I constantly found the length of the coloured image, transverse to the axis of the prism, considerably greater than its breadth, whenever the experiment was made on a clear day; but when a bright cloud was near the sun, I found it sometimes exactly as Mr. Line mentions, viz. broader than long, especially while the prism was placed at a great distance from the hole. Which experiment will not, I conceive, be questioned by Mr. Newton, it being so agreeable to the received laws of refractions. And indeed the observations of these two learned persons, as to this particular, are easily reconcileable to each other, and both to truth; Mr. Newton contending only for the length of the image, transverse to the axis of the prism, in a very clear day; and Mr. Line only maintaining the excess of breadth, parallel to the same axis, while the sun is in a bright cloud. Though as to what is further delivered by Mr. Newton, and opposed by Mr. Line, namely, that the length of the coloured image was five times the diameter of its breadth; I never yet have found the excess above thrice the diameter, or at most $3\frac{1}{4}$, while the refractions on both sides the prism were equal. So much as to the matter of fact.

Now as to Mr. Newton's theory of light and colours, I confess, his neat set of very ingenious and natural inferences, was to me on the first perusal a strong conjecture in favour of his new doctrine. But since several experiments of refractions remain still untouched by him, I conceived a further search into them would be very proper, in order to a better discovery of the truth of his assertion. For, accordingly as they are found either agreeing with, or disagreeing from, his new theory, they must needs much strengthen, or wholly overthrow the same. The experiments I pitched upon for this purpose, are as follow:

1. Having frequently observed that the form of objects viewed in the microscope, or rather of the microscope itself, consists almost in an indivisible point, I concluded that two very small pieces of silk, the one scarlet, the other violet colour, placed near together, should, according to Mr. Newton's theory, appear in the microscope in a very different degree of brightness, in regard that their unequal refrangibility must cause the scarlet rays or species to over-reach the retina, while placed in the due focus of the violet ones, and consequently occasion a sensible confusion in the vision of the former, one and the same point of the scarlet object affecting several nerves in the retina. Yet on frequent trials I have not been able to perceive any inequality in this point.

2. The second experiment was made in water. I took a brass ruler, and fastening to it several pieces of silk, red, yellow, green, blue, and violet, I placed it at the bottom of a square vessel of water: then I retired from the vessel so far as not to be able to see the ruler and coloured silks otherwise than by help of the refracted ray. Now, did Mr. Newton's doctrine hold, I conceived I should not see all the said colours in a straight line with the ruler, as the unequal refrangibility of different rays must needs displace some more than others. Yet in effect, on many trials, I constantly found them in as straight a line as the bare ruler had appeared in. 3. To confirm this experiment, I added a second refraction to the former of the water, by placing my prism so as to receive perpendicularly the refracted species of the silk and ruler; by which only the emergent

species suffered a second refraction. But still with equal success, as to their appearing in a straight line, to the eye placed behind the prism. 4. To these two refractions I further added a third, by receiving the coloured species obliquely on the prism; by which both incident and emergent species suffered their respective refractions. But still with the same success as formerly, as to the straight line they appeared in.

For further assurance in this experiment, lest prepossession, occasioned from previous knowledge of the silks situation in a straight line, might possibly prejudice the judgment of the eye, as sometimes I have observed to happen to the judgment the eye passes on the distance of objects, I called into the room some unconcerned persons, wholly ignorant what the experiment aimed at; and demanding whether they saw not the coloured silks and ruler in a crooked line? they answered in the negative.

5. The next experiment I made in uncompounded Colours, (as Mr. Newton terms them, prop. 5 and 13) as follows: Having cast two coloured images on the wall, so as the scarlet colour of the one fell in a straight line, parallel to the horizon, with the violet of the other; I then looked on both through another prism, and found them still appear in a straight line parallel to the horizon, as they had formerly done to the naked eye. Now according to Mr. Newton's assertion of different refrangibility in different rays, I conceive the violet rays should suffer a greater refraction in the prism at the eye, than the scarlet ones, and consequently both colours should not appear in a straight line parallel to the horizon.

6. Another experiment I made, in order to some further discovery of that surprising phenomenon of the coloured image, which occasioned Mr. Newton's ingenious theory of light and colours, as also his excellent invention of the reflecting telescope and microscope. Having then sometimes suspected, that not only the direct sun-beams, but also other extraneous light, might possibly influence the coloured spectrum, I hoped to discover the truth of this suspicion by means of the sun-spots, made to appear in the coloured image by placing a telescope behind the prism. But my endeavours proving ineffectual herein, by reason of some intervening difficulties, I thought at length of a more feasible method in order to the designed discovery, as in the following experiment.

I fastened a very white paper circle, about an inch in diameter, on my window-shutter; and viewing it through my prism, I found a coloured image painted by it on my retina, answerable in almost all respects to the image of the sun-beams on the wall, especially when the paper circle was indifferently well illuminated. This image indeed appeared contrary to the former as to the situation of colours, that is, the scarlet appearing above, the violet below, though but faint.

But this I was not surprised at, having observed, on dissecting the eye, that objects are painted on the retina after a contrary posture to what they appear to sight. Having thus rendered the coloured image much more tractable than formerly it was, I conceived good hopes of some further discovery in this point.

In pursuance then of my former suspicion, having fixed my prism in a steady posture, I caused the paper C. (fig. 2, 3, pl. xi.) to be applied close to the paper circle a b d: whereupon the former violet d, and scarlet colour of C, vanished into whiteness. Next, I removed the circle from the shutter, and placed it in the open window, supported only by the edge d: on which, to my astonishment, all the former colours exchanged postures in the retina, the scarlet now appearing below, the violet above; the intermediate colours scarcely discernible. And here, by the bye, it is very remarkable, that during this observation I clearly perceived both blue and scarlet light to be transparent, being able to discern several objects through both, namely, steeples opposite to my window. Whence it follows, that these colours do in great part arise from the neighbouring light. Lastly, I placed the paper circle anew, so as the one half b, was fastened to the shutter, the other semicircle a, being exposed to the open air. On which the semicircle a, became bordered with violet above, and scarlet below: but the other semicircle b, quite the reverse. Hence I make the following inferences.

First, that not only the light reflected from the paper circle, but also from the ambient air, has great influence on the coloured image, especially as to the violet and scarlet colours. Whence perhaps it will not hereafter seem strange, that the coloured spectrum on the wall is so long, but only that the breadth is not greater. Secondly, were there a more luminous body behind the sun, we should in all likelihood have the colours of the spectrum in a contrary situation to what they appear in at present: whence, thirdly, it seems to follow, that the present situation and order of colours arise not from any intrinsical property of refrangibility, as maintained by Mr. Newton, but from contingent and extrinsical circumstances of neighbouring objects. For accordingly as the body behind the paper-circle was more or less illuminated than the circle itself, all the several colours changed their situation.

8. The next experiment was made in order to Mr. Newton's doctrine of primary colours, as prop. 5. Having covered the hole in the window-shutter with a thin slice of ivory, the transmitted light appeared yellow; but on adding three, four, and more slices, it became red. Whence it seems to follow, that yellowness of light is not a primary colour, but a compound of red, &c.

9. The last experiment was made in reference to Mr. Newton's 12th prop. where from his own principles he renders a very plausible reason of a surprising phenomenon, related by Mr. Hook, namely of two liquors, the one blue, the

other red, both severally transparent, yet both, if placed together, became opaque. The reason whereof, says Mr. Newton, is, because if one liquor transmitted only red, the other only blue, no rays could pass through both. In reference then to this point, I filled two small glasses with flat polished bottoms, the one with aquafortis, deeply tinged blue; the other with oil of turpentine coloured red; both to that degree, as to represent all objects through them respectively blue or red. Then placing the one upon the other, I was able to discern several bodies through both: whereas according to Mr. Newton's theory, no object should appear through both liquors, because if one transmit only red, the other only blue, no rays can pass through both.

Mr. Newton's Answer to the preceding Letter. N° 128, p. 698.

Sir.—The things opposed by Mr. Line being upon trials found true and granted me, I begin with the new question about the proportion of the length of the image to its breadth. This I call a new one; for, though Mr. Line in his last letter spake against so great a length as I assign, yet as it seems to me, it was not to grant any transverse length shorter than that assigned by me, for in his first letter he absolutely denied that there would be any such length: but to lay the greater emphasis upon his discourse whilst in defence of common optics he was disputing in general against a transverse image; and therefore in my answer I did not prescribe the just quantity of the refracting angle with which I would have the experiment repeated, which would have been a necessary circumstance, had the dispute been about a just proportion of the length to the breadth. Yet I added* this note, that the larger the angle of the prism is, the greater will be the length in proportion to the breadth, not imagining but that when he had found in any prism the length of the image transverse to the axis, he would easily thence conclude, that a prism with a greater angle would make the image longer, and consequently that by using an angle great enough he might bring it to equal or exceed the length assigned by me, as indeed he might; for, by taking an angle of 70 or 75 degrees, or a little greater, he might have made the length not only five but six or eight times the breadth, and more. No wonder, therefore, that Mr. Lucas found the image shorter than I did, seeing he tried the experiment with a less angle.

The angle indeed which I used was but about 63° 12′, and his is set down 60°, the difference of which from mine being but 3° 12′, is too little to reconcile us, but yet it will bring us considerably nearer together. And if his angle was not exactly measured, but the round number of 60° set down by guess, or

* In my first letter in Philosophical Transactions, No. 121.

by a less accurate measure, as I suspect by the conjectural measure of the refraction of his prism by the ratio of the sines 2 to 3, set down at the same time, instead of an experimental one, then might it be two or three degrees less than 60, if not still less; and all this, if it should be so, would take away the greatest part of the difference between us.

But however it be, I am well assured my own observation was exact enough. For I have repeated it divers times since the receipt of Mr. Lucas's letter, and that without any considerable difference of my observations, either from each other or from what I wrote before. And that it might appear experimentally, how the increase of the angle increases the length of the image, and also that nobody who has a mind to try the experiment exactly, might be troubled to procure a prism which has an angle just of the size assigned by me; I tried the experiment with divers angles, and have set down my trials in the following table: where the first column expresses the six angles of two prisms which I used, which were measured as exactly as I could, by applying them to the angle of a sector; and the second column expresses in inches the length of the image made by each of those angles; its breadth being 2 inches, its distance from the prism 18 feet 4 inches, and the breadth of the hole in the window shutter $\frac{1}{4}$ of an inch.

	Angles.	Lengths of image.		Angles.	Lengths of image.
First prism.	56° 10′	$7\frac{3}{4}$	Second prism.	54° 0′	$7\frac{1}{3}$
	60 24	$9\frac{1}{2}$		62 12	$10\frac{1}{8}$
	63 26	$10\frac{1}{3}$		63 48	$10\frac{3}{4}$

You may perceive, that the length of the images, in respect of the angles that made them, are something greater in the second prism than in the first; but that was because the glass, of which the second prism was made, had the greater refractive power. The days in which I made these trials were pretty clear, but not so clear as I desired, and therefore afterwards meeting with a day as clear as I desired, I repeated the experiment with the second prism, and found the lengths of the image made by its several angles to be about $\frac{1}{4}$ of an inch greater than before, the measures being those set down in this table.

	Angles.	Lengths of image.
The second prism.	54° 0′	$7\frac{2}{3}$
	62 12	$10\frac{1}{2}$
	63 48	11

The reason of this difference I apprehend was, that in the clearest days the light of the white skies, which dilutes and renders invisible the faintest colours at the ends of the image, is a little diminished in a clear day, and so gives leave to the colours to appear to a greater length; the sun's light at the same time becoming brisker, and so strengthening the colours, and making the faint ones at the two ends more conspicuous. For I have observed that in days something cloudy, whilst the prism has stood unmoved at the window, the image would grow a little longer or a little shorter, accordingly as the sun was more or less obscured by thin clouds which passed over it; the image being shortest when the cloud was brightest and the sun's light faintest. Whence it is easy to apprehend, that if the light of the clouds could be quite taken away, so that the sun might appear surrounded with darkness, or if the sun's light were much stronger than it is, the colours would still appear to a greater length.

In all these observations the breadth of the image was just 2 inches. But observing that the sides of the two prisms I used were not exactly plain, but a little convex, the convexity being about so much as that of a double convex-glass of a 16 or 18-foot telescope, I took a third prism, whose sides were as much concave as those of the other were convex, and this made the breadth of the image to be 2 inches and a third part of an inch; the angles of this prism, and the lengths of the image made by each of those angles, being those expressed in this table.

Angles of the prism.	Lengths of the image.
58°	$8\frac{1}{2}$ inches.
$59\frac{1}{2}$	9
$62\frac{1}{2}$	$10\frac{1}{3}$

In this case, you see the concave figure of the sides of the prism, by making the rays diverge a little, causes the breadth of the image to be greater in proportion to its length than it would be otherwise. And this I thought fit to give you notice of, that Mr. Lucas may examine whether his prism have not this fault. If a prism may be had with sides exactly plain, it may do well to try the experiment with that; but it is better, if the sides be about so much convex as those of mine are, because the image will thereby become much better defined. For this convexity of the sides gives the same effect as if you should use a prism with sides exactly plain, and between it and the hole in the window shutter, place an object-glass of an 18-foot telescope, to make the round image of the sun appear distinctly defined on the wall when the prism is taken away, and consequently the long image made by the prism to be much more distinctly defined, especially at its straight sides, than it would be otherwise.

One thing more I shall add: that the utmost length of the image, from the faintest red at one end to the faintest blue at the other, must be measured. For in my first letter about colours, where I set down the length to be five times the breadth, I called that length the utmost length of the image; and I measured the utmost length, because I account all that length to be caused by the immediate light of the sun, seeing the colours, as I noted above, become visible to the greatest length in the clearest days, that is, when the light of the sun transcends most the light of the clouds. Sometimes there will happen to shoot out from both ends of the image a glaring light a good way beyond these colours, but this is not to be regarded, as not appertaining to the image. If the measures be taken right, the whole length will exceed the length of the straight sides by about the breadth of the image.

By these things set down thus circumstantially, I presume Mr. Lucas will be enabled to accord his trials of the experiment with mine; so nearly at least, that there shall not remain any very considerable difference between us. For, if some little difference should still remain, that need not trouble us any further, seeing there may be many various circumstances which may conduce to it; such as are not only the different figures of prisms, but also the different refractive power of glasses, the different diameters of the sun at divers times of the year, and the little errors that may happen in measuring lines and angles, or in placing the prism at the window; though, for my part, I took care to do these things as exactly as I could. However, Mr. Lucas may make sure to find the image as long or longer than I have set down, if he take a prism whose sides are not hollow ground, but plain, or, which is better, a very little convex, and whose refracting angle is as much greater than that I used, as that he has hitherto tried it with, is less; that is, whose angle is about 66 or 67 degrees, or, if he will, a little greater.

Concerning Mr. Lucas's other experiments, I am much obliged to him that he would take these things so far into consideration, and be at so much pains for examining them; and I thank him so much the more, because he is the first that has sent me an experimental examination of them. By this I may presume he really desires to know what truth there is in these matters. But yet it will conduce to his more speedy and full satisfaction, if he a little change the method which he has propounded, and instead of a multitude of things, try only the experimentum crucis. For it is not number of experiments but weight to be regarded; and where one will do, what need many? Had I thought more requisite, I could have added more; for before I wrote my first letter to you about colours, I had taken much pains in trying experiments about them, and written a tractate on that subject, wherein I had set down at large the principal of the experiments I had tried; amongst which there happened to be the principal of those experiments which Mr. Lucas has now sent me. And as for the experiments set down in my first letter to you, they were only such as I thought convenient to select out of that tractate.—But suppose those had been my whole store, yet Mr. Lucas should not have grounded his discourse upon a supposition of my want of experiments, till he had examined those few. For if any of those be demonstrative, they will need no assistants, nor leave room for further disputing about what they demonstrate.

The main thing he goes about to examine is, the different refrangibility of light. And this I demonstrated by the experimentum crucis. Now if this demonstration be good, there needs no further examination of the thing; if not good, the fault of it is to be shown: for the only way to examine a demonstrated proposition is, to examine the demonstration. Let that experiment therefore be examined in the first place, and that which it proves be acknowledged, and then if Mr. Lucas wants my assistance to unfold the difficulties which he fancies to be in the experiments he has propounded, he shall freely have it; for then I suppose a few words may make them plain to him: whereas should I be drawn from demonstrative experiment to begin with those, it might create us both the trouble of a long dispute, and by the multitude of words cloud, rather than clear up the truth. For if it has already cost us so much trouble to agree upon the matter of fact in the first and plainest experiment, and yet we are not fully agreed; what an endless trouble might it create us, if we should give ourselves up to dispute upon every argument that occurs, and what would become of truth in such a tedious dispute? The way therefore that I propound, being the shortest and clearest, not to say the only proper way, I question not but Mr. Lucas will be glad that I have recommended it, seeing he professes that it is the knowledge of truth that he seeks after. And therefore at present I shall say nothing in answer to his experimental discourse, but this in general; that it has proceeded partly from some misunderstanding of what he writes against, and partly from want of due caution in trying experiments; and that amongst his experiments there is one, which, when duly tried, is next to the experimentum crucis, the most conspicuous experiment I know, for proving the different refrangibility of light, which he brings it to prove against.

By the postscript of Mr. Lucas's letter, one not acquainted with what has passed, might think, that he quotes the observation of the Royal Society against me; whereas the relation of their observation, which you sent to Liege, contained nothing at all about the just proportion of the length of the image to its breadth according to the angle of the prism, nor any thing more (so far as I can perceive by your last) than what was pertinent to the things then in dispute, viz.

that they found them succeed as I had affirmed. And therefore since Mr. Lucas has found the same success, I suppose that when he expressed that he much rejoiced to see the trials of the Royal Society agree so exactly with his, he meant only so far as his agreed with mine.

And because I am again upon this first experiment, I shall desire that Mr. Lucas will repeat it with all the exactness and caution that may be, regard being had to the information about it, set down in this letter; and then I desire to have the length and breadth of the image with its distance from the prism, set down exactly in feet and inches, and parts of an inch, that I may have an opportunity to consider what relation its length and breadth have to the sun's diameter. For I know that Mr. Lucas's observation cannot hold, where the refracting angle of the prism is full 60 degrees, and the day is clear, and the full length of the colours is measured, and the breadth of the image answers to the sun's diameter: and seeing I am well assured of the truth and exactness of my own observations, I shall be unwilling to be diverted by any other experiments, from having a fair end made of this in the first place. Sir, I am, &c.

Postscript.—I had like to have forgotten to advise, that the experimentum crucis, and such others as shall be made for knowing the nature of colours, be made with prisms which refract so much, as to make the length of the image five times its breadth, and rather more than less; for otherwise experiments will not succeed so plainly with others as they have done with me.

An Account of two Books. N° 128, p. 705.

I. Tractatus de Ventriculo et Intestinis, cui præmittitur alius de Partibus Continentibus in genere, et in specie de Partibus Abdominis; Auth. Franc. Glissonio, M.D. et Coll. Med. Lond. Socio, nec non Soc. Regalis Collegâ. Lond. 1676, 4to.

In this treatise Dr. Glisson reasons largely concerning the nature of the muscular fibre, irritability, the peristaltic motion of the intestinal canal, digestion, &c. &c.

II. Pharmacopee Royale, Galenique et Chymique, par Moyse Charas, Apoticaire Artiste du Roy en son Jardin Royal des Plantes. A Paris, 1676, 4to.

This pharmacopæia was once in great repute, but is now quite obsolete.

Observations on some considerable Parts of Asia. By M. Tavernier. N° 129, p. 711.

Ispahan is about the size of Paris; but Paris has ten times more people than Ispahan. The air of Gomron, a seaport town in Persia, from April to November, is so unhealthy, that it breeds a very malignant fever, which, if it kill not, leaves behind the jaundice for the rest of the patient's life. After the end of March the wind changes, blowing mostly from the west or south-west, and is sometimes so hot and suffocating, that it stops respiration: whence the Arabians call it El-Samiel, that is, a wind of poison. And what seems very strange, if one lay hold on an arm or a leg, or any other part of a body, that has been newly stifled by this wind, it remains in the hand like grease, and as if the body had been dead a month before. There is the same kind of air about Mousset and Bagdat; concerning which he relates, that travelling once on the road from Ispahan to Bagdat, he would have been stifled, if he had not been in the company of some Arabian merchants, who, as soon as they perceived the wind coming, made him light from his beast, and throw himself as they did, flat upon the ground on his belly, covering themselves well with their cloaks. In which posture having remained for half an hour, and with much ado saved themselves from being suffocated, they rose up, finding their horses wet all over, and so faint that they were not able to carry their riders. But it is observed, that upon any river, the same wind does no harm. Sometimes the blast is so hot, that it burns as if lightning had passed.

In Persia few children have the small-pox, but, instead thereof, most of them are troubled with the scurf on the head, till they are 10 or 12 years old.—The Persians know nothing of the gout or stone; only the Armenians, who drink more wine than water, are afflicted with the latter of those two diseases.—The Persians, especially the better sort of them, are far less subject to sickness than the Europeans, because they fail not in spring to take inwardly a decoction of the wood of China, which they let boil for several days in water, according to the dose prescribed by the physician. E. g. the first day they put one ounce of it into three pints of water, increasing the dose of the root every day to the 12th, and thence to the 20th day. This drink is said to be very agreeable to the taste, and of the colour of our pale wines. While they are drinking this decoction, they must eat nothing but a little bread and a roasted chicken, without salt; and after they have done drinking, they must forbear eating fruit a whole month. When this drink is taken, the patient must be very well covered to sweat; which is copious, and all his linen becomes yellow, and even the walls of his chamber. This root soon spoils, and whilst it is good the author says a pound of it costs 100 crowns.

The women of the Turkish seraglio are always chewing mastic, to keep their teeth clean and white.—When the Nogaies, a sort of Tartars, have received any wound, they use no other ointment than boiled flesh, applied hot to the wound. And when the wound is deep, they thrust in a piece of fat as hot as the patient can bear it: and for this purpose they count the flesh and fat of horses best of all.—Those that are troubled with the colic, are ordered to eat horse-flesh; which they say cures many.—Near the isle of Baharen, they fetch fresh water from the bottom of the sea; and about Cape Comorin, and along the coast of Coromandel and Malabar, where there is no sweet water, the people come with their vessels at the time of low water as near to the sea as they can, and digging about two feet in the sand, they meet with fresh water fit for drinking.

Camels go with young 11 months, and can be without drink many days, even to nine; and the larger sort of them are able to carry a 1000 or 1500 pound weight. Their milk is a sovereign remedy against the dropsy.—The cows about Balsara, having no grass to feed on, are fed with the heads of fishes and dates boiled together.—The palm trees in the country of Balsara are thus propagated. They dig a hole in the earth, in which they range 250 or 300 date-kernels, one a-top of another pyramid wise, with the point upwards, so that the pyramid ends in one kernel: which being covered with earth, the tree grows up.—Craw fish creep up the white mulberry trees about sun-set, eating the fruit; and at break of day come down again into the rivers, near which those trees grow.—Porcupines kill lions, by darting their quills into their body.*—All along the Gulph of Persia there are vast numbers of a kind of locusts, which are eatable, and of which our traveller affirms that he opened one that was six inches long, and found 17 little ones in its belly, all of them moving.†

There is a talc in Persia, which being beaten into pieces, as small as lentils, and tinged with any colour, they mix it with chalk well slaked, and rubbing their walls with it, make them shine like jasper, which is very agreeable to the eye.—On the west of the Caspian Sea, a little above Chimaki, there is a rock whence drops an oil, of which the Persians make a varnish, by infusing in it some drops of mastic. This oil whilst issuing out of the rock, is as clear as water; but afterwards thickens gradually.—The best glue in the world is made of sturgeon; it being so strong that you may sooner break the matter thus glued any where else than in the place where it is glued. The method of the Turks in preparing it is this: when they have gutted the fish, they meet with a certain skin that covers the flesh;‡ and this they pull away from about the head to the end of the belly. This skin is very glutinous, and of the thickness of

* This is a vulgar error.

† Might not these "17 little ones" have been the maggots of some other genus, or worms; since locusts are oviparous?

‡ M. Tavernier is not accurate in matters which relate to natural history; which, indeed, was not to be expected from a merchant. This "certain skin" is the air-bladder of the sturgeon.

two paper leaves: this they roll up to the thickness of a man's arm, and so put it to dry in the sun: and when they would use it, they beat it with a hammer, and being well beaten they break it into little bits, which they put and keep in water for about half an hour in a little pot, and so set it over a gentle fire, stirring it continually till it becomes liquid, and taking heed of keeping it from boiling, which would utterly spoil it.

The Persians are exquisitely skilful in damaskeening with vitriol; but the nature of the steel used by them contributes very much to the good workmanship. This steel they fetch from Golconda, which is the only kind known that can be well damaskeened. When it is put to the fire to temper it, they very carefully give it only a little redness, like that of a cherry-colour; and instead of quenching it in water as we do, they only wrap it in a wet piece of linen cloth; for if they should give it the same degree of heat that we do to ours, it would become as brittle as glass.

Description of a New Hygroscope. By Mr. John Coniers, Apothecary and Citizen. In a Letter to the Editor, Oct. 23, 1676. N° 129, p. 715.

Among many trials made by me for the readiest and best discovery of the change or temperature of the air and weather, I have found out that by applying a hand and a circular index, or a quarter-circle, to a pannel made of duly seasoned deal wood, and that divided or slit in two parts, playing loose in a groove, and only fastened to the frame at each end, you have one of the best, if not the very best contrivance, for that purpose. I have made two several contrivances of it; the one I invented about five or six years since; here explained in the first figure, with some observations made at that time; the other some years after the former: both which I thought fit to communicate to you, to dispose of them as you shall think good.

So far the letter: which, with the invention and contrivance itself, the editor would have given notice of before this, and at the time when in N° 127 of these tracts the like invention, imparted from Dublin, was described, if he had not then been altogether unacquainted therewith. Wherefore, to do right to the ingenuity of this inventor, the description of this his instrument, in its two several contrivances, shall now be faithfully set down here, with the observations made by the former of them.

The Explanation of the first Contrivance, in Figure 4, *Pl.* 11.

AAAA is the frame of wood, for the two pannels of deal to play loose in, at top and bottom, to which at the two ends they are fastened.—BB, the two pannels of slit deal, three feet deep, and three feet broad a-piece, with a distance left in the middle for the scope of the motion.—C, the hand fastened by the axletree to the plate, and also with nail-holes which are to fasten it to the middle of the pannel, within half an inch of the scope for motion; at the lower or shorter end of the axletree there is fastened, by a wire like an S, a small silver chain within a straw's breadth of the axletree; which chain is to be carried and placed across the distance between the two pannels, and fastened to the opposite pannel by a brass noose, through which it is to slip, so as to be taken up or let down at pleasure.—D, the roller, with a weight annexed, which by a string is fastened to the lower end of the hand C; so that, as it relaxes and gives way, the weight will adjust the motion of the hand to the index E. This is an index of paper, pasted on the pannel opposite to the hand, and so, as in this figure, placed near the top, for the better convenience of the hand's motion; and this index being but a quarter of a circle, is divided into inches, more or fewer according to the scope which the hand of the pannels requires for their motion; but when the relax shall require more room for the hand, then the chain is to be taken up one link more, and thus there will be more play upwards and downwards: which taking up of the chain may yet be again repeated, when there is occasion, or the time of the year requires it.

Now if the chain be placed near the axis, the motion will be the nicer and larger; if farther off then it will be less: For instance, the motion of 2 more than that of 3, and 3 than that of 4, &c. as may be seen by the figures 2, 3, 4, 5, 6; which are placed in this figure below the hand.

These pannels of deal board move by shrinking most in summer, and swelling most in winter seasons; but will vary from this, according to the greater degree of heat or cold, moisture or drought.—For the most part, especially in spring and summer, this motion happens only in the day time; for then generally all night it rests and moves very seldom.—That one kind or manner of this motion happens in dry fair weather; but sometimes in the forepart of the forenoon, and sometimes not until the latter part of the forenoon, and then it relaxes or swells the deal for about two or three hours, more or less; and then all the afternoon it shrinks, sometimes even when a small rain has newly fallen or is then falling; but this not so often, and more seldom in winter, or cold moist weather.—This shrinking is very often gradual, or for the most part a little after a moist time, viz. the first day after moisture it shrinks a little, the second day more, and so still more according to the time of year, and as it is then inclined to moisture or drought, heat or cold.—The winds being in the north, north-east and east, winter and summer, for the most part at that time the deal shrinks in the night as well as in the day; but not so much: which is a sign of drying weather, and sometimes of frost or cold in winter; heat or scorching in summer in a clear day. But on the contrary, the south winds blowing, or the west and south-west, the deal then always relaxes, or at least is at a stand, provided this happen in the day time; for if in the night not so much; and so this will do some considerable time before rain.—By a constant observation of this experiment of the deal's motion and rest, you may be able to guess at the situation of the wind, without a weather-cock, provided you have a common and a sealed thermometer.—Also you may know the time of the year; for in the spring it moves quicker and more, than in winter; in summer it shrinks more than in the spring; in autumn it is less in motion than in summer.

The Explanation of the Second Contrivance by a Circular Motion for an Annual Revolution; and first in the Outward Parts; represented in Fig. 5, *Pl.* 11.

AAAA is the frame of wood for the pannels of deal to play loose in, at top and bottom.—BBBB, the crosses of deal or iron fastened to the frame on each side; to which is annexed the circular index, divided into 12 parts; in the centre of which the axis b for the hands is placed.—CC, the two pannels of slit deal, each 3 feet deep, and 3 feet broad; fastened at each end of the frame, with a distance left in the middle for the scope of the motion.

The Explanation of the Inward Work in Fig. 6.

A, A, the two hands.—B, b, the two brass pulleys or rollers, the one larger, the other less; to the larger a flat leaden weight is fastened with a cat-gut string; to the smaller is fastened a small silver chain, which is by the noose or loop of the brass C, to be fastened to the pannel under the middle of the cross near the gap or scope for the motion; and in the noose the chain to have a fastening, to be taken up or let down at pleasure.—D, the roller or pulley, to be placed on the other pannel opposite to the noose, and near the gap or scope between the two pannels; over which roller the small chain on its return to the axis is to be placed.—E, the axis on which the two rollers or pulleys B, b, are to be fastened, and the two hands A, A, for the index.—F, the weight annexed to the larger roller or pulley B, and the string or catgut to be moved, is to have the contrary posture for motion to the small roller or pulley, on which the silver chain is fastened: so that, as the shrinking of the pannel moves the axis one way, the relaxing may give way to the moving the hands or axis the other way by the power of the weights.

The circumference of the smaller pulley or roller Bb, on which the chain is fastened, is to be no larger than just so much scope or distance as the two pannels make by the extremity of their utmost swelling or shrinking; and so one full revolution of the hand on the index may answer the greatest shrinking and swelling in the year, and the distance between the two rollers or pulleys fixed on the axis must be the thickness of the pannels; so that the weight is to play or move on the one side of the pannel, and the chain on the other, without disturbance or rubbing against the sides of the pannel or the cross, between which, in the middle they are to be placed out of sight.

The deal board should be of the finest and straightest grained dram deal laid to dry two or three years. And to know whether it be sufficiently seasoned, take a small part of it, and weigh it in a nice pair of scales, and if you find the weight not increased many grains in wet weather, nor decreased in dry, you may then conclude it fit for your purpose.

An Occultation of Mars by the Moon, Sept. 1, *N. S.* 1676, *observed at Dantzic, by M. Hevelius.* N° 129, p. 721.

Time by Pend. Clock.	Names of fixed Stars.	Altitudes.	Time Correct. by Altit.	Remarks.
h. m. s.			h. m. s.	
1 1 25	Swan's tail	57° 10′	1 0 24	Mars nearly as far distant from the Moon's bright limb, as Mount Porphyrites was from Mount Ætna.
1 9 45	·	·	1 8 45	
1 36 39	·	·	1 35 42	Mars covered by the Moon.
1 45 25	Swan's tail	51 17	1 44 7	
2 47 54	·	·	2 46 29	Mars quite emerged, or end of Occultation.
3 19 50	Scheat in Peg.	45 3	3 18 19	

Mars was first covered by the moon about Mount Audus, thence proceeding as it were through the Loca Paludosa of the moon, by Mount Ætna, below the island Besbica, above the Palus Acherusia, above Mount Corax, by the Palus Mæotis, and a little above the isle Alopecia, and the very centre of the moon; and thence emerging at the great western lake. If it be asked how I could thus exactly trace out the planet's passage behind the moon, and even that of her obscure part; it proceeded from hence, that by those tubes of mine I could very distinctly perceive the principal of the larger spots in the shaded part of the moon; so that I could observe very plainly, that Mars emerged near the middle of Palus Mæotis.

The same Occultation observed at Greenwich, by Mr. Flamsteed; being Aug. 21, *O. S. before noon.* N° 129, p. 723.

By several altitudes of the sun's lower limb, taken before the occultation, it was found that the error of the clock was 4m. 55s. too slow. Then, afternoon, the sky being very clear, the following observations were taken:

Time by the Clock. h. m. s.	Time Correct h. m. s.	Distances, &c.		
10 45 3—	10 49 58..	Mars from the moon's bright limb..	5125=	42′ 8″
11 35 57—	11 40 52..	The same distance..............	1982=	16 18
11 57 31—	12 2 26..	♂ from zen. or dif. alt. inf. limb and ♂..	1912=	7 35
12 5 0—	12 9 55..	Planet no longer vis. to naked eye..		
12 9 44—	12 14 39..	Light of ♂ confounded with the moon's light. ♂ from zenit....	1158=	5 47
12 10 3—	12 14 58..	♂ quite covered by northern cusp..	3475=	17 20
12 24 58—	12 29 53..	41 in ♉ from the limb or cusp......	3935=	32 21
12 46 0—	12 50 55..	Diameter of the moon............	5971=	29 47
13 10 56—	13 15 51..	♂ emerg. perhaps 4″ or 5″ sooner		
13 13 29—	13 18 24..	♂ from the northern cusp........	3675=	18 20
13 22 0—	13 26 55..	☾'s alt. 23°. Her diameter......	5988=	29 55

The same Occultation observed at Oxford, Aug. 21, 1676, *P. M. by Mr. Halley.* N° 129, p. 724.

Correct time. h. m. s.	Distances, &c.		
11 43 30..	Centre of ♂ from the ☾'s nearest limb..	719½	= 12′ 40″
11 54 48..	The same..............................	409	= 7 12
12 3 25..	Centre of ♂ from the ☾'s north cusp......	1118	= 19 41
12 10 28..	The gibbous part of ♂ touched the ☾'s limb		
12 10 42..	♂ wholly covered; distance from the cusp..	963	= 17 14
13 10 41..	Centre of Mars emerged		
13 12 45..	♂ dist. from ☾'s northern horn...........	1018	= 17 55
13 52 35..	☾'s alt. 31°. Her diameter observed......	1698	= 30 1
13 57 52..	♂ from the ☾'s north horn............	2042	= 36 5
14 2 53..	♂ from the ☾'s south horn............	2266	= 40 3

Having carefully considered the moon's parallaxes, in the observations of this occultation at Dantzic and Greenwich, I find from the immersion the difference of meridians between Greenwich and Oxford 4 m. 57 s.; between Greenwich and Dantzic, 1 h. 14 m. 50 s. Also by the emersion the first of those differences is found 4 m. 59 s.; and the latter 1 h. 14 m. 41 s. Which near agreement shows the exactness of all the observations.

Extract of two Letters written by Mr. John Beaumont, jun. of Stoney Easton, in Somersetshire, concerning Rock-plants and their Growth, or Trochitæ and Entrochi. N° 129, p. 724.

Mr. Lister, N° 79 of the Trans. judges that shells found in stone quarries were never any part of an animal; and gives this probable reason for it, because quarries of different stone yield us quite different species of shells, not only one from another, but from any thing in nature besides, which either the land, salt, or fresh water yields, and though some seem of the same species, and much like each other, yet there is difference enough to hinder them from being sampled by any. I myself observed the same thing some years since, when I endeavoured to satisfy myself of the process of nature in this way; and have now by me several species of stones resembling shell-fish, which I gathered from ploughed fields and quarries, that are scarcely to be paralleled, as I judge, by all the collections of sea-shells extant.

To examine this opinion of petrifaction further; perhaps it might seem rash to deny a petrifaction of animals and vegetables, so many instances being alleged on all hands by judicious persons attesting it; though I cannot say, that my own observations have ever yet presented me with an ocular evidence of the thing: I only find, that the thing supposed to be petrified, becomes first crusted over with a stony concretion, and afterwards, as that rots away inwardly, the lapidescent juice insinuates itself by degrees into its room, and makes at last a firm stone, resembling the thing in shape; which may lead some to believe it really petrified. But though a real petrifaction were allowed in some cases, it would not be rational to plead this in all the figured stones we see, on account of the many grounds we have for the contrary. But I take these to be the chief reasons which make some so ready to embrace so generally this conceit of petrifaction, because they are prepossessed with an opinion against the vegetation of all stones, and for that they think it impossible for nature to express the shapes of plants and animals where the vegetative life is wanting, this being a faculty peculiarly belonging to that soul, whereas they seem to err in both: for, as what has been said concerning our stone-plant may suffice to prove their vegetation; so it will be as easy to show that nature can and does work the shapes of plants and animals without the help of a vegetative soul, at least, as it is shut up in common seeds and organs. To be satisfied of this, let them view the figurations in snow; let them view those delicate landscapes, which are very frequently found depicted on stones, carrying the resemblance of whole groves of trees, mountains, and valleys, &c.; let them descend into coal mines, where generally with us the clifts near the coal are all wrought with curious representations of several sorts of herbs, some exactly resembling fern branches, and therefore by our miners called the fern-branch clift; some resembling the leaves of sorrel, and several strange herbs, which perhaps the known vegetable kingdom cannot parallel; and though it could, here can be no colour for a petrifaction, it being only a superficial delineation. The like may be said of animals, which are often found depicted on stones; as all mineral histories will sufficiently inform them. Now since here is no place for petrifaction, or a vegetative soul, we can only say, that here is that seminal root, though hindered by the unaptness of the place to proceed to give these things a principle of life in themselves, which in the first generation of things made all plants, and I may say animals, rise up in their distinct species; God commanding the earth and waters to produce both, as some plants and animals rise up still in certain places without any common seed.

It seems to be a thing of a very difficult search, to find what this seminal root is, which is the efficient cause of these figures. Many of the ancients thought it to be some outward mover which wrought the figures in things for some end; the Peripatetics rather judged it to be some virtue implanted in the seed, and in substances having an analogous nature with the seed, &c. &c.

Thus, I have given you something of what I conceive, and practically know concerningthe vegetation of rock-plants, endeavouring also to render some account of those various figures which are found amongst minerals. Ishall conclude with a requestto you concerning athing, which mayprove verymuch to the advantage of thosewho are concerned in mineral adventures: it is a constant opinion among our miners, that lead ore discovers itself by an oily smell, and that chiefly in a morning, a little before the rising of the sun, especially when some showers have fallen in the night: this being so, I find two things in the Transactions, which give me hopes that this way of discovery may be much improved by art: the first is an intimation of a way shown by Sir William Petty, in his tract of double proportions, whereby we may discover a smell at a great distance, and so consequently the intenseness and remissness of it near at hand, wherein the chief difficulty will consist; for, where these smells rise, they commonly diffuse themselves to a furlong's circumference, or more; so that we are more at a loss to find exactly the place whence they rise, than to make a first discovery of them. The second thing is the statical baroscope of Mr. Boyle, which I conceive may give us some light of their true source, there being probably at that place a considerable variation in the pressure of the atmosphere by reason of the mineral-steams which are there in the greatest abundance. I am not ignorant that some strongly fermented beds of mineral earths and rusts, which are sometimes barren, send forth a ranker smell than ore itself, which may now and then deceive us; but because for the most part these are concomitants of ore, we may not look upon the attempt as fruitless.

An Account of some Books. N° 129, p. 742.

I. Ephemeridum Medico-physicarum Germanicarum Annus IV. et V, Anni 1673 et 1674, &c. cum Appendice: Franc. et Lip. 1676, 4to.

A continuation of the German Ephemerides, noticed in the 1st Vol. of this Abridgement.

II. Nouvelle Methode en Geometrie pour les Sections des Superficies Coniques et Cylindriques, qui ont pour Base des Cercles, ou des Paraboles, des Ellipses, et des Hyperboles; par Ph. de la Hire*, Parisien. A. Par. 1673, in 4to.

This author, in his first proposition, demonstrates all the proportions of the lines, which coming from one point, or being parallel among themselves, and meeting the sections, are cut by these sections, or by the lines that join the

* Philip de la Hire was born at Paris, 1640, and having learned drawing, perspective and gnomonics, to prepare him for his father's profession, that of a painter, he was sent to Italy to improve and perfect himself in that art. But his turn being only to mathematics, he devoted himself entirely to that science, in which he made great progress; at the same time that he drew well, and was a good landscape painter. On his return to France, he was sent along with M. Picard, into the northern provinces, to take the measurements of the country, in order to prepare a general map of the kingdom, more accurate than had before been drawn, as well as for an extension of the meridian before measured and begun by Picard. He became a member of the academy of sciences in 1678; and he had the honour of being professor of architecture and of mathematics to the king. He died in 1718, at 78 years of age. The principal of his other numerous and excellent works were, 1. New Elements of Conic Sections, 12mo. 2. A large Treatise on Conic Sections, in folio, 1685, enlarged from the book above described. 3. Astronomical Tables, 1702, in 4to. 4. School of Land Surveyors, 1692, in 12mo. 5. Treatise on Mechanics, 1695, in 12mo. 6. Treatise on Gnomonics, or Dialling; besides several papers printed in the memoirs of the Academy of Sciences.

M. de la Hire was a mathematician of the old school, who could never be induced to employ or countenance some of the more modern improvements in analysis, particularly the method of fluxions or differentials. He was nevertheless a sound mathematician, and well skilled in all the other branches of the science, possessing a true taste for the ancient method of geometry, of which his large book on conic sections is a striking instance, at a time when most of his countrymen had abandoned it. In other respects too, it would seem, that la Hire had his peculiarites: it is said that he never passed a windmill without pulling off his hat, in honour of the inventor.

His son, Gabriel Philip, who survived him only one year, practised physic with reputation, and was also a member of the academy of sciences. He painted for his amusement, as well as studied astronomy and the mathematical sciences; and for several years he undertook the care of calculating the ephemerides, called the Connoissance des Temps.

contacts, or by other tangents: which he says comprehends a great part of the propositions of Apollonius; and many others also of which he has not spoken: which seems to him very easy to understand, forasmuch as it is nothing else but a continual repetition of the application of one only line cut in three parts, which line he calls cut harmonically; not that the parts taken separately are in harmonical proportion, but that, by taking one of the extremes for one, and the same with that of the middle for another, and the whole for the last, these three lines should be in harmonical proportion.

After he had dispatched this proposition, he says that he was resolved to have concluded his book with the properties and relations of the ordinates to the rectangles of the parts of their diameters; but that he found himself insensibly engaged to add to it some other propositions of a more useful kind, and which might easily be demonstrated by the first; and then, the propositions of the ancients about the foci or puncta comparationis; and the demonstrations by him given of them he affirms to be different from those of others, that so this work of his might not only be entire, but new.

He has also given a method of demonstrating the sections of the conic surfaces that have for base parabolas, ellipses and hyperbolas; as also those of cylindrical surfaces, which have for base the same curves as well as the circle.

III. Ophthalmographia, sive, Oculi ejusque partium Descriptio Anatomica. Auth. Guil. Briggs, A. M. et Coll. Corp. Christi in Acad. Cantabr. Socio. Cantab. 1676, in 12mo.

A good description of the eye, accompanied with observations on vision.

A Declaration of the Council of the Royal Society, passed Nov. 20, 1676; relating to some passages in a late book of Mr. Hooke, entitled Lampas, &c.

Whereas the Publisher of the Philosophical Transactions, has made complaint to the Council of the Royal Society, of some passages in a late book of Mr. Hooke, entitled Lampas, &c. and printed by the Printer of the said Society, reflecting on the integrity and faithfulness of the said Publisher in his management of the intelligence of the said society: this council has thought fit to declare in the behalf of the publisher aforesaid, that they knew nothing of the publication of the said book; and further, that the said publisher has carried himself faithfully and honestly in the management of the intelligence of the Royal Society, and given no just cause of such reflections.

The Council having thus justified the publisher, he shall only add that part of a letter, written to him by M. Huygens, the 20th of Feb. 1675, which relates to the taking out a patent of his, the said M. Huygens's invention; and then let the world judge of the postscriber's accusation, about an endeavour to defraud him of his contrivance. The words of the said letter, Englished, are these:

For the rest, Sir, if you believe that a privilege, (so he calls a patent,) in England would be worth something, and that either the Royal Society or you might make some advantage thereof, I willingly offer you all I there might pretend to.

So that, if there was a desire in the publisher to take out a patent, it was for no other contrivance, but M. Huygens's, formerly sent to the Royal Society, and printed in N° 112 of these Transactions.

More Observations of Mons. Tavernier's Voyages; continued from the last Tract. N° 130, p. 751.

The second volume of these voyages treats of East-India, and the neighbouring islands, in three books: the first is of the roads from Ispahan to Agra, and from Agra to Dehli, and Gehanabat, where the court of the Great Mogul is at present; as also to the court of the king of Golconda, and to that of the king of Visapour, and to divers other places of India. The second is a historical and political description of the empire of the Great Mogul. The third, a narrative of the religion of the Mahometans in those parts, and of that of the Indian idolaters; with an account of the author's voyage by sea from Surat to Batavia in Java Major, and from thence into Holland; interspersing many particulars of divers kingdoms of the East.

Among the many observables contained in this volume we take notice, that there are to be met with, admirable jugglers and mountebanks in the road from Surat to Agra by the way of Amadavat; by some of whom the author affirms he saw done what follows: they kindled a great fire, and in it heated some iron chains red hot, which they laid about their bodies, without receiving any apparent hurt. They took also a little piece of wood, and having fixed it in the earth, they demanded of the spectators what kind of fruit they would have grow upon it; it being answered, mangos; one of the jugglers covered himself with a linen cloth, and stooped down to the ground five or six times. At which time one of the spectators having placed himself so that he could observe what the juggler did, saw, that with a razor he cut his flesh under his arm-pits, and with the blood thence issuing, rubbed the piece of wood. Whereupon every time that he raised himself from stooping, the planted stick of wood was visibly grown; and at his third rising, branches came forth with buds; at the fourth, the tree was covered with leaves: and at the fifth, blossoms were seen upon it.

That the author affirms to have given us an exact list of all the merchantable commodities, furnished by the empire of the Great Mogul, and the two kingdoms of Golconda and Visapour, and other neighbouring states; and of all that nature and art afford there: viz. silks; various cloths, white and painted; cottons, spun and unspun; indigo, saltpetre, spices, cardamom, ginger, and pepper; diamonds, rubies, pearls, bezoar, musk, sugar; besides some drugs, that indeed are found at Surat, but are brought thither for sale from other countries, as sal ammoniac, borax, gum-lac, saffron, cummin, myrrh, frankincense, opium, lignum aloës, liquorice, cassia, coffee. To all which he has annexed an account of the cheats used in divers of these commodities, especially in the silks, cloths, cottons, indigo.

That it is certain that the nut-meg tree is not planted, but the fruit of it sown by birds, said to swallow the nutmegs whole, and voiding them whole without digestion, covered with a viscous matter: whereupon they take root and grow up to a tree. Again, that the birds of Paradise eating this fruit, are intoxicated with it, and fall down dead on the place; whereupon emmets come and eat off their legs, and other parts.

Of pearls he has this remarkable observation, viz. that he had one pearl-oyster in his hand, that had ten pearls in it, though of different sizes; being, in his opinion, bred in oysters, as eggs are in the belly of fowls.

That musk, when it is first drawn out of a certain bag of the musk-deer, is like blood coagulated; that most of it comes out of the kingdom of Boutan, between 56 and 60 degrees northern latitude; but that Cochin-China also and Tunquin furnish some quantity. Of the people of this kingdom he relates, that they have had the use of muskets, cannon and powder for several ages; they report that they now have pieces of cannon, on which are found cyphers, or letters, demonstrating them to be above 500 years old. This is that very kingdom, (says our author,) through which the ambassadors of Muscovy passed, anno 1659, into China, taking their road all along the Great Tartary on the north of Boutan: which ambassadors, if they had complied with the customs and ceremonies of China, we might probably have at this day a beaten road by land from Muscovy to China, by the north of Tartaria Magna, and much more knowledge of the kingdom of Boutan; and of some other countries, of which we hardly know the names: a thing that might have proved a great advantage to all Europe.

That Bezoar is found among the contents within the stomach of certain goats, that feed on a plant, the name of which the author says he has forgotten. This plant is said to thrust out certain buttons, about which and the extremities of the branches, eaten by these goats, the bezoar is formed in their belly. It is added, that the bezoar takes its form according to that of the buttons and the ends of the branches. The goat that breeds these stones, is, by his description, a very fine and tall creature, having hair as fine as silk.*

* The Bezoar is a calculus concretion formed in one of the stomachs, (for they have several stomachs,) of certain species of antelope.

Of the Lake of Mexico; and a strange Sort of Rye, growing in some Parts of France. N° 130, p. 758.

The lake of Mexico has this of extraordinary and perhaps peculiar to itself, that part of its water is fresh, and the other part salt; whence it is believed to be derived from two sources, the one of sweet water, and the other from some mineral and saline-earth, found in the hills through which this water passes, being impregnated with the salt which is dissolved in its course: or if it have no peculiar source, the bottom or the earth under the water must be full of salt: which is confirmed by experience, much salt being made of it every day, of which that city drives a great trade with remote parts, even the Philippines themselves, whither it is transported in considerable quantities. That part of the lake which is sweet is still and quiet; the salt part is agitated and moved according as the winds blow. The sweet water is very good and wholesome, breeding plenty of little fish. That which is agitated is bitter salt, breeding no fish at all. The sweet water is higher than the other, and falls into it. The water of the salt part is seven leagues long, and as many broad, and above 22 in compass. That of the fresh water is near as large, and the whole lake about 50 leagues in compass.

M. Perrault related to the Royal Academy of Paris, that travelling through Sologne, he had been informed by some physicians and surgeons of that country, that the rye was there sometimes so corrupted, that those who did eat of the bread which had much of this corrupted grain in it, were seized with a gangrene; some in one part, some in another, some losing a finger, others a hand, others a nose, &c. and that this gangrene was not preceded by any fever, nor inflammation, nor considerable pain; as also that the gangrened parts fell off of themselves, without any need of separating them by any remedies or instruments. That these grains of rye are black without, and pretty white within, and when they are dry, they are harder and closer than the natural good grain. They have no ill taste. Some of them had hanging at their basis a substance of a honey-taste and consistence. They become much longer in the ear than the other, some of them being 13 or 14 lines long, and 2 lines broad, and at times seven or eight of them are found in one ear.

Rye in this manner degenerates in Sologne, Berry, the country of Blaise and Gastinois, and almost every where, especially in light and sandy land. It grows plenteously in wet years, and most of all when after a rainy spring there follow excessive heats. The bread made of the rye which contains some of this corrupted grain, tastes like other bread. The rye thus corrupted produces its effects chiefly when new, yet not till it has been used a considerable time. These effects are, to dry up the milk in women; to cause sometimes malignant fevers, accompanied with drowsiness and raving; to breed the gangrene in the arms, but mostly in the legs, which it first seizes, as the scurvy does.

It is preceded by a certain numbness in the legs, on which follows a little pain and some swelling, without inflammation, and the skin becomes cold and livid. The gangrene begins in the centre of the part, and appears not at the skin till a long while after.

The only remedy for this gangrene is to cut off the part affected. If it be not cut off, it becomes dry and lean, as if the skin were glued over the bones, and it is of a frightful blackness, without rottenness.

While the legs are drying up, the gangrene ascends to the shoulders. There is yet no specific remedy against this evil. Though there is some hope of preventing it by hot spirits and volatile salts. The orvietan and ptisan of lupins do considerable service. Poor people only are generally subject to this distemper.

M. Tuillier asserts, that in the year 1630, which was fatal to the poor of the countries subject to these evils, he being at Sully, and having understood by a physician and surgeon, that the cornuted rye was the cause of the gangrenes that were then very frequent, being desirous to satisfy himself, whether this grain was indeed the cause thereof, he gave of it to several animals, and they died of it.*

Observations on a Subterranean Fire in a Coal-mine near Newcastle. By Dr. Lucas Hodgson. May 15, 1676. N° 130, p. 762.

This subterraneous fire bears no analogy to other volcanos; it increases or decreases according to the subject it feeds on, which is for the most part a day-coal, as they call it, that is, the upper seam of the coal, next exposed to the air, so that you may light a candle at it in some places, in others it is some fathoms deep, according as the day-coal heightens or deepens. There is no sal ammoniac, nor any thing like it to be found, except at the fire. There being such a mixture of the steams of sal ammoniac and sulphur rising together in most places, it is hard to distinguish them; for though the flowers of brimstone seem to rise first, yet there is commonly a crust of sal ammoniac under them. The milky substance is only found where the sal ammoniac and sulphur are totally gone, and the acid part, or aluminous spirit of that white mass, will also fly off

* *Ergot* is the name given by the French both to the rye itself thus vitiated, and to the effects which it is supposed to produce in those who eat bread made of it. In the lvth vol. of the Transactions, there is a further account of this disorder by Mons. Tissot. It is denominated by Sauvages necrosis ustilaginea. The disease termed raphania is thought to originate from the same cause.

by the increase of the fire, leaving a caput mortuum, dry, styptick, and as hard as a stone; yet a pound of this mass, before the fire press too much upon it, will nearly afford by solution, &c. half a pound of tolerable crystalline allum. The neighbouring soil differs little from other grounds with us, having neither common salt nor nitre in it. I have industriously observed the springs that are near the fire, and find none of them that give the least suspicion of sal ammoniac. The water that runs from the adjacent collieries is vitriolic, giving as deep a tincture with galls as Scarborough spa, and differs in nothing from the ordinary waters of collieries. The other springs are of ordinary use, containing no mineral salts in them.—But I hope you will cease to wonder, that coal should produce a volatile salt by the action of fire, seeing I have gathered sal ammoniac from a burning brick-kiln, where nothing but clay and coal is burnt together, and I hope none will expect the volatile salt in the sal ammoniac from common clay. The reason that first prompted me to seek this salt there, was, that the smell of the kiln did somewhat resemble that of the subterranean fire. There is also a sort of mineral called slate, which is partly coal, partly alum-stone, partly marcasite, which being laid up in heaps and burnt, is used for hardening the coal-ways; on these heaps, whilst burning, I have often gathered both brimstone and sal ammoniac.

As for the experiment of pouring cold water on the powdered marcasite, the event was, that it produced a vitriolic water, but no heat.

Though it may seem incredible to some, that black coal should yield so white a volatile salt, yet they that know that all volatile salts whatever may be freed from their fœtor and intense colour, by transmuting them into a sal ammoniac by the mediation of an acid, as spirit of salt, spirit of vitriol, allum, &c. and then subliming them till they be white, will cease to doubt at this matter. The reason of which change, I presume, is, because, though these volatile salts carry over always some of the fœtid oil with them while in a state of volatility, yet being thus in a manner fixed, the fœtid oil must necessarily by force of fire rise first, leaving the subsequent compound salt or sal ammoniac without smell.

As to your inquiry concerning petrescent springs we have none near us. There is indeed a cave some miles off, from the roof of which hang large lumps of petrified water, like icicles, some of them reaching down to the ground like pillars; these icicles are good limestone, as I have tried. I shall conclude when I have acquainted you with a spirit of sugar, of which a distiller with us had a quantity; it seems to be the result of some anomalous fermentation, it is so strong that no man is able to smell at it in an open vessel without being made almost breathless; neither do I think the person that made it can make it again.

The spirit of sugar, here mentioned, was drawn from bare sugar-water, which is nothing but the water wherewith the molds, aprons, &c. are washed, fermented with the scum. And it was so exceedingly volatile, that it would not be carried, but lost all its force in the carriage, though it was very well stopped.

An Account of some Books. N° 130, p. 766.

I. Roberti Boyle, Nobilissimi Angli et Soc. Regiæ dignissimi Socii, Opera Varia. Gen. in 4to. 1677.

The works of this noble author having been already noticed in these Transactions, at the several times when they came abroad singly, the editor on looking over this Latin edition, shall only inform the reader; 1. That this edition has been made without the consent and knowledge of the author. 2. That the year in the frontispiece thereof is one and the same, as if the several books contained in this Latin volume had been published in one year; and that the enumeration of the several treatises, made in the catalogue of this Latin edition, is not according to the time wherein they were first printed.

II. An Account of several Travels through a great Part of Germany in four Journeys, &c. By Edw. Brown, M. D. Fellow of the College of Physic of London, and of the Royal Society. Lond. 1677, in 4to.

This learned and curious author, having given a relation of some remoter and less frequented countries of Europe in the year 1673, in this piece gives an account of Vienna; describing also his journey unto that place from England, by the Belgic Provinces and Germany; as also his return from Vienna by Austria, Trans-Danubiana, Moravia, Bohemia, Misnia, Saxonia, unto Hamburg; therein giving chiefly an account of the natural, artificial, and topographical, observables; with some of the customs and occurrences, which might be acceptable to the inquisitive reader, or serve as hints of further inquiry to such persons as may hereafter travel into those parts.

III. Caspari Bartholini, Thomæ filii, Diaphragmatis structura nova, unà cum Methodo præparandi Viscera, &c. Par. 1676, in 8vo.

A treatise on the structure of the Diaphragm, written when the author was scarcely 22 years old. Subjoined is an account of his method of injecting the viscera. The plates, as Haller has remarked, are very indifferent. This author was son of Thomas Bartholin, of whom some biographical memoirs have been given in the first vol. of our Abridgement. Like his father he arrived at great honours, being appointed a counsellor of state, &c.

IV. Longitude found by Henry Bond,* Senior, Teacher of the Mathematics. London, 1676, in 4to.

The attempt and pains of the author of this book are certainly very commendable, as he endeavours to explain the use of the inclinatory needle, and in so doing makes it known to the world that, as both the variation and inclination of the needle were found out first of all in this nation by two Englishmen, Mr. Robert Norman and Mr. William Burrows; he has now made it his business to apply it to a use, formerly, for ought we know, not thought of, viz. to find the longitude.

Meantime, the editor is desired here to take notice of a mistake committed in this book, viz. in the page printed next after the epistle to the reader, where it is said, that this treatise has been examined by six commissioners appointed by the king, and the truth of it affirmed to his majesty; whereas of the six persons there named, the Right Hon. the Lord Viscount Brouncker, chancellor to her Majesty, P.R.S. declares that he never so much as saw this treatise before it was printed, nor was ever present at any of the meetings of the other commissioners; the quality of the report of whom concerning this matter, the reader will doubtless be acquainted with in due time.

V. The Royal Almanack. By N. Stephenson, one of his Majesty's Gunners. London, 1677, in 12mo.

This almanack is a very useful diary of the true places of the sun, moon, and other planets; their rising, southing, and setting; as also of high water at London-bridge, with rules to serve other places after the new theory of tides, and directions of Sir Jonas Moore. To which are added the eclipses, with a table of equations for the regulating curious pendulum clocks and movements, to the sun: likewise a table of the sun's right ascension in time for every day at noon, and of thirty of the most notable fixed stars; together with the moon's and the other planets' appulses to the fixed stars, for the meridian of London, in the year 1677; as also a transit of Mercury under the sun, calculated for

* Mr. Henry Bond was an intelligent mathematician and teacher of navigation, whose table of the variation of the magnetic needle was published in the Philosophical Transactions, No. 40, p. 282, vol. i. of this Abridgement. Sir Charles Cavendish, in an original letter of his from Antwerp, Nov. 13, 1648, to Mr. John Pell, then professor of mathematics at Breda, mentions Mr. Bond as "an old mathematician at London, who speaks very meanly of himself, and yet he found an easy and short demonstration of that proposition concerning spherical triangles, which Mr. Oughtred demonstrated first, who told me Mr. Bond's demonstration was shorter."

October 28, next. All done with great care and pains at his Majesty's command.

New Experiments on the Superficial Figures of Fluids, especially of Liquors contiguous to other Liquors. By R. Boyle, Esq. N° 131, p. 775.

What has been said about the pores of liquors may be somewhat illustrated or confirmed, if I subjoin to it some of the trials I have made about the surfaces of fluids contiguous to other fluids. For this being a neglected subject, and the little that has been said about it consisting of a few slight and casual observations, that seem to have been rather presented to us, not to say obtruded upon us, than designedly made by us; I many years ago thought it might be worth while to spend some hours on experiments of this sort; which I was especially induced to do, because I think one may probably enough suppose, that in the tract of the universe that is yet known to us, there is not the hundredth, perhaps not the thousandth part, that is formed into solid bodies, such as the earth, the moon, and the other planets; and consequently all the rest is made up of celestial fluids and the atmospheres of solid globes, which, for ought we know, though not manifestly differing in transparency, may be disterminated by distinct surfaces. So that, to observe and consider the effects of the congruity and incongruity, that liquors, or such fluid bodies, as directly or otherwise fall under sensible observation, have when they are contiguous to each other, or to the surfaces of solid bodies, may not only improve what is yet known about the ascension of liquors in small pipes, but may perhaps serve to illustrate the formation of those great masses of matter, of which the Divine Architect has framed the mundane globes, and some other considerable parts of the universe, especially if we admit the Cartesian hypothesis, that the sun and all the fixed stars are fluid bodies.

The cause why water in narrow pipes ascends above the level of the surrounding water having been already inquired into by some ingenious men, and particularly by Mr. Hooke, I shall not now treat on that subject, nor mention what I have tried about it; but shall rather remark, that because I suspected that the concave figure, which may be observed in the surface of water included in slender pipes, may, at least in great part, depend on its relation to the contiguous fluid, which commonly is the air, I thought fit to try whether this concave figure would not be altered by substituting another liquor in the room of the air; and accordingly having procured a strongly alcalizat menstruum, viz. that made of fixed nitre, dissolved by the moisture of a cellar, into a tube of glass, sealed at one end, and not quite a quarter of an inch in bore, that the cavity, which in a great breadth would seem less deep, might be the more conspicuous; we gently poured on it some highly dephlegmed spirit of wine, which we knew would not mix with it, but swim above it, and presently we found the figure of the surface of the lower liquor changed, and the cavity quite destroyed; the surface that seemed, as it were, common to the two contiguous liquors, appearing flat or horizontal. And such a level superficies we had by putting those two liquors together in a much wider glass.

We found also, that by employing oil of turpentine instead of spirit of wine, the liquor almost totally lost its cavity. But if, instead of deliquated tartar, we put common water into the pipe, we found this liquor to retain its concave surface, though we put to it some oil of turpentine, and left it to rest on the water a good while. Having provided some pure oil of gum guaiacum, which is heavier than water, and poured a little of it into a slender tube, the upper superficies of it became concave; almost, if not altogether, like that which water would have had in the same place. But when I put a little water upon this oil it presently changed the figure of its surface, which became visibly, though not very much, protuberant or convex.

And as this oil, though heavier than water, is not so heavy as deliquated salt of tartar, I thought fit to try, whether the phenomenon would not be different upon the contact of those two liquors; and accordingly having put some oil of tartar into the slender pipe, and some drops of the oil of guaiacum to it, this liquor did not manifestly alter the concave figure of the surface of the liquor alcali, as the oil of turpentine had done; and having warily poured a little water on the oil of guaiacum, the upper superficies of it changed presently from a concave figure to a convex; so that this oil, in the midst of the other two liquors, appeared like a little red cylinder, which, instead of having circular bases, was protuberant at both ends, but more at that which touched the oil of tartar.

To vary a little the experiment, I put some essential oil of cloves into a new slender pipe, and having observed it to be somewhat concave at the top where it was contiguous to the air, we caused a little common water, perhaps a quarter of a spoonful or less, to be put to it, and found the surface of this oil also to become tumid. And since this liquor, as well as the forementioned oil of guaiacum, though it were so heavy as to sink in water, would not do so in deliquated salt of tartar, we put into another slender pipe first some of this last named liquor, then some of the aromatic oil, and lastly a little common water; by which means we found, that the little cylinder of oil did, like that of the oil of guaiacum, appear convex at both ends; but was unlike it in one circumstance, that the oil of cloves appeared more convex at the upper end, where it was contiguous to the water, than at the lower, that rested on the surface of the oil of tartar.

Having made these trials to alter, by another contiguous fluid than the air, the concave superficies of water and some aqueous liquors, I proceeded to try whether a change would not likewise be made on the convex figure of the surface of quicksilver included in the like slender glasses; and accordingly having taken one that was much longer, but of the like bore with the former, we put into it a small quantity of quicksilver, and having observed how the upper superficies swelled in the middle above the level of the parts where it touched the glass, we poured some water upon it, and found a manifest and considerable depression of the surface, though the protuberance was not quite suppressed.

This phenomenon having been for greater security several times repeated, I thought fit to try, what variation would be made by the greater or less height of the water incumbent on the mercury. And sometimes it seemed that when the aqueous cylinder was much longer, the depression of the mercurial surface was somewhat greater. But this did not so constantly happen, but we often observed, that though a very little water sufficed by its contact to make, in the judgment of the eye, a manifest abatement of the protuberance of the quicksilver, yet it had not the same effect on that ponderous fluid that it had when, being increased almost as high as the length of the pipe would permit, a greater weight of it was incumbent on the mercury.

Because the common atmospherical air we breathe is a fluid body abounding with grosser particles, and is by divers philosophers probably supposed to be much more dense and heavy than the æthereal substance, that makes the other part of the atmosphere; I thought fit to try for their sakes, whether the superficial figure of liquors would be altered by having the contiguous air withdrawn from about them, and so being left to be touched by the purer ether without it; and accordingly having conveyed into one of our pneumatical receivers a couple of such slender pipes as have been already described, one of them furnished with common water, and the other with quicksilver, we caused the common air to be pumped out, without observing any sensible change in the concave figure of the water, but as for the quicksilver I knew not what to conclude about it. For having repeated the trial twice or thrice, the mercury sometimes seemed manifestly to swell and be more protuberant on the exhaustion of the receiver, than when it was put in. But that which yet kept me doubtful was, that I observed, that on the withdrawing of the air's pressure on the quicksilver, there appeared in it some little bubbles, which I feared we had not been able altogether to free it from, and which might be suspected to have some interest in the phenomenon. I shall only add, that we conveyed into our receiver a clear chemical oil, that was heavier than water, and whilst it was contiguous to it, it had not a concave but a convex surface; and having placed the pipe furnished with both liquors in the pneumatical receiver, we pumped out the air, without finding that the oil sensibly altered its protuberant surface, as neither did the water lose the concave figure of its upper surface.

When clouds are condensed into rain, and lower aggregates of vapours into dew, it is supposed to be obvious, that the drops of those meteors do, in their passage through the air, acquire a round figure; and when we shake oil into water, the portions of the former fluid, during the little time they remain distinct, are found to be globular. But these phænomena are too few, and too transient, to afford any considerable observation of the figures of fluid bodies, especially if they be quiescent, and every way encompassed by other fluids. Wherefore I thought fit to try what I could do with chemical liquors unapt for mingling, to produce phænomena that may last long enough to allow us to observe them attentively, and in some cases to vary them.—For this purpose I first took fixed nitre, or which is analogous to it, salt of tartar, resolved per deliquium into a transparent liquor, and having filled a clear phial half full with this, I poured on it a convenient quantity of vinous spirit exactly rectified, that there might be no phlegm to occasion an union between the two liquors, which ought, as ours did, to retain distinct surfaces, and speedily regain them though the glass were well shaken. Then having found, by a trial formerly mentioned, that common oil of turpentine, if employed in a competent quantity, will not totally, and much less will readily, dissolve in spirit of wine; and also having observed, that if this spirit of wine be exquisitely dephlegmed, the oil though a chemical one, will not swim on it, but sink in it; I warily let fall some drops of the oil into the spirit, and had the pleasure to see that they fell towards the bottom of the glass, till their descent was stopped by the horizontal surface of the alcalizat liquor of fixed nitre. And because my design was chiefly to observe the superficial figure of a fluid encompassed by other fluids without touching any solid body, I shall here take notice of the chief phænomena that were produced of that kind, without staying to inquire into the causes or the consequences of them.

1. If the oily drops were but small, they seemed to the eye exactly enough spherical. For the oil differing but little in specific gravity from the spirit of wine, the drops did but just touch the surface of the subjacent alcali; and the same drops being but small, their own weight was not great enough visibly to depress them, and prevent that roundness which the pressure of the ambient spirit, or their own viscosity endeavoured to give them.—2. If an aggregate of drops were considerably larger than those newly mentioned, it would then

manifestly pass upon the alcalizat liquor as upon a floor, and appear somewhat elliptical; the weight of the upper parts depressing the drops, and making the horizontal diameter somewhat longer than the transverse.—3. If a yet greater portion of oil were let fall on the heavy liquor, it would for a pretty while appear in the form of a somewhat imperfect hemisphere, or some other large section of a sphere, the lower part being cut off, by the horizontal surface of the deliquated salt.—4. But if the quantity of oil were not too great, it was pleasant to observe, that though at first putting in it perhaps spread itself over the subjacent liquor, and lie as it were flat upon it; yet by slow degrees it would be crowded together into a figure of a lesser surface, and consequently less hindering the motions of the vinous liquor. For by the action of this spirit, the oil would by degrees be raised above the surface of the fluid nitre, and be reduced to the figure, either of half a globe, or of a greater segment of a globe, or even of an imperfect ellipsis, according as the bulk or weight of the oil made it more or less apt to resist the action of the ambient spirit.—5. Though these globules or portions of oil did often readily mingle, when they touched one another, yet divers times also we observed, that having warily approached them, we were able to make them touch without mingling; as if some odd subtile matter, that the eye could not discern, interposed, to keep them unconfounded. Insomuch that we have with pleasure made them so far bear against one another's surfaces, as manifestly to press them inwards, though being parted they would presently resume their former figure: which circumstance suggested to me suspicions, that I cannot now stay to name. But in case any of these oily portions came by a more pressing contact to be united, they would then alter the figures they had whilst separate, and take others, suitable to the bulk of the aggregate.—6. When a large portion of oil rested on the saline liquors; if then the ambient spirit were moderately and warily agitated, it was not unpleasant to observe the various figurations, which the convex and protuberant part of the mutilated globe would be put into by these shakes, without any visible solution of continuity, or considerable motion of the whole body, which would very quickly recover its former figure. Though if the agitation were too strong, some portions would be quite broken off, and presently turned into little globes.

I tried to produce another phenomenon that would not have been unpleasant, by putting together, in a pretty large vessel with other liquors, two oils, which first by reason of the oleaginous nature wherein they agreed, might exactly mingle and make a compounded liquor; and then by reason of their being one heavier, and the other lighter in specie than water, might by this liquor be again separated, and include between them the liquor that had divided them. But I found that the oils, being once united, would not be easily parted, but according to the prevalency of the lighter or heavier ingredient in the mixture, the compounded oil would almost totally either emerge to the top of the water, or lie beneath the bottom of it; I say almost totally, because some parts of the oil, which was not perhaps all uniformly mixed, did not keep in a body with the rest; but either was separated from the mass in the form of globules, or else sticking to the side of the glass, had the other part of its superficies, which was contiguous to the water, very variously figured, according as the bulk and degree of gravity of the adhering oil and other circumstances happened to determine. And it is chiefly on account of this various and odd figuration of our mixture, that I here mention this trial.

These are some of the phænomena I observed in oil of turpentine, when it was environed only with fluids; but, when permitted to be contiguous to the inside of the glass, and so to fasten part of its surface to a solid, the greater part of the surface which remained exposed to one or both of the contiguous liquors, would partly by their action, and partly by the gravity of the oil itself, be put into figures so various, and sometimes so extravagant, that it was much more pleasant to behold them than it would be easy to describe them; which therefore I shall not here attempt to do.

When the oil of tartar or nitrous alcali, happened to be very clear and colourless, I have sometimes made highly rectified spirit of wine float upon it, so that in most positions the phial seemed to have in it but one uniform liquor.

Taking deliquated alcali, made of nitre and tartar, and deeply tinged with cochineal; and that the liquors might not only be heterogeneous, but as differing in gravity and density as we could make them, we poured on it a peculiar kind of oil lighter than spirit of wine, and holding the plain where the two liquors were contiguous in a convenient position, in respect of the light and the eye, I observed it to make a strangely vivid reflection of the incident beams of light: so that this physical surface, which was flat, looked almost like that of quicksilver; and the bright figure of the flame of a candle was strongly reflected almost as from a close specular body.

To these phænomena I shall add another, which is, that though pure spirit of wine be so thin a liquor, and our oil is so light as to swim upon it; yet I found the confining surface very strongly reflexive.—I found also that some other essential oils, and particularly an unsophisticated oil of lemons, did with our tinged alcali, afford most of the same phænomena; but not so brisk a reflection: I say most chiefly, because with spirit of wine these subtile oils, as I formerly noted, will readily be confounded.

A Letter from Christianus Adolphus Balduinus to Mr. Oldenburg, Secretary of the Royal Society. Translated from the Latin.* N° 131, p. 788.

I should immediately have acknowledged the favour of the letter you wrote me last year, had I not thought it right to wait until I had perfected the method of preparing my phosphorus. This I have but lately done; and I now send you a specimen thereof, inclosed in a gilt silver box; which I request you to have the goodness to offer, with all humility and respect on my part, as a small present to his Majesty, as founder and patron of your Society; and to the illustrious President, Council, and Members thereof. This phosphorus contains the real spark, yea the most secret soul (imo secretissima anima) of the fire and light of nature, consequently the innate and invisible fire of philosophers; attracting magnetically the visible fire of the sun, and afterwards emitting and diffusing in the dark the splendor of the same: This happens from hence, viz. that the signature of the sun is contained in that universal magnet from whence this phosphorus is prepared;† as indeed is sufficiently apparent from the accompanying representation of the phenomenon, which lasts several days.‡ But although I have thus succeeded in getting possession of the universal magnet, I shall not desist from the farther prosecution of my chemical labours, as I am in expectation ere long of deriving from this source still greater and more important discoveries; concerning all which I shall not fail to transmit an account to your illustrious Society, as soon as I shall have completed my experiments.

Hayn, Sept. 1, 1676.

Note by Mr. Oldenburg.—This present was according to the tenor of this

* Christian Adolphus Baldwin was a magistrate of the town of Hayn or Grossenhayn in Saxony, and was much devoted to the then fashionable pursuit of alchemy. For some purpose or other he had dissolved a quantity of chalk in nitrous acid, and evaporated the compound to dryness in a strong heat. On breaking the glass retort in order to get at the residuum, some fragments thereof fell upon the floor of the laboratory, and remained there until the next day; when they attracted the notice of our alchemist by their luminous appearance in the dark. Thus was the discovery of this chemical phenomenon wholly accidental.—This phosphorus is a nitrate of lime. Besides the above letter, a communication concerning it was published in the Ephemerid. Natur. Curios. for 1674; but the most ample account of it is given in the author's treatises entitled Aurum superius et inferius Auræ superioris et inferioris hermeticum; et Phosphorus hermeticus seu Magnes Luminaris, 1673 et 1675.

† In the above reflections we have a tolerable specimen of the absurd mode of philosophizing among the chemical or rather alchemical writers at this period of time.—Note by the Translator.

‡ This phenomenon exhibits, in a glass vessel, various figures of the sun, some greater some less, which the substance employed by the author assumed, to the no small diversion of the spectators.—Original.

letter, made to his Majesty, and afterwards to the Royal Society; it fully justified the generous presenter in the experiment, made before them both, at several times; and that not only by day-light, even when the weather was gloomy and misty, but also by the flame of a candle.

An Account of three Books. N° 131, p. 790.

I. Clavis Philosophiæ Naturalis, Aristotelica Cartesiana, Editio secunda, aucta Opusculis Philosophicis varii Argumenti; quibus Errores Scholarum passim deteguntur, ac Veritas Philosophiæ, quam Cartesianam vocant confirmatur. Auth. Johanne de Raei, Phil. in Illustri Athenæo Amstelod. Prof. prim. Amstelod. Anno 1677, in 4to.

As the first edition of this book, printed many years since, contained chiefly six dissertations, viz. concerning 1. Vulgar and philosophical knowledge:—2. Philosophical principles in general: 3. The nature of body: 4. The origin of motion, together with an appendix, giving an account of Aristotle's opinion of the first mover: 5. The communication of motion, and the action of bodies upon one another: 6. The subtile ethereal matter; so this second edition is enlarged with seventeen discourses, which are chiefly concerning logic, metaphysics, with some parts of the Cartesian and Aristotelian philosophy, now long since exploded.

II. Nouvelle Science des Temps, ou Moyen general de concilier les Chronologues; par le S. Menard, Seigneur d'Iserné. A Par. in 12mo.

There being found so little certainty among chronologists, this author endeavours to reconcile them, by proposing four principles, whereby he pretends to make it out, that they may be made to agree together, in respect of the several æras and epochs of time.

III. England's Improvement by Sea and Land: to outdo the Dutch without Fighting: to pay Debts without Money: to set at Work all the Poor in England with the Growth of our own Lands: to prevent unnecessary Suits in Law, with the Benefit of a Voluntary Register: Directions where vast Quantities of Timber may be had for the building of Ships: with the Advantages of making the Great Rivers of England Navigable: Rules to prevent Fires in London, and other Great Cities: with Directions how the several Companies of Handicrafts-men in London may always have cheap Bread and cheap Drink. By Andrew Yarranton, Gent. in 4to.

This author, it seems, has discovered the mysteries of trade universally for all parts of England. And he detected the mysteries of iniquity, how some wealthy merchants and overbusy factors hinder trade and our staple manufactures, for private lucre, to the great damage of their own native country. He advises good remedies; and proposes what trades are proper to be advanced in the several parts of England; how to be there advanced; and what the peculiar conveniences.

Thus he runs through all the intrigues of trade, noting the secret abuses and obstacles; and offering genuine remedies, confirmed by the experience of foreign nations, large territories and principalities.

A Continuation of Mr. Boyle's Experiments published in the last Number of the Transactions. N°. 132, p. 799.

On pouring into a slender phial, more than half filled with common water, that had been a little warmed, a little essential oil of aniseeds, its upper surface became somewhat concave, as that of the water was; but the lower surface surrounded by the water was very convex, appearing almost of the figure of a large portion of a sphere. This being done, the phial was stopped, and suffered to rest for some time in a cold place, by which means the water continuing fluid as before, the oil of aniseeds was found coagulated in a form approaching to that it had whilst in a fluid state.

And it was worth observing, how great a difference there was between the dull reflection it made when it was coagulated, and the fine reflection it had made whilst fluid. The latter of which reflections brought into my mind, how vivid the reflective power of some fluids is, in comparison of that of the generality of solid bodies, of which there is scarcely any, if there be any at all, that is observed to have a stronger reflection than clean quicksilver; and yet I have sometimes found, that even this may be increased by the addition of a liquor. For having observed that quicksilver and rectified petre oleum are, the former of them the heaviest, and the latter the lightest of all the visible fluids; and having also observed the latter of them to be considerably reflective, I had the curiosity to try among other things that related to them, the following experiment. Some distilled quicksilver being put into a small phial, and held in such a posture, that the incident light was strongly reflected to my eye, I slowly put to it some petroleum, which being well rectified was very clear; and observed, that as this liquor covered the quicksilver, there was at the imaginary plain where they both met, a brisker reflection than the quicksilver alone had given before.

On putting a competent quantity of a resinous or gummy substance, that looked like high coloured amber, but was easy to melt, into a deep round glass with a wide mouth, and holding it by the fire in a moderate warmth, till it was brought to a fluid state; we then transferred it into a pneumatical receiver, and having caused the air to be pumped out by degrees, we found that store of bubbles appeared at the top of the liquor, and made there a copious froth, many of them being, by reason of the viscosity of the fluid, very large, and divers of them, because of the nature and texture of it and the thinness of the films, exhibiting the colours of the rainbow. When this substance had resumed its consistent form, there were intercepted between the upper and the lower surfaces of it, some large bubbles which had a considerable reflection.

Water being so considerable a body here below, I thought it would be worth while to endeavour to observe its surface when contiguous to other fluids than air, and if possible, when surrounded by them. For though it is taken for granted, that the falling drops of rain are spherical, yet their descent is so swift, both by reason of their gravity in respect of the air, and the height from whence they fall, that I fear men have rather supposed than observed that their figure is spherical; which will be the more questionable, if it be true, which is commonly thought, that hail is but rain frozen in its passage through the air. Now the surface of water may have different figures, according as it is totally incompassed with heterogeneous fluids, or, as it is only in some places contiguous to one or more of them. In the former case it is not easy to make an observation, because there are not any two liquors that will not mingle either with one another or with water. Having cautiously therefore conveyed into some oil of cloves, some portions of common water of different sizes, taking care as far as we could, that they might not touch one another; by which means, the oil being transparent, and yet a little coloured, it was easy to observe, that the smaller portions of water were so near totally environed with the oil, that they were reduced into almost perfect globes; those portions that were somewhat larger, as about twice the size of a pea, would be of a figure somewhat approaching to that of an ellipsis; and those portions that were yet somewhat larger, though they seemed to be sunk almost totally beneath the oil, yet they held to it by a small portion of themselves, and their surface was easily enough distinguishable from that of the oil. These larger portions of immersed water, being almost wholly environed with the other liquor, were by it reduced into a round figure, which was commonly somewhat elliptical, but more depressed in the middle than that figure requires. But all this is to be understood of those portions of water, that touched only the oil and the air: for those that touched each other without mingling, and much more those that adhered more or less to the sides of the glass, had their surfaces too differingly and irregularly figured to be here attempted to be described.

Having put into a slender pipe a little oil of cloves, and upon this some oil of turpentine, that so the water might both above and beneath be touched by heterogeneous liquors, the oil of cloves was not manifestly tumid at the top, nor the lower surface of the oil of turpentine, for the upper was concave, very convex; for somewhat convex it was downwards. And from this it will be easy to conclude the figure of the cylindrical portion of water intercepted between these two oils.

Having taken oil of aniseeds, thawed by a gentle warmth, and common water, and having put them together in a conveniently shaped glass, they were suffered to stand in a cold place till the oil was coagulated; which done, it was parted from the water, and by the roughness of its superficies, showed that when its parts were no longer agitated, or whatever other agent or cause it were, to which it owed its fluidity, then the contiguous water grew unable to inflect, or otherwise place them after the manner requisite to constitute a smooth surface. And what happened to that part of the oil's surface that was touched by the water, happened also to that which was contiguous to the air; save that the asperity of the last surface was differing from the other. But I have often observed, that the upper surface of the oil of aniseeds, when this liquor comes to be coagulated by the cold air, was far from smooth, being variously asperated by many flaky particles, some of which lay with their broad, and others with their edged parts upwards.

An inequality and ruggedness of superficies I have also observed in water, on covering it with chemical oil of juniper, and exposing it in very cold weather; though the oil continued fluid, yet the water, being frozen, had no longer a smooth superficies, as when in its liquid state it was contiguous to the oil. And the like inequality, or rather a greater, we observed in the surface of water frozen, which had chemical oil of turpentine swimming on it; yet a no less, if not a much greater roughness, may be often observed in the surfaces of divers liquors that abound with water, when those liquors being frozen, their surfaces have an immediate contact with the air. I shall here add, that having purposely caused a strong and blood red decoction of the soot of wood to be exposed in a large glass, in a very cold night, I was more pleased than surprised, to find in the morning a cake of ice, curiously figured, being full of large flakes, shaped almost like the broad blades of daggers, but neatly fringed at the edges. And these figures seemed to be as it were imbossed, being both to the eye and the touch raised above the horizontal plain or level of the other ice.

This may be observed in the best sort of what the chemists call regulus martis stellatus, where the figure of a star, or a figure somewhat like that of the decoction of soot lately mentioned, will frequently appear embossed on the upper superficies of the regulus; and such a raised figure I have on a mass of regulus made of antimony without mars. But if to those two bodies copper be also skilfully added, the superficies will be often adorned with new figures according to circumstances: though the most usual I took notice of was that of a net, that seemed to cover the surface of the compounded regulus.

Account of Two Books. N° 132, p. 808.

I. Palæologia Chronica: a Chronological Account of Ancient Time, in three parts: Didactical, Apodictical, Chronical. By Robert Cary, LL.D. Devon; London, 1677, in fol.

The design of this elaborate work seems to be, to determine the just interval of time between the great epocha of the creation of the world, and that other of the destruction of Jerusalem by Titus Vespasian, in order to the assignment of such particular time, wherein persons and actions of old had their existence. For the performance of which, the learned author divides this his book into three main parts.

In the first he treats not only of his measure in general, which is the year, and its parts; but also of the Julian year in particular, by him esteemed the fittest for his use. Having showed the use of this period, he adds the method of reducing the years of other reckonings to the Julian year, and to that of the Julian period: as that of the Egyptian or Nabonassaræan; that of the city of Rome; the Grecian and Jewish year, &c.

In the second part, are laid down the two bases of Chronography, viz. astronomical observations, and historical tradition: of which the former may be considered as certain and demonstrative; the latter must be distinguished according to the historians, as they are with us more or less creditable, or more or less consonant with others of good credit. Here occurs first, a thesaurus of astronomical phænomena, or a table of eclipses and other celestial appearances, with the time in which they were observed, according to the writings of historians and mathematicians. Next, creditable memorials of the succession of princes and rulers, serving to direct these inquiries, as is that considerable astronomical canon deduced from Nabonassar to Antoninus Pius, under whom Claud. Ptolomæus, the famous Egyptian mathematician flourished.

Having thus in many places of the world searched out the originals of government, by following the line of their successions ordine retrogrado; he passes in the last place to survey the reckonings of the Holy Land, the Jews and Hebrews of old time, according to those ancient records, the holy scriptures; that so if he can obtain this end of his labours, which is, to see a good agreement between these several lines, viz. of the Gentile draught, and of the Jewish protraction, men may sit down well content therewith, as having mastered a matter of no small importance.

The third and last part, which is canonical, as the first has been didactical, and the second apodictical, is drawn much after the pattern of Helvicus the German chronologer, as is owned by the author himself, which is one of the most comprehensive and best forms that is extant.

II. A Touchstone for Gold and Silver Wares, or a Manual for Goldsmiths, and all other Persons, whether Buyers, Sellers, or Wearers of any Manner of Goldsmith's work, &c. By W. B. of London, Goldsmith, in 8vo.

END OF VOLUME ELEVENTH OF THE ORIGINAL.

Some Agrestic Observations and Advertisements, from Dr. John Beale, communicated to the Editor. N° 133, p. 816. *Vol. XII.*

These observations are of no use now.

Observations on Animalcula seen in Rain, Well, Sea, and Snow-water; as also in Pepper-water. By M. Leewenhoeck. N° 133, p. 821.

In the year 1675, I discovered very small living creatures in rain water, which had stood but few days in a new earthen pot, glazed blue within. This invited me to view this water with great attention, especially those little animals appearing to me ten thousand times less than those represented by M. Swammerdam, and by him called water-fleas, or water-lice, which may be perceived in the water with the naked eye.

The first sort I several times observed to consist of 5, 6, 7, or 8 clear globules, without being able to discern any film that held them together, or contained them. When these animalcula or living atoms moved, they put forth two little horns, continually moving. The space between these two horns was flat, though the rest of the body was roundish, sharpening a little towards the end, where they had a tail, near four times the length of the whole body, of the thickness, by my microscope, of a spider's web; at the end of which appeared a globule of the size of one of those which made up the body. These little creatures, if they chanced to light on the least filament or string, or other such particle, were entangled therein, extending their body in a long round, and endeavouring to disentangle their tail. This motion of extension and contraction continued a while; and I have seen several hundreds of these poor little creatures, within the space of a grain of gross sand, lie fast clustered together in a few filaments.

I also discovered a second sort, of an oval figure; and I imagined their head to stand on the sharp end. These were a little larger than the former. The inferior part of their body is flat, furnished with several extremely thin feet, which moved very nimbly. The upper part of the body was round, and had within 8, 10, or 12 globules, where they were very clear. These little animals sometimes changed their figure into a perfect round, especially when they came to lie on any dry place. Their body was also very flexible; for as soon as they struck against any the smallest fibre or string, their body was bent in, which bending presently also jerked out again. When I put any of them on a dry place, I observed that, changing themselves into a round, their body was raised pyramidal-wise, with an extant póint in the middle; and having laid thus a little while, with a motion of their feet, they burst asunder, and the globules were presently diffused and dissipated, so that I could not discern the least thing of any film, in which the globules had doubtless been enclosed; and at this time of their bursting asunder, I was able to discover more globules than when they were alive.

I observed a third sort of little animals, that were twice as long as broad, and to my eye yet eight times smaller than the first. Yet I thought I discerned little feet, whereby they moved very briskly, both in a round and straight line.

There was a fourth sort, which were so small that I was not able to give them any figure at all. These were a thousand times smaller than the eye of a large louse. These exceeded all the former in celerity. I have often observed them to stand still as it were on a póint, and then turn themselves about with that swiftness, as we see a top turn round, the circumference they made being no larger than that of a small grain of sand, and then extending themselves straight forward, and by and by lying in a bending posture. I discovered also several other sorts of animals; these were generally made up of such soft parts, as the former, that they burst asunder as soon as they came to want water.

May 26, it rained hard, the rain growing less, I caused some of that rain-water, running down from the house-top, to be gathered in a clean glass, after it had been washed two or three times with the water. And in this I observed some few very small living creatures, and seeing them, I thought they might have been produced in the leaden gutters in some water that had remained there before.

I perceived in pure water, after some days, more of those animals, as also some that where somewhat larger. And I imagine, that many thousands of these little creatures do not equal an ordinary grain of sand in bulk; and comparing them with a cheese-mite, which may be seen to move with the naked eye, I make the proportion of one of these small water-creatures to a cheese-mite, to be like that of a bee to a horse; for, the circumference of one of these little animals in water is not so large as the thickness of a hair in a cheese-mite.

In another quantity of rain-water exposed for some days to the air, I observed some thousands of them in one drop of water, which were of the smallest sort that I had seen hitherto. And in some time after I observed, besides the animals already noted, a sort of creatures that were eight times as large, of almost a round figure; and as those very small animalcula swam gently among each other, moving as gnats do in the air, so did these larger ones move far more swiftly, tumbling round as it were, and then making a sudden downfal.

In the water of the river of Maese I saw very small creatures of different kinds and colours, and so small, that I could very hardly discern their figures; but the number of them was far less than of those found in rain-water. In the water of a very cold well in the autumn, I discovered a great number of living animals very small, that were exceedingly clear, and a little larger than the smallest I ever saw. In sea water I observed at first, a little blackish animal, looking as if it had been made up of two globules. This creature had a peculiar motion, resembling the skipping of a flea on white paper, so that it might very well be called a water-flea; but it was far less than the eye of that little animal, which Dr. Swammerdam calls the water-flea. I also discovered little creatures therein that were clear, of the same size with the former animal, but of an oval figure, having a serpentine motion. I further noticed a third sort, which were very slow in their motion; their body was of a mouse colour, clear towards the oval point; and before the head and behind the body there stood out a sharp little point angle-wise. This sort was a little larger. But there was yet a fourth sort somewhat longer than oval. Yet of all these sorts there were but a few of each. Some days after viewing this water, I saw 100 where before I had seen but one; but these were of another figure, and not only less, but they were also very clear, and of an oblong oval figure, only with this difference, that their heads ended sharper; and although they were a thousand times smaller than a small grain of sand, yet when they lay out of the water in a dry place, they burst in pieces, and spread into three or four very little globules, and into some aqueous matter, without any other parts appearing in them.

Having put about one-third of an ounce of whole pepper in water, and it having lain about three weeks in the water, to which I had twice added some snow-water, the other water being in great part exhaled; I discerned in it with great surprise an incredible number of little animals, of divers kinds, and among the rest, some that were three or four times as long as broad; but their whole thickness did not much exceed that of the hair of a louse. They had a very pretty motion, often tumbling about and sideways; and when the water was to let to run off from them, they turned round like a top; at first their body changed into an oval, and afterwards, when the circular motion ceased, they returned to their former length. The second sort of creatures discovered in this water, were of a perfect oval figure, and they had no less pleasing or nimble a motion than the former; and these were in far greater numbers. There was a third sort, which exceeded the two former in number, and these had tails like those I had formerly observed in rain-water. The fourth sort, which moved through the three former sorts, were incredibly small, so that I judged, that if 100 of them lay one by another, they would not equal the length of a grain of coarse sand; and according to this estimate, 1,000,000 of them could not equal the dimensions of a grain of such coarse sand. There was discovered a fifth sort, which had near the thickness of the former, but almost twice the length.

In snow-water, which had been about three years in a glass bottle well stopped, I could discover no living creatures; and having poured some of it into a porcelain tea-cup, and put therein half an ounce of whole pepper, after some days I observed some animalcula, and those exceedingly small ones, whose body seemed to me twice as long as broad, but they moved very slowly, and often circularly. I observed also a vast multitude of oval-figured animalculæ, to the number of 6000 or 8000 in a single drop.

Some Observations, made by Sig. Cassini, concerning the two Planets about Saturn, formerly discovered by him, as appears in N° 92 *of these Tracts.* N° 133, p. 831.

One of these two planets, which is distant from the centre of Saturn 10 diameters and a half of his ring, makes its revolution about Saturn in 80 days. It was discovered at the Parisian Observatory, anno 1671, about the end of Oct. and in the beginning of Nov. in his greatest occidental digression, and after many cloudy days it ceased to appear, for a reason which was then unknown, but has since been discovered. For, after many revolutions of this small planet had been observed, it was found to have a period of apparent augmentation and diminution, by which period it becomes visible in its greatest occidental digression, and invisible in its greatest oriental elongation.

Hence it seems, that one part of its surface is not so capable of reflecting to us the light of the sun as the other part is. Whence we may conjecture, that the globe of this satellite has some diversity of parts, analogous to that of the earth, the one part of whose surface is covered by the sea, which is not so fit to reflect from all parts the light of the sun, as the continent which makes up the other part; so that this planet, by a rotation about its axis, or by an exposition of the same hemisphere to Saturn, much after the manner of the hemisphere of the moon to the earth, sometimes turns to us the part analogous to the continent, sometimes that part which answers to the sea.

This vicissitude of phases in this planet was the cause that it could not be found since it was first discovered in the year 1671, till the middle of Dec. 1672; after which time it disappeared once again, until the beginning of Feb. 1673, at which time, having been observed 13 days successively, it afforded the opportunity of determining the period of its motion.

Since that time, as often as Saturn has been distant enough from the sun to enable us to discern this planet, it has always been seen in all its occidental digressions and in the conjunctions with Saturn, which have since happened with a great latitude, as well in the upper part of its circle as in the lower, and it could never be seen in the oriental digressions, where it remains invisible in every revolution of 80 days for a whole month together.

It begins then to appear 2 or 3 days before the conjunction in the inferior part, and to disappear 2 or 3 days after the conjunction in the superior part.

The other planet, which was discovered about the end of the year 1672, has its greatest digression from the centre of Saturn only one diameter and two-thirds of his ring, and the period of its revolution about Saturn is 4 days and a half, but more precisely, 4 days, 12 hours, and 27 minutes. Its latitude augments also according as the ring enlarges. We have not yet been able to distinguish it in the conjunctions, either in the upper or lower part of its circle, but only in the greatest, as well oriental as occidental, digressions. And this satellite being alternately one day towards its conjunction and the other day towards the digression, it is ordinarily seen only every third day, and rarely 2 days together, when it falls out that at the hour of observation it is in the middle between the conjunction and digression.

Lastly, the apparent magnitude of these planets is so little, that posterity will have cause to wonder that their discovery was begun by a glass of 17 feet.

An Account of some Books. N° 133, p. 833.

I. Pharmacopœia Collegii Regalis Lond. A. 1677, in fol.

II. Catalogus Plantarum Angliæ, et Insularum adjacentium, tum indigenas, tum in agris passim cultas complectens, &c. edit. secunda; operâ Johannis Raii, M.A. è Soc. Regia. Lond. 1677, in 8vo.

In this edition the accurate and learned author presents the curious with a considerable number of plants not contained in the first, which amount to about

46, some of which were forgotten in the former edition, some were newly found out by him. Besides many useful observations.

III. Aëro Chalinos; or, A Register for the Air, &c. By Nathan. Henshaw, M.D. Fellow of the Royal Society. Lond. 1677, in 12mo.

This tract consists of 5 chapters, of which the first treats of fermentation; the second of chylification; the third of respiration; the fourth of sanguification; and the fifth of the salubrity of frequently changing the air; with a discovery of a new method of doing it without removing from one place to another by means of an air-chamber fitted to that purpose.

IV. A Philosophical Essay of Music. Lond. 1677, in 4to.

This author's design being to explain the nature of music, he begins to inquire into the cause of sound; in order to which, he considers some of the chief phænomena of sound. According to him, sound may be caused by the trembling of solid bodies, without the presence of gross air, and also by the restitution of gross air, when it has been divided with any violence. Having laid down this hypothesis, and left his reader to apply it to the phænomena, he proceeds to the discourse of music itself, and makes it a considerable part of his business to show, how this action that causes sound is performed by the several instruments of music; having taught his reader first, what a tone is, and that the tones useful in music are those within the scale, in which they are placed as they have relation to each other. Secondly, wherein consists that relation of tones and the union of mixed sounds. Which done, he explains, how tones are produced, and what assistances are given to the sound by instruments. Where he teaches, that wherever a body stands on a spring that vibrates in equal terms, such a body put into motion will produce a tone, which will be more grave or acute, according to the velocity of the returns; wherefore strings vibrating have a tone according to the thickness and tension of them; and bells that vibrate by cross ovals, produce notes according to their size or the thickness of their sides.

But finding it more difficult to show how tones are made by a pipe, where there are no visible vibrations; he considers the frame of a pipe, and the motion of the air in it, and thereby attempts to find the cause of the tone of a pipe, and the pulse that gives the sound, explaining how tones are made in violins, harpsichords, and dulcimers.

To this he subjoins an ingenious discourse on the varying and breaking of tones, endeavouring to explain how it is caused, both in strings and pipes, where occur divers pertinent observations concerning the motion of pendulums, the trumpet marine, and the true trumpet, as also the sackbut. Where he takes notice of the advice of Vitruvius in his architecture, that in the structure of a theatre there should be vases or hollow pots of several sizes, to answer all the notes of music placed on the stage in such manner that the voice of those who sang on the stage might be augmented by their ringing; Vitruvius mentioning divers ancient theatres, where there were some of brass, some of earth.

After this, he descends to the consideration of the nature of keys in music, and of a single tune. Next he treats of schisms and the scale of music.

Having dispatched that work, he proceeds to music that consists of several parts in concert, which is made up of harmony, formality, and conformity. Of which, harmony is the grateful sound produced by the joining of several tones in chord to each other; formality requires that the succeeding notes be agreeable to the former; and conformity, that each part have the like tendency to the succeeding notes. Lastly, he speaks of time, or the measures of music; the due observance of which is grateful, for the same reason given for the formality of a single tune, because the subsequent strokes are measured by the memory of the former, and if they comprehend them, or are comprehended by them, it is alike pleasant; the mind cannot choose but compare one with the other, and observe when the strokes are coincident with the memory of the former.

On the Trembling of Consonant Strings, a new Musical Discovery. By Dr. Wallis. N° 134, p. 839.

SIR.—I have thought fit to notice a discovery that has been made here, about three years since, or more, which I suppose may not be unacceptable to those of the Royal Society, who are musical and mathematical. It is this, whereas it has been long since observed, that if a viol or lute string be touched with the bow or hand, another string on the same or another instrument not far from it, if an unison to it or an octave, or the like, will at the same time tremble of its own accord. The cause of it having been formerly discussed by divers, I do not now inquire into. But add this to the former observation, that not the whole of that other string trembles, but the several parts severally, according as they are unisons to the whole, or the parts of that string which is so struck. For instance, supposing AC (fig. 7, pl. xi) to be an upper octave to $\alpha\gamma$, and therefore an unison to each half of it, stopped at β; now if, while $\alpha\gamma$ is open, AC be struck: the two halves of this other, that is, $\alpha\beta$ and $\beta\gamma$, will both tremble, but not the middle point at β. Which will easily be observed, if a little bit of paper be lightly wrapped about the string $\alpha\gamma$, and removed successively from one end of the string to the other. In like manner if AD (fig. 8) be an upper twelfth to $\alpha\delta$, and consequently an unison to its three parts equally divided in β, γ. Now if, $\alpha\delta$ being open, AD be struck, its three parts, $\alpha\beta$, $\beta\gamma$, $\gamma\delta$, will severally tremble, but not the points β, γ; which may be observed in like manner as the former. In like manner, if AE (fig. 9) be a double octave to $\alpha\varepsilon$; the four quarters of this will tremble, when that is struck, but not the points β, γ, δ. So if AG be a fifth to $\alpha\eta$, (fig. 10); and consequently each half of that stopped in D, an unison to each third part of this stopped in $\gamma\varepsilon$, while that is struck, each part of this will tremble severally, but not the points γ, ε; and while this is struck, each of that will tremble, but not the point D. The like will hold in less concords, but the less remarkably as the number of divisions increases.

This was first of all, that I know of, discovered by Mr. William Noble, a Master of Arts of Merton College, and by him showed to some of our musicians about three years since; and after him by Mr. Thomas Pigot, a Bachelor of Arts, and Fellow of Wadham College; who giving notice of it to some others, found that, unknown to him, the same had been formerly noticed by Mr. Noble, and upon notice from him, by others; and it is now commonly known to our musicians here. I add this further, which I took notice of on occasion of making trial of the other, that the same string as $\alpha\gamma$, being struck in the midst at β, each part being unison to the other, will give no clear sound at all, but very confused. And not only so, which others also have observed, that a string does not sound clear if struck in the middle, but also, if $\alpha\delta$ be struck at β or γ, where one part is an octave to the other; and in like manner, if α ε be struck at β or δ; the one part being a double octave to the other. And so if $\alpha\zeta$ be struck in γ or δ, (fig. 11) the one part being a fifth to the other, and so in other like consonant divisions; but still the less remarkable as the number of divisions increases. This and the former I judge to depend upon one and the same cause, viz. the contemporary vibrations of the several unison parts, which make the one tremble at the motion of the other; but when struck at the respective points of divisions, the sound is incongruous, by reason that the point is disturbed which should be at rest.

A lute or viol-string will thus answer, not only to a consonant string on the same or a neighbouring lute or viol, but to a consonant note in wind-instruments, which was particularly tried on a viol, answering to the consonant notes on a chamber-organ, very remarkably, but not so remarkably to the wire-strings of a harpsichord. Which, whether it were because of the different texture in metal-strings from that of gut-strings, or, which I rather think, because the metal-strings, though they give to the air as smart a stroke, yet not so diffusive as the other, I list not to dispute. But wind-instruments give to the air as communicative a concussion, if not more, than that of gut-strings. And we feel the wainscot seats on which we sit or lean to tremble constantly at certain notes on the organ or other wind instruments; as well as at the same notes on a bass-viol. I have heard also of a thin fine Venice glass cracked with the strong and lasting sound of a trumpet or cornet, near it, sounding an unison or a consonant note to that of the tone or ting of the glass. And I do not judge he thing very unlikely, though I have not had the opportunity of making the trial.

An Improvement of the Bononian Stone shining in the dark.

Signor Malpighi, in a late letter of his to the editor, of the 9th of March, takes notice, that one Signor Zagonius had a way of making out of the Bononian stone calcined, statues and pictures variously shining in the dark. But he adds, that unfortunately that person lately died, without discovering to any one his method of preparing the said stone.*

An Extract of a Letter written from Aberdeen, Feb. 17, $167\frac{6}{7}$, by Mr. George Garden. Concerning a Man of a strange Imitating Nature, as also of several human Calculi of an unusual Size. N° 134, p. 842.

I remember when Mr. Scougall and I were with you last summer, we had occasion to speak of a man in this country very remarkable for something peculiar in his temper, that inclines him to imitate unawares all the gestures and motions of those with whom he converses. We then had never seen him ourselves. Since our return we were together at Strachbogie where he dwells, and notwithstanding all we had heard of him before, were somewhat surprized with the oddness of this dotterel quality. This person, named Donald Monro, being a little old and very plain man, of a thin slender body, has been subject to this infirmity, as he told us, from his very infancy. He is very loath to have it observed, and therefore casts down his eyes when he walks in the streets, and turns them aside when he is in company. We had made several trials before he perceived our design; and afterward had much to do to make him stay. We caressed him as much as we could, and had then the opportunity to observe, that he imitated not only the scratching of the head, but also the wringing of the hands, wiping of the nose, stretching forth of the arms, &c. And we needed not strain compliment to persuade him to be covered; for he still put off and on as he saw us do, and all this with so much exactness, and yet with such a natural and unaffected air that we could not so much as suspect he did it

* It has been already mentioned in a note at p. 139, volume i. of this Abridgement, that the method of preparing the Bononian stone for shining in the dark, is no longer a secret.

on design. When we held both his hands, and caused another to make such motions, he pressed to get free: but, when we would have known more particularly how he found himself affected, he could only give us this simple answer, that it vexed his heart and his brain.

I took occasion lately to visit a poor woman in the neighbouring parish, who has been long afflicted with the gravel, and has passed four stones of an unusual size; of which I have one by me, which though it be not the largest of the 4, is yet more than 5 inches about the one way, and 4 the other. They are all oval; the first and a part of the second were smooth; but the other two very rough; and the last the largest, which came away about Christmas last, was bloody on one side when I saw it. This puts me in mind of that stone of a prodigious size, which was found last year in a gentleman's bladder in this country, after his decease, weighing 32 ounces.

M. Leewenhoeck's manner of observing the Animalcula in divers Sorts of Water. N° 134, p. 844.

I thus order my division of the water, and the enumeration of the animalcula: I suppose that a drop of water equals a pea in bulk; and I take a little quantity of water of a round figure, as large as a millet-grain; this I reckon to be the $\frac{1}{91}$ part of a pea: for when the axis of a millet-seed makes 1, that of a pea makes $4\frac{1}{2}$: whence it follows, that the grain of a millet is at least the $\frac{1}{91}$ part of a pea, according to the received rules of mathematicians.* This small quantity of water I gather up into a very slender glass-pipe, dividing by this means that little water into 25 or 30 parts, of which I observe one part after another, and show the same to others.

Other spectators, as well as myself, judged that in $\frac{1}{30}$ part of the water, equalling the bulk of a millet-seed, he saw more than 1000 living animals: but they wondered much more, when I said I saw in it two or three kinds of much smaller animals besides, which did not appear to them, because I saw them by another microscope, which I still reserve to myself alone. Hence it is manifest, that if in the $\frac{1}{30}$ part of one millet-seed there are seen 1000, there may be seen 30,000 in one such whole seed, and consequently in a drop of water, which is 91 times larger than such a seed, there may be seen 2,730,000. For, $4\frac{1}{2} \times 4\frac{1}{2} \times 4\frac{1}{2} = 91\frac{1}{8}$; and $91 \times 30,000 = 2,730,000$.

Otherwise, I compare the quantity of the water to the bulk of a grain of sand; in which quantity of water I doubt not at all that I see more than 1000 animalcula. Now if the axis of a grain of sand be 1, the axis of a drop of water

* That is, by cubing the diameters.

is at least 10, and consequently a drop is a 1000 times larger than that sand, and therefore 1,000,000 living creatures in one drop of water. In which computation I rather lessen than heighten the number.

These, Sir, I thought good to add to the observations I have made, and showed to others, with the applause of the beholders. The rest, and the make of my microscopes I cannot yet communicate. After I had sent away my former letter, I gave not over observing the animalcula in water; examining also distilled and boiled waters. Last winter, when the severe cold had killed the little creatures, observing the water thawed by the warmth of the room in which it had stood for a whole day with a fire in it, I found after 24 hours were elapsed, and another time after 17 hours were passed, that some living animals appeared again in that water.

Continuation of the Hortulan and Rural Advertisements. By Dr. John Beale. N° 134, p. 846.

Omitted, for the same reason as before.

An Account of Observations made for several Years together, concerning three New Stars, one in the Whale's Neck, the other two near the Head and Breast of the Swan. By M. Hevelius. N° 134, p. 853. *Translated from the Latin.*

It is well known that the new star in the neck of the Whale has been continually observed, from the year 1638 to the year 1662, and in the same place in the heavens; but not always of the same magnitude, nor shining with the same lustre; but in some years disappearing, and again shining out, sometimes sooner, sometimes later, without observing any certain periods of time: as fully appears in the short history of that wonderful star, published in 1662, with my account of Mercury seen in the sun, p. 164. But in the subsequent years, especially from 1665 to the present time, that star has not been so diligently explored. And therefore, for continuing that short history, I have added the following table, exhibiting the appearances and returns of that star: by which it may be known at once, its beginning and ending, how it has increased and decreased, when it disappeared, and when it shone out again. You will there see that the said new star in Collo Ceti appeared in every year, till Oct. 1672, though as it were with a different phase: but that after that it never appeared again for an interval of 4 years, viz. from about Oct. 1672, to Dec. 1676, as I may say that I sought for it with watchful eyes, every time that I made observations in clear nights.

Therefore, by how much the less it has been observed by the ancients, that the fixed stars have suffered such wonderful changes, and that continually for the space of so many years; again for so many years hid, that these could never have been discovered without a telescope; so much the more ought they to be noted; that we may excite posterity, by the sedulous watching of such phænomena, to search more and more in the great works of creation. It is true several new stars have been observed by our predecessors; but none of this kind that I remember, except those two which have appeared in our own ages, viz. that in the breast of the Swan, discovered by Kepler, I think in the year 1601, and that other under the head of the Swan which appeared in 1672. Of these, what in like manner I have of late years observed, will fully appear by the following ephemeris: viz. that the star in the breast of the Swan, which from about the year 1662 certainly disappeared, was seen to appear again in clear nights in 1665; so that the following year 1666, it could be observed again with the sextant as a very minute star; and from that time gradually increasing, but without arriving at its former magnitude, being of the third order, or its former splendor, as it appeared in the years 1657, 1658, and 1659: and now while I at present write it only shines as a star of the 6th magnitude. And that other below the head of the Swan, which I first saw in the summer of the year 1670, came to appear as a star of the third magnitude in the months of October and November, after which it sensibly decreased in lustre and magnitude, till it vanished; yet it returned again the following year 1671, in the month of April, and was visible all the summer, although with a different phase, and so decreased till the month of March 1672, when it could no longer be seen, though I have often with diligence looked for it. From whence astronomers will clearly perceive what has occurred with regard to those new stars for the 12 years elapsed; but as to what may happen hereafter, that must be learned from the observations of future years.

Dantzic, Jan. 2, *N. S.* 1677.

The Ephemeris of the New Stars.

An. 1665, Nov. 28.—The new star in the Swan's breast, which for some time, from 1662, was quite hid, seemed in a clear sky as it were reviving.

An. 1666, Sept. 21.—The new star in Collo Ceti no where appeared; but that in the Swan's breast appeared to the naked eye, even while the moon shone.

Sept. 24.—The new star before the Swan's breast was less than the three preceding in the neck, and hardly seemed of the sixth magnitude.

An. 1667, Jan. 7.—The new star in the Whale's neck did not yet appear.

Jan. 13.—Neither does it yet appear.

Feb. 2.—It first appeared very bright, being equal to that in the mouth of the Whale, or to that in Nodo Lini.

Feb. 7.—It was still equal to that in the Whale's mouth.

Feb. 10.—It appeared extremely bright.

Feb. 27.—It yet appeared very bright, even where the moon shone; and was larger than that in the Whale's mouth.

March 13.—It still appeared very bright, and nearly of the same magnitude.

An. 1668, Oct. 26.—The new star in Collo Ceti first appeared, but only like a very minute star.

Nov. 7.—It was almost equal to the middle star in the Whale's mouth.

Nov. 16.—It was nearly equal to that in the mouth.

An. 1669, Jan. 28.—It was less than that in the mouth.

Sept. 26.—It appeared only like a star of the sixth magnitude.

Oct. 16.—It was greater and brighter than that in the mouth.

Oct. 24.—It was equal to the bright star in the jaw.

Nov. 19.—It was greater than that in the mouth, but less than that in the jaw.

An. 1670, Aug. 27.—It shone exceedingly bright, being nearly equal to the stars of the second magnitude, and to that in the jaw.

Sept. 3.—It still shone very bright; and the other in the breast of the Swan seemed to increase.

Sept. 8.—It was still equal to that in the jaw. But the other under the head of the Swan appeared evidently to decrease; so that it seemed to me hardly greater than the superior of the two unformed stars preceding the Swan's head, that is of the fifth magnitude. But that in the breast of the Swan appeared yet a little to increase.

Oct. 13.—The new star under the Swan's head scarcely at all appeared, as also the head of the Swan, while the new star in the breast appeared pretty bright.

Oct. 14.—The new star under the Swan's head was very faint, and scarcely at all to be seen.

Dec. 5.—The new star in Collo Ceti is still decreased, so as hardly to equal a star of the sixth magnitude.

An. 1671, April 29.—The new star under the Swan's head, seen again of the third magnitude, exceeded that in the Swan's beak, and almost equalled that in the lower wing; but rather less than that in the breast, except that its light was more dull. But the other, in the breast, appeared yet hardly larger than in the year past; so that it equalled only a star of the sixth magnitude.

May 17.—It seemed rather less than that in the beak, and that in the shoulder of the Eagle, as also duller in its light; but greater than that in the point of Sagitta, and almost equal to the following star in the neck of the Harp.

May 25.—It seemed less than on the 29th of April, when it was first seen; so that it seems decreasing. It is less than that in the Swan's beak, or that in the bend of the southern wing, also less than those in the neck of the Harp and the Eagle's shoulder; hardly appearing larger indeed than the less of the two in the Swan's foot, and that in the Eagle's breast.

June 26.—It appeared less than that in the Swan's neck; so that it decreases remarkably. But that other before the Swan's breast, seemed rather larger than last year.

July 3.—The same new star under the Swan's head was rather less than that in the neck.

July 18.—It now hardly equals the stars of the fifth magnitude.

August 2.—It hardly now appeared of the sixth magnitude, being less than all the other stars about the neck and head of the Swan, and only twinkling now and then.

Aug. 6, 7, 12, 14, 15, 16, 17, 25.—It decreased more and more, so as to be scarcely perceived these days, till on the last it was quite invisible. But that other in the neck of the Whale was equal to the star in the cheek, or even larger.

Sept. 11, 12.—The former was no longer to be seen; but that in the neck of the Whale was equal to that in the mouth, viz. of the fourth magnitude.

Oct. 30.—The new star in the Whale's neck scarcely appeared of the sixth magnitude.

Nov. 3.—The same star no longer appeared.

An. 1672, March 29.—The new star under the Swan's head appeared, though hardly equal to the sixth magnitude. But that other in the breast seemed rather increasing.

August 9.—The new star in the neck of the Whale shone with brilliant rays, being greater than that in the mouth, though less than that in the jaw. But that under the Swan's head shone no more this year.

Sept. 17.—That in the Whale's neck was less than that in the cheek, being less than the fourth, or even the fifth magnitude.

Sept. 25.—It is now scarcely of the sixth magnitude.

Oct. 9.—It no longer appeared; and so it continued invisible all the time to Dec. 23, 1676, though I looked for it very attentively, whenever I made observations in a clear night.

An. 1675, July 22.—The new star in the Swan's breast shines again at nights, but only as a star of the sixth magnitude.

An. 1676, Dec. 10.—I well remember that I could not see the new star in Collo Ceti, though I observed many small stars in the same part of the heavens.

Dec. 23.—The sky being very clear, we saw very plainly the new star in the neck of the whale. It shone with such brightness and was so large, that it not only equalled, but even exceeded that in the Whale's jaw.

Dec. 31. It was again observed larger than that in the jaw, that is, of the second magnitude.

An. 1677.—Jan. 1. It again shone out very brightly, rather larger than the star in the jaw, than that in the extremity of the wing of Pegasus and Marcab, in colour and light not unlike to the jaw. Yet I remember to have formerly observed, when it was of the second magnitude, that it was rather whiter and brighter.

An Account of two Books. N° 134, p. 859.

I. Traité de la Percussion ou Choc des Corps, &c. par Monsieur Mariotte, de l'Academie Royale des Sciences. A Par. 1673, in 12°.

In this work are treated the effects resulting from the shock of bodies, both elastic and non-elastic: in which the author confirms the reasonings of some preceding philosophers, and delivers some new ones of his own.

From several reasons and experiments by him delivered, he concludes, that the greatest part of hard bodies, as steel, marble, glass, ivory, jasper, &c. have a ready and strong springy power; and that all the motions of reflecting bodies are only made by springs. Whereunto he adds, that if it should be supposed that hard bodies are inflexible, it would be impossible to explain their motions when their weights are unequal, and that the phænomena do no ways agree to such an hypothesis. But taking it for a mere hypothesis, what he pretends to have demonstrated concerning the springiness of hard bodies, he tells us, that by that means all motions befalling those bodies, after they have any way impelled one another, may easily be accounted for.

II. Johannis Trithemii Steganographia, vindicata, reserata, et illustrata, &c. Auth. Wolfgango Ernesto Heidel, Wormatiensi. Mogun. 1676, in 4to.

This Steganography, or the art of signifying ones mind to another by an occult or secret way of writing, having been censured as supposititious by some, and pernicious, magical and necromantical by others; this author undertakes to vindicate it from those imputations, and to give the true key and meaning thereof.

Of an unusual Meteor. By Dr. Wallis. N° 135, p. 863.

Sir,—I know not whether in your transactions you have any where taken notice of that unusual meteor, which happened on Wednesday Sept. 20th, 1676, about 7 o'clock at night, or soon after; which, though it seemed very low, was seen in most parts of England much at the same time, and much in the same manner. I hear of it from divers persons who saw it in Oxford, Northamptonshire, Gloucestershire, Worcestershire, Somersetshire, Hampshire, Kent, Essex, London, &c. Some here call it a draco volans. I have sometimes been fancying it might be higher than they imagined, only casting a light so low. And if I had heard any thing from it abroad, should have inclined to think it a comet, passing swiftly by us, very near the earth, even through our air. But if it had been so, it must be a very small one, otherwise we should have heard more of it.

In the dusk of that evening, there appeared a sudden light, equal to that of noon-day; so that the smallest pin or straw might be seen lying on the ground. And above in the air was seen, at no great distance as was supposed, a long appearance as of fire; like a long arm, with a great knob at the end of it, shooting along very swiftly: and, at its disappearing, seemed to break into small sparks or parcels of fire, like as rockets and such artificial fire-works in the air usually do. It was so surprising and of so short continuance, that it was scarcely seen by any who did not then happen to be abroad: its duration, by report, less than half a minute. It seems surprising that it was seen in most parts of England, and at or near the same time: which argues, that either it was higher than the observers imagined, or else that it had a very swift motion. This made me conjecture that it might be some small comet, whose linea trajectoria passed very near our earth, or upon it. That comet which has now appeared, in this and the last month, confirms me in the same opinion; which I conjecture may be the very same which passed by us in September last. Which way its motion was when near us, I cannot conclude, so as to satisfy myself. For most that saw it, being suddenly surprized, took little more notice of it, than that it suddenly appeared and was suddenly gone, but saw it so little time as scarcely to mark which way. My conjecture on the whole, though perhaps but a conjecture, has at least so much of probability in it, as to deserve some consideration: and may serve, if true, to give us some light into the nature of comets; which perhaps will seldom have been found to come so near us, as this seems to have done.

On four Sorts of factitious Shining Substances. N° 135, p. 867.

Two of these four substances have been already noticed in two of the late Transactions, viz. N° 131, and N° 134; one of them being the factitious paste of Dr. Baldwin, shining in the dark like a glowing coal, after it has been a while exposed to the day or candle-light; the other, the Bononian stone calcined, which imbibes light from the sun-beams, and so renders it again in the dark, whereas the former needs no sun-shining. To these we shall now add two other sorts. The one is by the Germans called phosphorus smaragdinus, said to be of this nature, that it collects its light not so much from the sun-beams, or the illuminated air, as from the fire itself; seeing that, if some of it be laid on a silver or copper-plate, under which are put some live coals, or a lighted taper, it will presently shine; and if the same matter be shaped into letters, they may be read.* The other is called phosphorus fulgurans, which is made both in a liquid and dry form; and not only shines in the dark, and communicates a sudden light to such bodies as it is rubbed on, but being included in a glass-vessel well closed, it now and then fulgurates, and sometimes rises as it were into waves of light. It is further said, that a little portion of it having been kept two whole years, has not yet lost its power of shining: so that it is believed, if a considerable piece of it were prepared, it would serve for a perpetual, or at least a very long lasting light.

Observations of the late Comet, made at Paris by Signor Cassini. N° 135, p. 868. *Translated from the Latin.*

M. Romer first observed the new comet, April 28, N. S. 1677; and, having presently informed me of it, at $4^h\ 6^m\ 31^s$ after midnight, we took its altitude 12° 22′ 10″. I judged it to be in a vertical, declining about 33° from the east towards the north.—On the 29th it was seen by M. Picard for a moment through the clouds, at $3^h\ 9^m\ 31^s$ after midnight, at an altitude of 4° 39′.

May 2d in the morning, the right ascension of the mid-heaven by the fixed stars being 267°, the altitude of the comet was 4° 5′: the distance of the vertical about 42° 8′ from the north towards the east.—May 4, at $3^h\ 30^m$, the comet's altitude was 5° 33′; the distance of the azimuthal from the north towards the east about 42° 32′.—May 5, at $3^h\ 32^m$, the comet's altitude was 5° 10′; the azimuthal distance from the north to the east about 44° 10′.

* The substances here enumerated make but a very small part indeed of the number of mineral bodies which, under certain circumstances, become luminous in the dark. See Mr. Thomas Wedgewood's experiments on the production of light from different bodies by heat and attrition, in the 82d volume of the Philosophical Transactions.

Afterward the cloudy time, both morning and evening, prevented any further observations of the comet.

By these observations it appears that the comet at first was in the triangle, afterwards near the head of Medusa; and showing that it proceeded according to the order of the signs by a line that was very near, and almost parallel to, that which was described by the comet in Feb. 1590. The magnitude of its head, seen through the telescope, was almost equal to the disk of Jupiter, or somewhat less. It did not appear perfectly round, but of an oval figure, the longer diameter being parallel to the horizon; which seemed to be owing to the horizontal refraction. Its coma, seen through the telescope, was broad, and nearly parabolical; but to the naked eye it seemed narrow, and a little inflected towards the west.

The same Comet observed at Dantzic, by M. Hevelius. N° 135, p. 869, 871. *Translated from the Latin.*

A comet has lately appeared which was first observed here at Dantzic, April 27, in the morning. On the 28th it could not be seen for the clouds. But on the 29th it arose, or rather appeared to sight, at 1^h 52^m in the direction north-east by north. Its head was not large, but yet bright, consisting of one shining nucleus, like that of the year 1665. It stretched out a tail pretty luminous, with divaricating rays turned upwards, near 2° in length. The line of direction of the tail produced passed between Alamac, the bright foot of Andromeda, and her girdle, bisecting the distance between these two stars. It was then above the head of Aries in the triangle, between the apex and the northern star in the base, viz. in 5° of Taurus, and in 19° of north latitude. At the same time its distance from the sun, according to longitude, was only 5°, but in its own great circle 20°. Since the comet was then so near the sun, it could not show a longer tail, though I believe it had one much longer, and in a few days it will probably appear still shorter.

April 30, it was found in 9° of Taurus, and 18° north latitude, and nearly as far from the sun, being in 12° of Taurus. It spread its tail again 2° or more, to the northern star in the base of the triangle, which star could plainly be seen with good tubes at the end of the tail.—May 1, at 2^h 32^m in the morning, it was found in 11° of Taurus, with 18″ north latitude, nearly in conjunction with the sun, from which it was distant as many degrees. Its tail was yet pretty bright, but a little shorter, though broader, which it stretched out to the bright foot of Andromeda. From April 29, when I first saw it, to this day May 1, it passed over nearly 5° 30′ in its own direction.

As far as I can collect from these observations, it moves directly towards the left foot of Perseus, above Taurus, to the feet of Gemini, if it so long continue. The descending node may be about 20° of Gemini, by conjecture, where it will pass the ecliptic to the southward, with an inclination of its orbit of near 27°.

May 2, in the evening, at 8^h 45^m, though no stars appeared in that part of the heavens, and there was a strong twilight, yet I presently found the comet with my optical tube. A little after, its altitude was 3° 30′. On account of the twilight the tail appeared very thin, which was extended between the knees of Cassiopea, but nearer to the left. It went down about 10 that evening, in the north-north-west.

May 3, in the morning, the comet rose at N. N. E, at 1^h 23^m, though we saw the tail a little sooner, viz. at 1^h 18^m. It was in 14° of ♉, nearly in conjunction with the sun, having 17° latitude, and nearly the same distance from the sun. This day the tail was longer, extending near 3 degrees, very bright and well defined. Thus we saw it with the naked eyes at 3^h 34^m, and with the telescope at 3^h 40^m, at an altitude of 11° 30^m; so that the sun at that time was depressed only 6° below the horizon; and we had seen it longer, but for some small intervening clouds. Its daily motion seemed to decrease, as far as I could judge by conjecture, without a calculation. For between the 29th and 30th of April it moved near 2° 45′; between April 30 and May 1, but 2° 15′; between May 1 and 2, but 1° 55′; and between May 2 and 3, only 1° 40′. But this will be seen plainer from the observations themselves and the calculation.—On May 4, in the evening, the air being very pure at 8^h 53^m, the comet was seen, but a little obscurer than the former days, and the tail shorter.—On May 5, at 1^h 41^m in the morning, it was seen with the tail directed towards the right knee of Cassiopea, its longitude 17° of Taurus, with 16° north latitude, and at the same distance from the sun. Its proper motion from the 3d to the 5th of May was nearly 2° 40′, the latitude decreased, viz. from its beginning near 3°; so that the proper motion of the comet, from April 29 to May 5, was 12°.—May 6, in the morning, its place was in 18° of ♉, with 15° 30′ north latitude, the sun being then in 17° of ♉; the daily motion then about 50′. The head at that time seemed thinner and weaker than the tail, as the sun was only about 16½ degrees distant.—May 6, in the evening, it was seen with the optic tube at 8^h 35^m, with the tail shorter and thinner; but its situation below, and the twilight strong, the comet could not be seen with the naked eye.

On May 7, the comet was first seen at 2^h 22^m, at 3° altitude, so that it appeared very thin; its place was then 19° of ♉, and 15° north latitude; its distance from the sun 16°, the sun being in 18° of ♉; its proper motion decreasing more and more, as far as could be known without a calculation.—May 8, from one o'clock in the morning it was sought for in vain with the naked eye, but was discovered with the 12-foot telescope, having still a tail, though very short, extending a little to the left hand from the vertical circle. As near as could be guessed, it was in 20° of ♉, at the distance of 15° from the sun, which was then in 19° of ♉; the comet was then nearly in a line with the right shoulder of Perseus, and Algol of Medusa, its diameter being hardly equal to the half of Jupiter's. It was still conspicuous enough by the tube, so that at 3^h 45^m we could distinctly perceive it, at near 9° altitude; whence it may be collected that the arch of vision was scarcely then 5°; for the sun was hardly 5° below the horizon, and all the stars had disappeared, excepting the planet Jupiter only; and the sun rose at 4^h 6^m.—May 8, in the evening, the comet could no longer be seen, either with the naked eye or with any telescope, nor yet on any day afterwards.

Mr. Flamsteed's Account of his Observations of the late Comet from Greenwich, May 18, 1677. N° 135, p. 873.

The first certain notice I had of the comet was on April 21, I waited the rising of it; but immediately after midnight the heavens were overspread with clouds, and continued so till sun-rise next morning, preventing me of my desires. The next night, April 22, I again waited for its rising, the heavens being now exceedingly serene and clear, at about 2 o'clock after the midnight following, I saw the tail raised almost perpendicular to the horizon; soon after the head appeared through a thin vapour, from which the tail pointed, as near as I could guess, on the star in the knee of Cassiopea, its length being about 6 degrees, and breadth at the top about 7 or 8 minutes. Viewing the head with a telescope of 16 feet, I found it was not perfectly round, but indented, and not near 1 minute diameter. Afterwards I hasted to measure its distances from several fixed stars, which were as follow: April 22.

14^h	44^m	0^s	its head and the foot of Androm. Alam.	11°	26′	0″
	47	15	that distance repeated	11	26	50
	55	3	its head from Capella	31	1	15
	59	10	the same repeated	31	1	24
15	12	2	its head from Algol in Medusa's	8	16	54
	21	22	Mirach	19	35	0
	27	54	Almech again	11	33	30
15	36	20	Capella again	30	59	45

At 15^h $21\frac{1}{2}^m$ p.m. the height of the comet was about $5\frac{1}{2}°$; therefore the distance of the head of the comet from Algol correct by refraction was 8° 19′, from Mirach, 19° 37′.

And admitting with Mr. Hevelius the place of Mirach now in ♈ 21° 40′ 34″, with north latitude 25° 57′, its distance from Algol will be 23° 42′ 40″, and the place of the head of the comet in ♉ 14° 48⅙′, with north latitude 17° 8′.

At 15^h 28^m I state the correct distance of the comet's head from Capella 31°, from Alameck 11° 40′; and therefore its true place in ♉ 14° 50½′, with north latitude 17° 6′ 25″; agreeing very well with the place derived from the former distances from two other and different stars.

The tail was not, it seems, directly opposite to the sun; for the sun's place was now ♉ 30° 7′; but the comet being in 14° 47′ of the same sign, that is 1° 40′ in consequence of the sun, the tail ought, if it had been exactly opposite to the sun, to have lain in consequence of the head; but the knee of Cassiopea is now in ♉ 13° 24′ in antecedence of the comet, whose tail lay not therefore in consequence, but in antecedence of the line passing through its head and the sun, at about an angle of 10°.

Next night, being that following the 23d of April, about ¾ of an hour after 2 I saw its tail, which appeared much shorter than last morning through a break of the clouds; which soon after opening wider I saw the head too, and hasting I measured its distance. April 23 at 14^h 51^m p. m. from Mirach 21° 9′; but before I could get the plane of the sextant to Algol, the clouds came over the comet again, and I could see it no more.

Hence, and from a coarse observation of it sent me by an ingenious friend, I found its motion was direct, and its latitude decreasing.

An Account of some Books. N° 135, p. 875.

I. The Natural History of Oxfordshire, being an Essay toward the Natural History of England. By Robert Plot,* LL.D. Printed at the Theatre in Oxford, 1677, in fol.

* Robert Plot, author of the well-known History of Oxfordshire, &c. was, according to the authors of the General Biographical Dictionary, 8vo. born of a genteel family, in 1641, at Sutton-Barn, in Kent, and educated at the Free-school of Wye, in the same county. In 1658 he went to Magdalen Hall in Oxford; took the degree of Bachelor of Arts in 1661, and that of Master of Arts in 1664, and both the degrees in law in 1671. He afterwards removed to University College. Being a very ingenious man, and particularly attached to natural history, he was elected a Fellow of the Royal Society, and in 1662 was chosen one of the secretaries. He published the Philosophical Transactions from No. 143 to No. 166 inclusive. In 1683, Elias Ashmole, Esq. appointed him the first keeper of his museum; and about the same time he was nominated by the vice-chancellor the first professor of chemistry in that university. In 1687 he was made secretary to the Earl-marshal, or Court of Chivalry, which was then renewed, after it had lain dormant since the year 1641. In

The worthy and learned author of this work having very generously undertaken to make a fuller and stricter survey of the natural and artificial things of England than has been made hitherto, and being induced to this undertaking by the consideration of advancing thereby both the knowledge of nature, and the business of trade; has begun to execute this noble design by giving us a very particular account of what occurred to him, for the most part upon his own personal inquiry, in Oxfordshire. An attempt so considerable, that if it were pursued by fit persons all over the world with care, judgment, and diligence, would in time produce a just history of nature, and furnish both the philosopher with good materials to work with, and generally all sorts of men with the pleasure and useful knowledge of the riches and wonders of the world.

But Dr. Plot's work is too well known to require the reprinting of the very minute account of it first given in the Philosophical Transactions.

II. L' Architecture Navale, avec le Routier des Indes Orientales et Occidentales. Par le Sieur Dassié. A Par. 1677, in 4to.

The author of this book desires it to be considered as a small essay or forerunner of abundance of excellent researches of his curiosity, which he says, he is preparing for the public. His main design in this work he affirms to have been no other than to reduce into art, as methodically as he could, a science so necessary and useful to the state, to render it familiar, and to quicken those that are skilled in the mathematics and in naval architecture, to inquire after infallible ways of making ships sail better, and to find out the just weight of a ship's burden, and its true symmetry, and so to bring this art to perfection.

III. Philosophical Dialogues concerning the Principles of Natural Bodies. By W. Simpson, M.D. Lond. 1677.

A work replete with false theories and reasonings.

IV. A New Treatise of Chemistry, &c. written in French by Christopher Glaser,* and now faithfully translated by F.R.S. Lond. 1677, in 8vo.

1688 he received the title of historiographer to James the Second. In 1690 he resigned his professorship of chemistry, and also his place of keeper of the museum, to which he presented a large collection of natural curiosities, being such as he had figured and described in his Histories of Staffordshire and Oxfordshire, and those distinguished by the title of Scrinium Plotianum Staffordiense. In 1694-5, Henry Howard, Earl-marshal, nominated him Mowbray herald extraordinary, and two days after he was constituted registrar of the court of honour. He died of the stone in April 1696, leaving two sons by a wife whom he had married in 1690. His principal works are the Histories of Oxfordshire and Staffordshire, both in folio. He was also the author of several other learned and ingenious productions, and left several manuscripts behind him, among which were large materials for the natural history of Kent, Middlesex, and in particular of the City of London.

* Christian Glaser, a native of Basil, was held in considerable estimation as a chemist in the 17th century. He settled at Paris, and became apothecary to Lewis XIV. and to the Duke of Orleans,

This author here publishes a short and easy method for the happy attainment of all the most necessary preparations of chemistry.

As for the theory he speaks succinctly, yet seems to say so much of it as may suffice for direction to the preparations; performing his operations on minerals, vegetables, and animals.

On a Cheap and Useful Pump. By Mr. John Conyers. N° 136, p. 888.

Here, AA (fig. 12, pl. xi) is the body of the pump, made of oak, elm, or deal planks; with a valve at bottom *aa*. BB the bucket, in the midst of which there is a valve *b*, not visible in the figure, being concealed by the sides of the leather *bb*. CCC the iron to raise the bucket. DD the wood at the bottom of the bucket containing the valve. EE the handle for raising the bucket, to be managed by fewer hands than ordinary pumps are; which may be altered so as to employ a horse or mill, or other such like way more advantageous than that of this handle managed by the strength of men. FF a square taper-box, with holes in the sides, and open at the bottom; into the narrower part of which is inclosed the narrower end of the body of the pump. GG an additional bucket of a larger dimension, to be placed in the iron work of the pump about H, when it shall be needful to lengthen the taper of the pump, and so to raise the water more forcibly to a greater height. II the spout of the pump, to cast out the water of the same breadth with the side of the pump, at the place represented by the figure. KK the iron or wooden work set off or bent back, if needful, and placed at the back of this pump, for the easier and more capacious motion of the pump-handle, in which it moves.

This pump, which was used at the new canal of Fleet river, was 8 feet long, and 1 foot 8 inches broad at the top, and about 8 inches broad at the bottom, where it is inserted in the box, and threw out 8 gallons at a stroke; and 21 strokes being made in 1 minute, there was delivered about 169 gallons in each minute.

If it be asked, why the pnmp and the bucket is not of the same breadth throughout as high as the bucket moves? I answer, that it cannot be allowed of any other fashion than a tapering one, because the celerity of the motion in the narrowest part of the pump would thereby be obstructed in its supplying the delivery of the water, which is thus provided for the evacuation answering to the size of the upper or broader part of the pump.

That this kind of pump may, by the same contrivance, be made of a tree

The work above-mentioned was translated into English and German, and was deemed one of the best chemical treatises of those days.

bored through with a taper-bore, and a basket may be used at the bottom of the pump instead of the box colander.

*On the Motion of Light. By M. Romer.** N° 136, p. 893.

Philosophers have been endeavouring for many years to decide, by some experiment, whether the action of light be conveyed in an instant to distant places, or whether it requires time. M. Romer of the Royal Academy of Sciences has devised a way for this purpose, taken from the observations of the first satellite of Jupiter, by which he demonstrates, that for the distance of about 3000 leagues, which is nearly equal to the diameter of the earth, light needs not one second of time.

Let A (fig. 13, pl. 11) be the sun, B Jupiter, C the first satellite of Jupiter, which enters into the shadow of Jupiter, to come out of it at D; and let EFGHLK be the earth placed at divers distances from Jupiter.—Now suppose the earth being at L, towards the second quadrature of Jupiter, has seen the first satellite at the time of its emersion or issuing out of the shadow at D; and that about $42\frac{1}{2}$ hours after, viz. after one revolution of this satellite, the earth being in K, see it returned to D; it is manifest, that if the light require time to traverse the interval LK, the satellite will be seen returned later to D, than it would have been if the earth had remained at L; so that the revolution of this satellite being thus observed by the emersions, will be retarded by so much time, as the light shall have taken in passing from L to K; and that on the contrary, in the other quadrature FG, where the earth by approaching goes to

* Olaus Romer, or Roemer, a noted Danish astronomer and mathematician, was born at Arhusen in Jutland, 1644. Having acquired great skill in those sciences, when M. Picard was sent by Louis XIV to make observations in the north, in 1671, he was so much pleased with this young man, that he engaged him to return with him into France, where the king settled a pension on him, and honoured him with the appointment of mathematical preceptor to the Dauphin: he was also joined with Picard and Cassini, in making astronomical observations; and in 1672 he was admitted a Member of the Academy of Sciences. During the ten years he resided at Paris, he acquired great reputation by his discoveries, and among them that above noticed, concerning the progressive motion of light. Yet it is said he complained afterwards, that his coadjutors ran away with the honour of many things that belonged to him. Probably this induced him to return into his own country, which he did in 1681, where he was appointed to the office of mathematician to the King, and astronomical professor, with a large salary. He afterwards was honoured with several other offices of dignity, particularly that of counsellor of state, and burgomaster of Copenhagen. Roemer was preparing to publish the result of his observations when he died, in 1710, at 66 years of age. This loss however was supplied by his pupil, Peter Horrebow, professor of astronomy at Copenhagen, who published them, with his method of observing, in 1735, under the title of Basis Astronomicæ. Several of his pieces were also printed in the Memoirs of the Academy of Sciences at Paris, particularly in vol. i and x of the collection of 1666.

meet the light, the revolutions of the immersions will appear to be shortened by so much as those of the emersions had appeared to be lengthened. And because in 42½ hours, which this satellite very near takes to make one revolution, the distance between the earth and Jupiter in both the quadratures varies at least 210 diameters of the earth, it follows that if for the account of every diameter of the earth there were required a second of time, the light would take 3½ minutes for each of the intervals GF, KL; which would cause near half a quarter of an hour between two revolutions of the first satellite, one observed in FG, and the other in KL; whereas there is not observed any sensible difference.

Yet it does not follow hence that light requires no time. For after M. Romer had examined the thing more nearly, he found that what was not sensible in two revolutions, became very considerable in many being taken together, and that for example, 40 revolutions observed on the side F, might be sensibly shorter than 40 others observed in any place of the Zodiac where Jupiter may be met with; and that in proportion of 22 for the whole interval of HE, which is the double of the interval that is from the earth to the sun.

The necessity of this new equation of the retardment of light is established by all the observations that have been made in the Royal Academy, and in the Observatory, for the space of 8 years; and it has been lately confirmed by the emersion of the first satellite observed at Paris, the 9th of November last, at 5 o'clock, 35^{m} 45^{s} at night, 10 minutes later than it was to be expected, by deducing it from those that had been observed in the month of August, when the earth was much nearer to Jupiter: which M. Romer had predicted to the said academy from the beginning of September.

Of Damps in Mines. By Mr. Roger Moslyn. N° 136, p. 895.

The coal work at Moslyn, in Flintshire, lies in a large parcel of wood-land, which has a great fall to the sea side, which is directly north. The dipping or fall of the several rocks or quarries of stone that are above the coal, and consequently of the coal lying under them, partly crosses the fall of the ground, so that the dipping of it falls within a point or less of due east; and the stratum runs, in some 40, in others 50, or even 60 yards under the level of the sea. This work is on a coal of 5 yards in thickness, and has been worked about 36 or 38 years. When first found it was very full of water, so that it could not be wrought down to the bottom of the coal. But a witchet or cave was driven out in the middle of it on a level, for gaining room to work, and drawing down the spring of water that lies in the coal, to the eye of the pit. In the driving of which witchet, after they had gone a considerable way under ground, and were in want of fresh air, the fire-damp gradually began to breed, and to appear in crevices and slits of the coal, where water had lain before the opening of the mine, with a small blueish flame continually moving, but not out of its first seat, unless the workmen came and held their candle to it, and then being weak, the blaze of the candle would drive it with a sudden hiss away to another crevice, where it would soon after appear, blazing and moving as before. This was the first knowledge of it in this work, which the workmen made but a sport of, and so partly neglected it, till it had got some strength, when one morning the first collier that went down going forwards in the witchet with his candle in hand, the damp presently darted out violently at his candle, when the blast struck the man down, and scorched him so as to disable him for working some time after. Some other small warnings it gave them, which induced them to employ a man on purpose, more resolute than the rest, to go down a while before them every morning, to chase it from place to place, and so to weaken it. His usual manner was, to put on the worst rags he had, and to wet them well in water, then as soon as he came within the danger of it, he fell grovelling down on his belly and so crept forward, holding in one hand a long wand or pole, having fixed at the end of it burning candles, which he reached by degrees towards it; then the damp would fly at them, and if it missed of putting them out, it would quench itself with a blast, and leave an ill-scented smoke behind it. Thus they dealt with it till they had wrought the coal down to the bottom, the water following, and not remaining as before in the body of it among sulphureous and brassy metal that is in some veins of the coal. The fire-damp was not seen or heard of otherwise, till the latter end of the year 1675, which happened as follows:—

After long working of this five yards coal, they discovered on the rising grounds, where the signs of the coal and the coal itself came near the day, that another roach of coal at 14 yards depth beneath the former, which proved to be $3\frac{1}{2}$ yards thick; and a profitable coal, but something more sulphureous than the other, and extended under all the former works. As they sunk the lower part of it many appearances of the fire-damp occurred in watery crevices of the rocks flashing and darting from side to side of the pit, and showing rainbow-colour-like on the surface of the water in the bottom; but on drawing up of the water with buckets, which stirred the air in the pit, it would leave burning, till the colliers at work with their breath and sweat and the smoke of their candles thickened the air in the pit, then it would appear again, and sometimes they lighted their candles in it when they went out; and so in this pit it did no further harm.

But on sinking a pit within the hollows or deads of the upper work, at 16 or

17 yards distance from the first pit; after sinking 6 or 7 yards deep the fire-damp began to appear as formerly, accompanying the workmen still as they sunk, still using the same means as before, sometimes blowing it out with a blast of their mouth, at other times with their candles, or letting it blaze without interruption. But as they sunk down, and the damp got still more and more strength, they found that the want of air perpendicularly from the day was the great cause and encourager of this damp; for the air that followed down into this pit, came down at the first sunk pit, at the forementioned distance, after it had been dispersed over all the old hollows and deads of the former work, that were filled up with noisome vapors, thick smothering fogs, and in some places with the smothering damp itself. Nevertheless they continued sinking to 15 yards deep, plying the work night and day; but on intermission for 48 hours and more on account of Sundays, &c. the damp gaining great strength in the interim, when the workmen went down they could see it flashing and shooting from side to side like sword-blades across each other, that none durst venture to go down into the pit. Upon this they took a pole, and bound candles several times to the end of it, which they no sooner set over the eye of the pit than the damp would fly up with a long sharp flame, and put out the candles, leaving a foul smoke each time behind it. Finding that these things had little effect, they bound some candles to a hook hanging at the rope's end that was used up and down in the pit; when they had lowered down these a little way into the shaft of the pit, the damp came up in a full body, blowing out the candles, dispersing itself about the eye of the pit, and burning the men and their clothes, and throwing them down, making a noise like the roaring of a bull, but louder, and leaving a smoke and very noisome smell behind it. Upon this discouragement these men came up, and made no further trial: after this the water that came from it, being drawn up at the other pit, was found to be blood-warm, if not warmer, and the crevices of the rocks where the damp lodged, were all fired about Candlemas day following. In this juncture there was a cessation of work for three days, and then the steward, thinking to fetch a compass about from the eye of the pit that came from the day, and to bring wind by a secure way along with him, that if it burst again it might be done without danger of men's lives, went down and took two men along with him, which served his turn for this time. He was no sooner down but the rest of the workmen that had wrought there, disdaining to be left behind in such a time of danger, hasted down after them, and one of them, more indiscreet than the rest, went headlong with his candle over the eye of the damp-pit, at which the damp immediately catched, and flew over all the hollows of the work, with a great wind and a continual fire, and a prodigious roaring noise. The men at first appearance

of it had most of them fallen on their faces, and hid themselves as well as they could in the loose sleck or small coal, and under the shelter of posts; yet nevertheless the damp returning out of the hollows, and drawing towards the eye of the pit, it came up with incredible force, the wind and fire tore most of their clothes off their backs, and singed what was left, burning their hair, faces and hands, the blast falling so sharp on their skin, as if they had been whipped with rods; some that had least shelter were carried 15 or 16 yards from their first station, and beaten against the roof of the coal and sides of the posts, and lay afterwards a good while senseless, so that it was long before they could hear or find each other. As it drew up to the day-pit, it caught one of the men along with it that was next the eye, and ascended with such a terrible crack, not unlike but more shrill than a cannon, so that it was heard 15 miles off along with the wind, and such a pillar of smoke as darkened all the sky for a great while. The brow of the hill above the pit was 18 yards high, and on it grew trees 14 or 15 yards long, yet the man's body and other things from the pit were seen above the tops of the highest trees, at least a 100 yards. On this pit stood a horse-engine, of substantial timber and strong iron-work, on which lay a trunk or barrel for winding the rope up and down, of above 1000 pounds weight; it was then in motion, one bucket going down, and the other coming up full of water. This trunk was fastened to the frame with locks and bolts of iron; yet it was thrown up, and carried a good way from the pit; and pieces of it, though bound with iron-hoops and strong nails, blown into the neighbouring woods; as were also the two buckets, and the ends of the rope, after the buckets were blown from them, stood a while upright in the air like pikes, and then came leisurely down. The whole frame of the engine was moved out of its place; and the clothes, caps and hats of those men that escaped, were afterwards found shattered to pieces, and thrown into the woods a great way from the pit. This happened the beginning of February 1675, being a season when other damps are scarcely felt or heard of.

Mr. Leewenhoeck's Letter from Delf, the 14th of May 1677, concerning his Observations on the Carneous Fibres of a Muscle, and the Cortical and Medullary Part of the Brain; also on Moxa and Cotton. N° 136, p. 899.

I took the flesh of a cow; which I cut asunder with a sharp knife, and using a microscope I severed before my eyes the membrane from it; by which I plainly saw that fine membrane or film, in which these carneous fibres lie interwoven, and of which I speak in my former letter; where I say that those membranes are made up of so many filaments or threads, as if with our naked eye we saw the omentum of an animal. Observing these membranes more narrowly, I saw that they wholly and only consist of small threads running through each other; of which some appeared to be 10, 20, and even 50 times thinner than a hair.—Having taken off these membranes from the carneous filaments, I saw very clearly these carneous threads, which in this piece of flesh were as thick as a hair on the hand. Where they lay rather thick on each other, they appeared red; but the thinner they were spread the clearer they showed.

I have used several methods of viewing the particles of these carneous filaments, and have always found that they are composed of globular parts. I have also divided before my eye, into many small parts, very small pieces of these carneous filaments, which pieces were several times smaller than a grain of sand; and I have observed besides, that, when the flesh is fresh and moist, and its globules are pressed or rubbed, they dissolve and run together, in appearance like an oily or thick waterish matter. Which globules appearing so small, that 1,000,000 of them would not make one grain of gravel sand. The general figure of these globules being roundish, but a little compressed, like a multitude of very small blown bladders, lying on a heap.

I have examined also that membrane of the brain, which is called pia mater, and found that it is permeated by very many small veins, besides those which with the naked eye we see on the brain, especially having first separated the thin membrane from the brain, under which I have seen small veins of an admirable and incredible fineness, and as far as I was able to discern they consist of exceedingly thin filaments.—I have further observed, that the said veins which thus run through the thin membrane, disseminate their ramifications through the brain, after the manner as vines lying upon the earth shoot roots into the ground; imagining the brain to be like the earth, and the veins like the roots in it.

Proceeding to the parts of the brain itself, I must still say of them that they consist of no other parts but globules; but where the brain lay spread very thin, cut through with a knife, as if they had been separated from each other, there they appeared like a very clear matter resembling oil. Continuing my observations, not only of the brains of beasts but also of fishes, and particularly of a cod-fish, and representing it very plainly to my eye, I saw that the said oleaginous matter had not been caused by the knife, but was a matter by itself, wherein the aforesaid globules lay. I saw moreover, but most plainly in the brain of a cod-fish, that the said oleous matter consisted of yet much smaller globules than the other.—The former or larger globules of the brain I judged about the size of those which I formerly said the blood was made up of, which render the blood red. And these greater globules which compose the brain, are very irregular in respect of what those of the blood are.

I remember that having formerly observed the brain of a duck, I then judged that the appearance was caused only by the close union which the globules had to each other, and which changed into threads by a little stretching. But continuing my observations for almost a whole month together, I have seen plainly the very great number of exceedingly small veins running through the brain; of which I could not at first assure myself in the brains of beasts that they were indeed veins, because they are difficult to discern: but coming to observe the brains of cod-fish, I plainly saw those numerous vessels or veins which were very clear, disseminating themselves by their small branchings, and being 15 or 20 times finer than a single thread of a silkworm. These small vessels or veins I have seen in great numbers in a part of the brain not larger than a grain of sand: besides I saw vessels filled with blood, or appearing red; as also vessels that had the thickness of a single thread of a silkworm and very clear. Pursuing these observations about the brains of beasts, I was able very plainly to represent to myself the vessels abovementioned; and I could not without great admiration behold them, partly by reason of their great number, partly of their extraordinary subtlety; so that if one of the red blood globules were divided into 8 parts, and were of a firm substance, it could not pass any of these small vessels. And the oftener I repeated my observations the plainer I could see those manifold little vessels with their ramuscles, which were all very feeble, and by the least touch broke asunder.

Among the said globules, of which the brain partly consists, I have seen blood globules, which may plainly be discerned from the brain-globules, especially by the perfect roundness which the blood globules had. These blood globules I imagined came out of the sanguineous vessels which run through the brain, and had been cut in pieces by the knife.

Between the cortical and medullary part of the brain I can see little or no difference, especially when I represent them before me very thin: only this I noted, that the little veins or vessels which ran through the cortex were of a dark and brown colour, whereas those in the medulla were clearer and more transparent.

I have seen in the brain, and most in the cortical part, such small sanguineous red vessels, which came out of larger ones, that I cannot comprehend how the globules could pass through them; and when we see the blood globules single, they have little or no colour, whereas on the contrary the blood in these small veins was yet red: and the red colour even penetrated through the veins, and coloured the neighbouring parts of the brain red.

I have also observed the spinal marrow of a calf, pullet, sheep, and cod-fish; which I have found to consist of no other parts than those of the brain; yet with this difference, that besides the globules in the brain, there lay in the spinal marrow a great number of shining oleaginous globules of divers sizes, some of them 50 times larger than others; and those also very soft and fluid. These spinal marrows were also furnished with exceedingly thin and manifold small veins or vessels; and besides these very small veins, there ran up and down along these spinal marrows brown filaments, thinner than the hair of the head. I perceived that each filament was not one single vessel by itself, but that each of them consisted of divers very small threads or vessels, lying by each other, between which threads there lay very clear vessels of the fineness of a single silk-worm thread.

Lately being at the house of M. Huygens, he showed me some of that moxa, which by burning it upon any gouty part removes the gout. This moxa agrees in shape with cotton: for as there is no other difference between hair and wool, than that hair is coarser and longer than wool, both being made up of globules, and they being clear about the rounder end; so little difference is there between the moxa and cotton, for they have both two flat sides. Such a shape has also the roughness that is found lying within, against the red bark of a chesnut; only with this difference, that that of moxa is much thinner than that of cotton, and that of cotton thinner than that of the chesnut. I have put some of the moxa on fine post-paper, and some cotton likewise, after I had somewhat cut it asunder with scissars, that so by its being shorter the fire might the better pass from one part to the other. The burnings caused on the paper by both were very near alike; and I concluded, that if the burning had any effect in the gout, it proceeded not from any peculiar quality in the moxa, but only from the burning itself; and that if the burning were made with cotton it would produce as good effects as if made with moxa.

Having considered the saying of surgeons, that cotton is fiery and malignant if any wound be dressed with it; I have found, that that firiness or malignity consists in this, that cotton having two flat sides, and consequently every part of it two sharp sides, which being thinner than globules that make up the carneous filaments, and being also stiffer than the globular flesh; it happens, that cotton being laid upon a wound, not only the globules of the yet sound flesh are annoyed by the sharp sides of it, but also the new matter which is conveyed to make new flesh, and is yet softer than the flesh already made, is the more easily cut asunder and dissolved; whereas, on the contrary, linen rags having roundish parts, and many of them lying firm together, and so making up a greater body, do not so wound the globular parts of the flesh.

Description of a curious Celestial Globe, showing the apparent Motions, from East to West, and from West to East, of the Sun, Moon, and fixed Stars. Made by M. Didier L'Alleman, Watchmaker at Paris. N° 136, p. 905.

The size of this globe is only of 4 inches diameter. The body is of burnished steel, where all the figures of the constellations are designed in silver-colour, but the stars themselves of all magnitudes are put on in embossed gold. The circles of the longitude of the stars which separate the signs, and which come from the poles of the Zodiac, are marked by gold-wires; as also the equator, the tropics, and the poplar circles.

This globe moves from east to west in 24 hours; showing the sun exactly to rise and set as in the great world; also the moon, with the stars of the constellations; likewise how the sun of this globe comes to his meridian, with an admirable regularity. Here also it is seen, that every day the sun sensibly passes one degree from west to east, which is its own proper motion finished by him in a year, and thereby describing to us the inequality of days and nights. Also every day the mean motion of the moon from west to east, how she increases according as she removes from the sun, so that it shows visibly the first quarter of the moon, the end of the second quarter, which is the full; then the third quarter which is the last quadrature; and lastly, her conjunction with the sun. And thus she is seen to finish every month her synodical course; and by her diurnal motion of 24 hours she shows the flux and reflux of the sea, or high and low water.

The meridian serves for a needle, to show the hours which are marked on the Zodiac, where the sun moves regularly, which has two main rays, one going directly northward, the other southward. That of the north marks the way or degree which the sun makes from west to east, on the signs of the Zodiac, and on a circle of silver, where the 360 degrees of the circle are marked. The other ray, of the south, marks on another circle of silver the days of the month, where the 365 days are noted.

This Globe may generally serve for the whole world, as it can be put to all the elevations of the pole. There is but one great spring, the primum mobile, which puts all the rest in motion. It is wound up by the antarctic-pole; and by the arctic-pole, the movement may be accelerated or retarded.

A Description of the Diamond Mines, as it was presented by the Earl Marshal of England, to the Royal Society. N° 136, p. 907.

The parts of the world containing diamonds, are the island Borneo, and the continent of India on both sides the Ganges: Pegu is reported to have several mines;* but the king contents himself with his mines of rubies, sapphires, topasses, emeralds, gold, silver, brass, tin, and lead, and several other commodities his country affords, in great plenty, rather than to suffer new inquiries to be made, lest the discovery of such an additional treasure should invite some of his neighbours, more potent, to invade him.

The diamond-mines in Coromandel are generally near rocky hills or mountains; a great range of which hills begins near Cape Comorin, extending in breadth about 50 English miles, and running in length quite through Bengala. The kingdoms of Golconda and Visiapore, have mines sufficient to furnish all the world plentifully with diamonds; but their kings permit digging only in some places, lest they should become too common, and lest it should tempt invaders of the country.

In the kingdom of Golconda are 23 mines now employed, or that have been so lately. Colure was the first mine used in this kingdom, Golconda. The earth is somewhat yellowish, not unlike the colour of gravel dried; but whiter in some places, where it abounds in smooth pebbles, much like some of those of the gravel-pits in England. They used to find great numbers in the vein, if it may properly be so called, the diamonds not lying in continued clusters, but frequently so very scattering, that sometimes in the space of $\frac{1}{4}$ of an acre of ground, digged between 2 or 3 fathoms deep, there has nothing been found; especially in the mines that afford large stones, lying near the surface of the earth, and about 3 fathoms deep; deeper they could not dig for water; it being in a vale near a river.

The diamonds found in these mines are well shaped, many of them pointed, and of a good lively white water; but it also produces some yellow ones, some brown, and of other colours. They are of ordinary sizes, from about six in a mangelin,† to five or six mangelins, each; some of 10, 15, 20, they find but rarely. They have frequently a bright and transparent coat, inclining to a greenish colour, though the heart of the stone be purely white; but the veins of these mines are almost worn out.

The mines of Codawillikul, Malabar, and Buttepallam, consist of a reddish earth, inclining to an orange-colour, with which it stains the clothes of the labourers that work in it. They dig about 4 fathoms deep. They afford stones generally of an excellent water, and crystalline skin; being of smaller sizes than those of Colure, Ramiah, Gurem, and Muttampellee, and have a yellowish

* Since this account was written, diamonds have been found on the continent of America also, and particularly in the Brasils.

† A mangelin is four grains in weight; says Linschoten.—Orig.

earth, like Colure; their stones like those of the two former mines, but mixed with many of a blue water.

The next mine is Currure, the most famous of them all, and most ancient. In it have been found diamonds of a scize weight, which is about 9 ounces troy, or 81$\frac{1}{7}$ pagos weight. It is only employed by the king for his own private use: the diamonds that are found in it are very well spread, and large stones, it yields few or none small. They have generally a bright skin, which inclines to a pale greenish colour, but within are purely white. The soil is reddish as many of the others.

About 60 or 70 years ago, when it was under the government of the Hindoo, and several persons permitted to adventure in digging, a Portuguese gentleman went thither from Goa, and having spent in mining a great sum of money to the amonnt of 100,000 pagos, and converted every thing he brought with him, even to his clothes into money, while the miners were at work for the last day's expence, he had prepared a cup of poison, resolving, if that night he found nothing, to drink his last with the conclusion of his money; but in the evening the workmen brought him a very fair spread stone of 20 pagos weight.

Not far from Currure are the mines of Lattawaar and Ganjeeconta, which are in the same soil as Currure, and afford stones not unlike: but Lattawaar yields many representing the great end of a razor-blade, thin on one side and thick on the other, very white and of an excellent water; but the best of the mine is worn out, and Ganjeeconta employed only for the king's private use.

Jonagerre, Pirai, Dugulle, Purwillee and Anuntapellee, consist also of red earth, and afford many large stones; part of them of a greenish water; but the most absolute mines are of Wazzergerre and Munnemurg, the others rather representing pits than mines; for there they sink through high rocks till they go so far below their basis, that they can go no further for water, in some places 40 or 50 fathoms deep. The surface of the rocks consist of hard, firm, white stone, into which they cut a pit like a well, of about 4 or 5, in some places 6 feet deep, before they come to a crust of a mineral stone, like the mineral of iron; when they fill the hole with wood, and keep there as hot a fire as they can for 2 or 3 days, till they think it sufficiently heated; then they pour in water till they have quenched it, which also slakes and mollifies both stone and mineral; both being cold, they dig again, take out all the crumbled stuff, and dig up what they can besides, before they heat it anew; the crust seldom is thicker than 3 or 4 feet, after which they come to a vein of earth, that usually runs under the rock 2 or 3 furlongs; sometimes much further: this they dig all out and search, and if their first attempt prove successful, they go to work again, digging as deep as they can, till they come to water; for the drawing whereof, wanting the help of engines known in Europe, they can go no deeper, although the vein lies lower. All the lumps of the mineral they break in pieces, and often find diamonds enclosed in them. The earth they dig out is red: many large stones are found here; the smallest about 6 in a mangelleen. They are of mixed waters, but the most part good, only of disagreeable shapes, many cragged pieces of stones, some as if they had been parts of very large ones, others with pieces broken off.

In Langumboot they dig as they do at Wazzergerree and Munnemurg; the rock is not altogether so solid, but the earth and stones it produces much alike. Wootoor lies near Currure, and affords stones of a like magnitude, shapes and waters. It is employed only for the king's use: and singular, in that its diamonds are found in black earth.

Muddemurg far exceeds all the rest for diamonds of a delicate shape, water, and bright transparent coat; yet it has also many veiny ones, but those likewise of so curious shape and water, that it is difficult to discover them from the good, especially the small ones. It produces stones of divers magnitudes, from 10 and 12 in a mangelleen, to 6 or 7 mangelleens each, besides some large ones. The earth is red, but seated in the woods, and the water so bad, that to all, except the people bred there, it presently occasions fevers and destroys abundance, insomuch that it is forsaken. Yet it has been more profitable than any of the rest, the vein frequently lying near the superficies of the earth, seldom running deep, and is better furnished than any other yet discovered.

Mellwillee, or the New Mine, has its earth very red, and many of the stones found there are generally well-shaped, their size from 5 or 6 in a mangelleen to those of 14 or 15 each, and some larger; but greatest numbers of the middle size. Most of them have a thick dull skin, inclining to a yellowish water, not altogether so strong and lively as of the other mines; very few of them of a crystalline water and coat. They are reported to be apt to flaw in splitting. Several that promise by their seeming whiteness when rough, discover their deceitfulness after passing the mill, and too often have a yellowish tincture.

Visiapore is known to contain mines enclosing stones as large and good as those of Golconda; but the king, for reasons already given, makes use but of the meanest: whereby, as Golconda is famous for the largeness of those it affords, Visiapore is noted for the smallest; whose mines, though they seldom or never render an adventurer a fortune or estate at once, as sometimes those of Golconda do, by a great stone, or several found together; yet they are more populous and better employed, the small stones lying thicker in the earth, so that the generality are gainers, and few but they get their expence; whereas those of Golconda dig away a considerable fortune and find nothing, others not their charges, and where one is a gainer, several lose.

There are 15 mines employed in the kingdom of Visiapore, as follow:—In Ramulconeta mines, in red earth, about 15 or 16 feet deep, they seldom find a diamond of a mangelleen weight, but small ones, to 20 or 30 in a mangelleen. They are generally of an excellent crystalline water, have a bright clear skin, inclining often to a pale greenish colour, are well shaped, but few of them pointed. There are also found among them several broken pieces of diamonds, by the country people called shemboes.—In Banugunnapellee, Pendekull, and Moodawarum, they dig as at Ramulconeta, and in the same kind of earth; they also afford stones much alike, being neighbouring places.—Cummerwillee, Paulkull, and Workull, are not far distant, and produce stones much alike, out of the same coloured earth, but very small ones, even to 100 in a mangelleen.

Lungepoleur mines are of a yellowish earth, like those of Coleur, its diamonds are generally well shaped, globular, few pointed, of a very good crystalline water, and bright skins; many of them have a thick dark grass-green skin, some spotted also with black, that they seem all foul, yet are not so, but within purely white and clean. Their sizes are from 2 or 3 mangelleens downwards, but few very small.—Pootloor mines are of reddish earth, but afford stones much like those of Lungepoleur, only smaller, under a mangelleen; the general sizes are of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$ of a mangelleen.—Punchelingull, Shingarrampent, and Tondarpaar, are also of red earth; their diamonds not unlike those of Coleur, only rarely or never any large ones are found there.—Gundepellee has the same earth with the former, and produces stones of equal magnitude; but frequently of a pure crystalline water, in which they exceed the former.

Donee and Gazerpellee dig both in red earth likewise, and afford stones alike, the greatest part whereof are of good shapes and waters. They have also many shemboes, and some of bad waters, some brown, which these people call soft or weak watered, being esteemed of a softer and weaker body than others, by reason they have not so much life when cut, and are subject to flaw in splitting, and on the mill; their general product is in stones of middle sizes: but Gazerpellee has besides many large ones, and is the only mine noted for such in the kingdom of Visiapore.

The diamonds in all the mines are so scattered and dispersed in the earth, and lie so thin, that in the most plentiful mines it is rare to find one in digging, or not till they have cleared them of the cloddy matter, in which they are enclosed; and some of those of Melwillee have the earth so fixed about them, that till they grind them on a rough stone with sand, they cannot move it sufficiently to discover if they are transparent. Near the place where they dig, they make a cistern of about 2 feet high, and 6 feet over, with a small vent in one of the sides, about two inches from the bottom, by which it empties itself into a little pit, made in the earth to receive small stones, if any should run through. The vent being stopped, they fill the cistern with water, soaking therein as much of the earth dug out of the mines, as it can conveniently receive at a time; then they break the clods, pick out the great stones, and stir it with shovels till the water is all muddy, the gravelly matter falling to the bottom. Then they open the vent, letting out the foul water, and supplying it with clean, till all the earthy substance be washed away, and none but gravel remains at the bottom. Thus they continue washing till about 10 o'clock in the fore noon, when they take the gravelly stuff they have washed, and spread it on a place made plain and smooth, near the cistern, which being soon dried by the heat of the sun at that time of the day, they very curiously look it over, that the smallest bit of a stone can hardly escape them.

Some of the expertest labourers are employed in searching; he that sets them at work usually sitting by, and overlooking; but it is hardly possible, especially where many are employed, to watch them so narrowly, but that they may steal part of what they find, as many times some of them do, and, selling it privately, convert to their own use. If they find a large stone, they carry it not presently to their employer, but keep on looking, having an eye on him till they observe he takes notice of it, when with a turn of their hand they give him a glimpse of it, but deliver it not till they have done work, and then very privately, it being the general endeavour to conceal what they find, lest it should come to the knowledge of the governor of the place, and he require a share, which in the kingdom of Golconda is usually practised, without respect to any agreement made with them.

The miners, those that employ them, and the merchants that buy the stones of them, are generally ethnics; not a mussleman, that ever I heard of, followed the employment. These labourers and their employers are Tellingas, commonly natives of or near the place. The merchants are the Banians of Guzzarat, who for some generations have forsaken their own country to take up the trade, in which they have had such success, that it is now solely engrossed by them; who, corresponding with their countrymen in Surrat, Goa, Golconda, Visiapore, Agra and Dillee, and other places in India, furnish them all with diamonds.

The governors of the mines are also idolaters: in the king of Golconda's dominions a tellinga brammee rents most of them, whose agreement with the adventurer is, that all the stones they find under a pagoda weight, * are to be

* A Pagoda weight is 9 mangelleens.—Orig.

their own; all of that weight and above it to be his, for the king's use. But although this agreement be signed and sealed, he minds not at all the performance thereof, but endeavours to engross all the profit to himself by tyrannically squeezing both merchants and miners. Both merchant and miner go generally naked, only a poor rag about their middle, and a sash on their heads; they dare not wear a coat, lest the governor should say they have thriven much, are rich, and so enlarge his demands on them. The wisest, when they find a great stone, conceal it till they have an opportunity, and then with wife and children run all away into the Visiapore country, where they are secure.

The government in the Visiapore country is better, their agreement observed, taxes easier, and no such impositions on provisions; the merchants go handsomely clad, amongst whom are several persons of considerable estates, which they are permitted to enjoy peaceably, by reason whereof their mines are much more populous and better employed than those of Golconda.

It is observable, that notwithstanding the agreement with the adventurers of the mines, that all stones above a certain weight shall be for the king's use; yet in the metropolis of either kingdom, as the cities of Golconda and Visiapore are, there is no seizure, all stones being free.

An Account of some Books. N° 136, p. 917.

I. *The Primitive Origination of Mankind, considered and examined according to the Light of Nature; by the Honourable Sir Matthew Hale,* Knight, late Lord Chief Justice of his Majesty's Court of King's Bench, London,* 1677, *in fol.*

The worthy and learned author of this book principally considers these particulars:

I. That according to the light of nature and right reason the world was not eternal, but had a beginning. II. That, if there could be any imaginable doubt of the world's having a beginning, yet by the necessary evidence of

* Sir Matthew Hale, a learned writer and judge, in the times of Charles I. Cromwell, and Charles II. was born 1609, at Aldersley in Gloucestershire, and educated at Magdalen Hall, Oxford. He conducted himself with so much prudence during the civil wars, and after, that he was esteemed by both parties, and enjoyed honourable employments under them. In 1675 he resigned his office of Lord Chief Justice of the King's Bench, and died a few months afterwards. Sir Matthew was a very learned man, a sound lawyer, an upright judge, and an exemplary christian. His writings are numerous, on theology, philosophy, and on law. The principal of which, besides the work above noticed, were, 1. Contemplations, moral and divine, 8vo. 2. Observations on the Experiments of Torricelli, 8vo. 3. Essay on the Gravitation of Fluids. 4. Observations on the principal Natural Motions, particularly Rarefaction and Condensation. 5. The Life and Death of Pomponius Atticus. 6. Pleas of the Crown. 7. History of the Royal Statutes, &c. All of which are much esteemed.

natural light it appears, that mankind had a beginning, and that the successive generations of men were in their original ex non genitis. III. That those great philosophers who asserted this origin of mankind ex non genitis, both ancient and modern, and rendered it by hypotheses different from that of Moses, were mistaken. Here the several hypotheses of Plato, Aristotle, Empedocles, Epicurus, Avicon, Cardan, Cæsalpinus, Beregardus, and others, are examined. IV. That the Mosaical system, as well of the creation of men as of the world in general, abstractedly considered, without relation to the divine inspiration of the writer, is highly consonant to reason, and on a bare rational account highly preferable to the sentiments of those philosophers, who either thought mankind eternal, or substituted hypotheses of his first production different from the Mosaical.

II. Tractatus Medicus de Morbis Castrensibus Internis, Auth. Joh. Valentino Willio, Medico Regio Castrense. Haf. 1676, in 4to.

A treatise on camp diseases, superseded by modern publications on the same subject.

III. Hebdomas Observationum de Rebus Sinicis. Auth. Andræa Mullero, Greiffenhagio. Colon. Branden. A. 1674.

This tract contains, 1. An epitome of the history of China, both of the most ancient and the most modern. 2. A conjecture that the true religion and knowledge of God has been known in China. 3. A list of the kings of China. 4. A representation of the famous Chinese herb, called Gniseng. 5. A memorable conjunction of the planets in the time of Noah's flood. 6. A specimen of a geographical commentary on Paulus Venetus's Oriental history. 7. Of the weekly distribution of days, and their denomination taken from the planets, being used among the Chinese themselves.

IV. The Curious Distillatory, &c. written originally in Latin by Joh. Sigisin. Elsholt, and translated by T. S., M.D. Physician in ordinary to his Majesty. Lond. 1677, in 12mo.

The author of this tract treats on the art of distilling coloured liquors, spirits, oils, &c. from vegetables, animals, and minerals; in the doing of which he intermixes many experiments easy to perform, yet curious and useful, relating to the production of colours, of consistence and heat, in divers bodies that are colourless, fluid, and cold; and particularly several experiments on the blood of diseased persons, and its serum.

V. Medicina Statica, or Rules of Health, originally written by Sanctorius,* now translated by J. D. Lond. 1676. in 12mo.

* Sanctorius Sanctorius, who acquired so much celebrity by the treatise above-mentioned, was born at Capo d'Istria, and studied at Padua; where he afterwards obtained a medical professorship.

This ingenious and useful tract, now appearing in English, is known to have been long since published in Latin by the famous Sanctorius, whose design in it was, by a certain balance to satisfy intelligent persons, who desire to take care of their health, that those things are true which he has taught concerning the weight of insensible perspiration, and its causes, time, advantages and disadvantages, excess and defect; as also touching the air, meat and drink, sleep and waking, exercise and rest, and the affections of the mind.

VI. Systema Horticulturæ, containing in English the Art of Gardening, in three Books. By J. W., Gent. 8vo.

Treating of the excellency, situation, soil, form, walks, &c. of gardens. Secondly, of all sorts of trees planted for ornament or shade, winter-greens, flower-trees, and flowers. Thirdly, of the kitchen-garden, and of the great variety of plants propagated for food, and for any culinary uses. Illustrated with sculptures, representing the form of gardens, according to some of the newest models.

The Manner of Hatching Chickens at Cairo. By Mr. John Graves, late Professor of Astronomy at Oxford; and communicated by Sir George Ent, late President of the College of Physicians, London. N° 137, p. 923.

The people begin in the midst of January to heat the ovens, employing every morning 100 kintars, or pounds weight of camel's, or of Buffalo's dung, and

He died at Venice in 1636, aged 75. The Medicina Statica is a work of much originality, being the result of experiments prosecuted for many years with great perseverance and exactness, (by means of a new contrived weighing machine, or statical chair) and it must be confessed that many of the propositions or aphorisms relative to the free exit of the insensible perspiration as a preservative of health, and to its deficiency or suppression as a cause of disease, are incontrovertible. At the same time it cannot be denied that he assigned to the excretory function of the skin an influence by much too powerful and extended, not duly taking into consideration what dependency health and disease have upon the various other excretions; nor how much the deficiency of one kind of excretion (for instance that of the insensible perspiration) is often compensated by an increase or excess of some other. It is therefore not surprising that the doctrine of Sanctorius should have met with opponents, among the earliest and most conspicuous of whom may be mentioned Hippolytus Obicius, a physician of Ferrara, and author of an attack entitled, Staticomastix sive Staticæ Med. Demolitio; in which, however, there is more of satire than of argument. On the other hand, the Medicina Statica has not been without its advocates, among whom must be numbered Lister, and the learned de Gorter. Sanctorius had a turn for mechanical contrivances; and besides the statical chair with which he made his experiments on perspiration, he invented an instrument for extracting the stone from the bladder; an instrument for measuring the pulse; a thermometer for ascertaining the degrees of heat in disordered states of the body; a pensile bed; a bathing apparatus, &c. The other works published by this author are lib. xv. de Method. Vitand. errorum qui in arte Medica contingunt, fol. 1602; also Commentaries upon Hippocrates, Galen and Avicenna; and a book De Remed. inventione, 4to. 1631. His collected writings amount to 4 vols. 4to.

the like quantity at night, till the midst of February. About which time the ovens are so hot, that a person can hardly endure to lay his hand on the walls. After this, they put the eggs into the ovens, to hatch the chickens, which they continue successively till the end of May.

The eggs are first put upon mats in the lower ovens, which are on the ground; 7 or 8000 eggs in number, and laid only double, one upon another. In the ovens above these lower, the fire is made in long hearths or little channels, having some depth to receive the fire, from whence the heat is conveyed into the lower ovens before-mentioned. The eggs which are directly under these hearths, lie treble one upon another; the rest, as was said, only double. At night when they renew the fires in the hearths above-mentioned, they then remove the eggs that were directly undermost, lying treble one upon another, in the place of those which lay on the sides only double, and these being now removed, they lay treble under the hearth, because the heat is there greater than on the sides where the eggs are only double. These eggs continue in the lower ovens 14 days and nights; afterwards they remove them into the upper ovens, which are just over the lower. In these, there being now no more fire used, they turn all the eggs 4 times every day or 24 hours. The fire in the upper ovens, when the eggs are placed in the lower, is thus proportioned: The first day, the greatest fire. The second, less than the first. The third, less again. The fourth, more than the third. The fifth, less. The sixth, more than the fifth. The seventh, less. The eighth, more. The ninth, without fire. The tenth, a little fire in the morning. The eleventh, they shut all the holes with flax, &c. making no more fire; for if they should, the eggs would break. They take care, that the eggs be no hotter than the eye of a man, when they are laid upon it, can well endure. The twenty-first or twenty-second day the chickens are hatched, which the first day eat not; the second they are fetched away by women, who give them corn, &c.

When the chickens are hatched they put them into the lower ovens, which are covered with mats. Under the mats is bran, to dry the chickens: and upon the mats straw, for the chickens to stand on.

The master of the ovens has a third part of the eggs for his cost and pains, out of which he is to make good unto the owners, who have two-thirds in chickens for their eggs, such as may happen to be spoiled or miscarry.

The ground plot of the house and ovens is delineated according to fig. 1, pl. 12. Here ab is a long entrance, on each side of which are 14 ovens, sometimes more, sometimes less. The bottoms and sides of those ovens which are on the ground are all made of sun-dried bricks, upon which they put mats, and on the mats the eggs. The tops of these ovens are flat, and covered with sticks, except two long spaces, which are made of sun-dried bricks, and are the hearths above-mentioned, in which the fires are made to heat the eggs, lying under them in the lower ovens. Above these lower ovens are so many others, made of sun-dried bricks, and arched at the top. Where also there are some holes, which are stopped with tow, &c. or left open, as they please, to govern the heat in the ovens below.

The plan of the upper oven is according to fig. 2. Here, a, is the mouth of the oven, opening on the long entrance ab above-mentioned. b and c entrances into the ovens adjoining. d, e two hearths 3 or 4 inches deep, in which they make the fire to heat this and the oven below. The depth of the lower oven is about $2\frac{1}{2}$ feet English. The second above 4 feet.

On Barnacles. By Sir Robert Moray. N° 137, p. 925.

In the western isles of Scotland, much of the timber wherewith the common people build their houses, is such as the western ocean throws upon their shores. The most common trees are fir and ash. They are usually very large, and without branches; which seem rather to have been broken or worn off, than cut; and they are so weather-beaten that there is no bark left on them, especially the firs. Being in the island of Uist, I saw lying on the shore a cut of a large fir-tree, of about $2\frac{1}{2}$ feet diameter, and 9 or 10 feet long; which had lain so long out of the water, that it was very dry; and most of the shells that had formerly covered it were worn or rubbed off. Only on the parts that lay next the ground, there still hung multitudes of little shells, having within them little birds perfectly shaped, supposed to be barnacles. The shells hung very thick and close to each other, and were of different sizes; they were of the colour and consistence of muscle-shells, and the sides or joints of them joined with such a kind of film as muscle-shells have, which serves them for a hinge to move upon when they open and shut.

The figure of the barnacle-shell is here represented, fig. 3, pl. 12. It is thin about the edges, and about half as thick as broad. Every one of the shells has some cross seams or sutures, which, as I remember, divide it into five parts, nearly after the manner as in the figure. These parts are fastened to each other with such a film as muscle-shells are. These shells hang at the tree by a neck, longer than the shell. This is of a kind of filmy substance, round and hollow, and creased, not unlike the wind-pipe of a chicken, spreading out broadest where it is fastened to the tree, from which it seems to draw and convey the matter which serves for the growth and vegetation of the shell and the little bird within it.

This bird, in every shell that I opened, the least as well as the largest, I found so curiously and completely formed, that there appeared nothing wanting, as to the external parts, for making up a perfect sea-fowl: every little part appearing so distinctly, that the whole looked like a large bird seen through a concave or diminishing glass; colour and feature being every where so clear and neat. The little bill like that of a goose; the eyes marked; the head, neck, breast, wings, tail, and feet, formed; the feathers every where perfectly shaped, and blackish coloured; and the feet like those of other water-fowl, to my best remembrance. All being dead and dry, I did not look after the inward parts of them. But I carried about 20 or 24 away with me. The largest I found upon the tree was but about the size of the figure here representing them. I never saw any of the little birds alive, nor ever met with any person that did. Only some credible persons have assured me, they have seen some as large as their fist.*

A Description of the Island Hirta. By Sir Robert Moray. N° 137, p. 927.

Hirta lies from Snod, in the isle of Skye, west and by north. From the nearest land to it in the Hereisch, from whence people commonly take boat, it lies due west, and the distance of about 50 miles. There are three islands together, Hirta, Soa, and Burra; but Hirta only is inhabited. The other two are excellent pasturage for sheep, every sheep there having two lambs every year.

Burra is inaccessible, except to the men of Hirta only, on account of the difficulty of landing; there being but about a foot broad of a landing place, and that only to be attempted when the boat rises. For their usual way is, when they come near the rock they turn the boat, and set the side to the shore, two men, one at each end of the boat, with two long poles keeping it off, that the waves may not dash it too violently against the rock, when it rises; at which time only he who is to land makes his attempt. If he miss his landing place he falls into the sea, and the rest of the people haul him aboard by a small rope fastened about his middle, to prevent that danger. But when he safely lands, which they seldom miss to do, the rest of his fellows land one by one; except

* In the present advanced state of natural history, it is almost unnecessary to observe that the supposed birds contained in the shells here mentioned were no other than the proper inhabiting animals belonging to the Linnæan genus *lepas* among the *testacea*; the animal itself resembling the genus *triton*, having several pair of curved and jointed feelers or tentacula, which, from their peculiar disposition and fringed edges, bear a rude general resemblance to the tail of a bird. The species here meant is the *lepas anatifera* of Linnæus.

so many as they leave to attend their little boat, which is commonly of six oars.

If there be any strangers, as many go from the nearest islands in summer, they must be tied about the middle with a strong rope; and when the men of Hirta have climbed up to the top of the rock, which is above 24 fathoms, before they set their foot on grass, they haul up the strangers to them with the ropes. When they have gathered as many eggs, and killed as many fowls as will load their boat, they lower all down into the boat, and the ablest fellow is always left behind, who, having none to help him, must throw himself into the sea, and so recover the boat. Burra lies from Hirta about six miles northward.

Soa lies near Hirta, on the south-west. In this, except fowls, there is nothing remarkable but a creek, frequented by large seals. The people are so daring, that they go in their boat, about four of them, in that narrow passage, to kill these seals with poles, having scarcely room for their oars, and every where seeming to close up the mouth of the passage. If the wind change during their being there, it is not possible to save man or boat. There are several rocks rising out of the sea amongst these islands, which the people of Hirta call stacks: some 10, 20, or 24 fathoms above water, without any grass upon them. On the round tops of the rocks a great number of fowls breed, and in all the cliffs. Among the rest there is one called Stacka Donna, on the top of which breeds such an abundance of fowls, that though it seems inaccessible, yet the men of Hirta have ventured to go thither. After they have landed with much difficulty, a man having room but for one of his feet, he must climb up 12 or 16 fathoms high. He then comes to a place, where having but room for his left foot and left hand, he must leap from thence to such another place before him; which, if he hit right, the rest of the ascent is easy, and with a small cord, which he carries with him, he hauls up a rope, by which all the rest come up. But if he misses that footstep, as often happens, he falls into the sea, and the rest draw him by the small cord. After sitting still till he be a little refreshed, he then tries it again; for every one there is not able to undertake it.

Hirta island is 2 miles in length, accounted Five-penny-land. In it are 10 families. The men seldom grow old; and seldom was it ever known that any man died in his bed there, but was either drowned or broke his neck. The men are strong, large, and well complexioned. Their food is only young fowls and eggs; their drink whey and water. They are much given to keeping of holy days, having a number of little chapels, where sometimes they watch whole nights, making merry together with their offerings.

The chief employment of their women is to harrow the land, which they must do when their husbands are climbing for fowls.

Their ordinary way of dividing the land, is one halfpenny to every family. The rocks also are divided, such and such on every halfpenny. And there is a kind of officer left by the master of the island, who governs in his absence, and so regulates, that the best climbers and the worst are mixed together, that so none of the land be unlaboured; that is, that all the shelves of the highest rocks be searched for eggs.

The way of their climbing is thus; they go two and two with a long rope, not made of hemp, but of cow-hides salted, and the thongs cut round about, and plaited six or nine fold. Each end of the rope is tied about each one of their middle, and he that is foremost goes till he comes to a safe standing, the other standing firm all that time to keep him up, in case his feet should have slipped: when the foremost is come to a safe standing; then the other goes, either below or above him, where his business is; and so they watch time about; seldom any of them being lost when this is observed.—When any couple is to be married, the aforesaid officer brings them to one of their chapels, and administers an oath to them; and thus they are married.

Their children, about the age of 15 or 16, come with the master of the isle to the Hereisch island, and are there baptized.

An ordinary way of killing the fowls in the mist is this, some of these fellows lie beside the door of the little houses they have in their islands, flat upon their backs, and open their breasts; which when the fowls perceive, they perch upon them, and are presently taken. And thus hundreds are killed in one night.

*Observations on a Cameleon. By Dr. Jonathan Goddard, late Professor of Physic at Gresham College, London.** N° 137, p. 930.

There is nothing of sufficient importance in this paper to justify reprinting it.

Account of the Iron Works in the Forest of Dean. By Henry Powle, Esq. N° 137, p. 931.

The forest of Dean, comprehending that part of Glocestershire between the rivers Wye and Severn, consists generally of a stiff clay. The country is full of hills, but no where high, and rarely of a steep ascent. Between them run many little springs of a more brownish colour than ordinary waters, and often leaving in their passage tinctures of rust. The ground is naturally inclined to

* See N° 49, p. 369, vol. i.

wood, especially hasel and oak; of which last sort it has produced formerly very stately timber; though now almost totally destroyed by the increase of the iron works.—On the surface of the earth in many places, lie abundance of rough stones, some of them of great bulk; but where the mines are sunk, they rather meet with veins of scaly stone than hard and solid rocks. Within the forest is found great plenty of coal and iron-ore; and in some places red and yellow ochre; which are all the minerals that are yet discovered there.—The iron-ore is found in great abundance in most parts of the forest: differing both in colour, weight and goodness. The best, which they call their brush-ore, is of a bluish colour; very ponderous and full of little shining specks, like grains of silver. This affords the greatest quantity of iron; but being melted alone, it produces a metal very short and brittle, and therefore not so fit for common use.

To remedy this inconvenience they make use of a material which they call cinder, being only the refuse of the ore after the metal has been extracted; which being mingled with the other in a due quantity, gives it that excellent temper of toughness, for which this iron is preferred before any that is brought from foreign parts.

To understand this rightly, it is to be noted, that in former times when their works were few, and their vent small, they made use of no other bellows but such as were moved by the strength of men: by reason whereof their fires were much less intense than in the furnaces they now employ. So that having in them melted down only the principal part of the ore; they rejected the rest as useless, and not worth their charge. This they call their cinder, which is now found in an inexhaustible quantity through all parts of the country, where any former works have stood.

After they have provided their ore, their first work is to calcine it: which is done in kilns, much after the fashion of ordinary lime-kilns. These they fill up to the top with coal and ore, stratum super stratum, until it be full; and so setting fire to the bottom they let it burn till the coal be wasted, and then renew the kilns with fresh ore and coal, in the same manner as before. This is done without fusion of the metal, and serves to consume the more drossy parts of the ore, and to make it friable, supplying the beating and washing which are used for other metals.—From hence it is carried to the furnaces, which are built of brick or stone, about 24 feet square on the outside, and near 30 feet in height; within not above 8 or 10 feet over, where it is widest, which is about the middle; the top and bottom having a narrower compass, much like the shape of an egg.

Behind the furnace are placed two huge pair of bellows, whose noses meet at a little hole near the bottom. These are compressed together by certain buttons, placed on the axis of a very large wheel, which is turned about by water, in the manner of an overshot-wheel. As soon as these buttons are slid off, the bellows are raised again by the counterpoise of weights; by which they are made to play alternately, the one giving its blast all the time the other is rising.

At first the furnaces are filled with ore and cinder, intermixed with fuel, which in these works is always of charcoal; laying them hollow at the bottom, that they may more easily take fire: but after they are once kindled, the materials run together into a hard cake or lump, which is sustained by the shape of the furnace, and through this the metal as it melts, trickles down into the receivers, which are placed at the bottom, where there is a passage open, at which they take away the scum and dross, and let out the metal as they see occasion.—Before the mouth of the furnace lies a great bed of sand, in which are made furrows of the shape into which they intend to cast the iron. Into these when the receivers are full they let in the metal; which is made so very fluid by the violence of the fire, that it not only runs to a considerable distance, but continues boiling for a good while.

After these furnaces are once at work, they keep them constantly employed for many months together, never suffering the fire to slacken night nor day; but still supplying the waste of the fuel and other materials with fresh, poured in at the top.

Several attempts have been made to introduce the use of sea-coal in these works, instead of charcoal; the former being to be had at an easier rate than the latter; but hitherto they have proved ineffectual. The workmen finding by experience, that a sea-coal fire how vehement soever, will not penetrate the most fixed parts of the ore, and so leaving much of the metal unmelted.

From these furnaces they bring their sows and pigs of iron, as they call them, to their forges. These are of two sorts, though standing together under the same roof: one they call their finery, the other the chafery. Both of them are open hearths, on which they place great heaps of sea-coal, and behind them bellows, like to those of the furnaces, but not near so large. Into the finery they first put the pigs of iron, placing three or four of them together behind the fire, with a little of one end thrust into it. Where softening by degrees they stir and work them with long bars of iron, till the metal runs together into a round mass or lump, which they call a half-bloom. This they take out, and giving it a few strokes with their sledges, they carry it to a great weighty hammer, which is raised by the motion of a water-wheel, where applying it dexterously under the blows, they presently have it beaten out into a thick short square. This they put into the finery again, and heating it red hot, they work it out under the same hammer, till it comes into the shape of a bar in the middle, with two square knobs in the ends. Lastly, they give it other heatings in the chafery, and more workings under the hammer, till they have brought it into bars of several shapes and sizes, fit for sale. And if they omit any one process, it will be sure to be deficient in toughness, which is accounted its perfection: though for several purposes, as for the backs of chimneys, hearths of ovens, and the like, they have a sort of cast-iron, which they take out of the receivers of the furnace in great ladles, and pour it into moulds of fine sand: but this sort of iron is so very brittle, that being heated, with one blow of a hammer it breaks all to pieces.

The Method of making Cerusse. By Sir Philiberto Vernati. N° 137, p. 935.

Pigs of clean and soft lead are cast into thin plates, a yard long, 6 inches broad, and as thin as the back of a knife. These are artfully rolled round in such a manner that the surfaces no where touch; for where they do, no cerusse grows.—Thus rolled they are put each in a pot, just capable to hold one, sustained by a little bar from the bottom, that it come not to touch the vinegar, which is put into each pot, to effect the corrosion.—Next a square bed is made of new horse-dung, large enough to hold 20 pots abreast, and so to make up the number of 400 in one bed.—Then each pot is covered with a plate of lead; and lastly all with boards as close as possible. This repeated four times, makes what is called one heap, containing 1600 pots.

After three weeks the pots are taken up, the plates unrolled, laid upon a board, and beaten with battledoors till all the flakes come off: which if good, prove thick, hard and weighty: if otherwise, fuzzy and light; or sometimes black and burned, if the dung prove not well ordered: and sometimes there will be none.—From the beating-table the flakes are carried to the mill; and with water ground between millstones, until they be brought to almost an inpalpable fineness. After which it is moulded into smaller parcels, and exposed to the sun to dry, till it be hard, and so fit for use.

Sometimes two pots alike ordered, and set one by each other, without any possible distinction of advantage, shall yield the one thick and good flakes, the other few and small or none.—Sometimes also the pots are taken up all dry, and so prove then the best: sometimes again they are taken up wet.—It sometimes happens too that the plates that cover the pots yield better and thicker flakes than the rolls within. And the outsides next to the planks larger and better than the insides next to the rolls, and to the spirits that first arise from the vinegar.

The accidents happening to the workmen, are immediate pain in the stomach, with exceeding contortions in the guts, and costiveness that yields not to cathartics, and hardly to often repeated clysters: best to lenitives, oil of olives, or strong new wort. They also fall into acute fevers, and asthmas, or shortness of breath. And these are effected principally by the mineral steams in the casting of the plates of lead, and by the dust of the flakes; also by the steams coming from the heaps, when the pots are taking up. Then with a vertigo or dizziness in the head, with continual great pain in the brows, blindness, stupidity, and paralytic affections; loss of appetite, sickness, and frequent vomitings, generally of pure phlegm, sometimes mixed with choler, to the extreme weakening of the body. And these chiefly in those that have the charge of grinding, and over the drying place.

Account of Two Books. N° 137, p. 936.

I. The True Intellectual System of the Universe. The First Part. Wherein all the Reason and Philosophy of Atheism is confuted, and its impossibility demonstrated. By R. Cudworth, D.D.* London, 1678, in fol.

The design of the author is to demonstrate these three things: 1. That there is an omnipotent intelligent Being presiding over all.—2. That this Being has an essential goodness and justice: the differences of moral good and evil not being by will and law only, but also by nature; according to which the Deity acts and governs mankind.—3. That necessity not being intrinsical to the nature of every thing, but men having such a power over their own actions, as to render them accountable for the same; there is therefore a distributive justice running through the world.

* Dr. Ralph Cudworth was born 1617, at Aller, in Somersetshire. He studied at Cambridge, where while he was tutor he had Sir Wm. Temple among his pupils. Here also he held various other important and lucrative offices, as, the Hebrew professorship, and successively the mastership of Clarehall and Christ's College; and his church preferments were the rectory of North Cadbury, the vicarage of Ashwell, and a prebendary in the cathedral of Gloucester. He died at Cambridge 1688, aged 71, leaving among other children, a daughter named Damaris, who distinguished herself by her learning. Dr. Cudworth was well acquainted with the learned languages, the belles lettres, and antiquities; he was a sound divine, a deep metaphysician, and a good mathematician. In philosophy he followed the mechanical principles; and in metaphysics, the ideas and opinions of Plato. His chief works, besides the Intellectual System against Infidels, abovementioned, were—1. A Discourse on the Lord's Supper, 1642. 2. A Sermon against Absolute Reprobation. 3. A Treatise concerning Eternal and Immutable Morality. The Intellectual System was translated into Latin by Mosheim, with notes and dissertations, Jena, 1733, 2 volumes folio, and since at Leyden, 2 volumes 4to. In 1743 also came out a complete edition of the Intellectual System, with some additional pieces, edited by Dr. Birch; and an abridgement has been published by Thomas Wise, in 2 volumes 4to, which is esteemed.

II. The Six Voyages of John Baptista Tavernier,* Baron of Aubonne, through Turkey, into Persia and the East Indies. London, 1678, in fol.

Some observations have before been cited out of this book; and no further account of it is now necessary.

On the Culture or Planting and Ordering of Saffron. By the Hon. Charles Howard. N° 138, p. 945.

Saffron heads planted in a black rich sandy mold, or in a mixed sandy land, between white and red, and only moderately rich, yields the greatest plenty of saffron.—Plough the ground in the beginning of April, and lay it very smooth and level.—About three weeks or a month after, spread upon every acre 20 loads of rotten dung, and plough it in.—At Midsummer plough it again, and plant the saffron-heads in rows 3 inches distant from each other every way, and 3 inches deep.—Observe this order in planting of whole fields, whereby the heads will lie every way 3 inches square from each other. Only paths or shallow trenches are to be left 2 or 3 yards asunder, which serve every year to lay the weeds to rot that are to be weeded and pared off the ground.

As soon as the heads begin to shoot or spear within the ground, which is usually a fortnight before Michaelmas, hoe or pare the ground all over very thin: and rake lightly all the weeds and grass very clean, lest it choak the flowers, which will soon after appear; and are then to be gathered, and the saffron to be picked and dried for use.—In May the saffron-grass will be quite withered away; after which, the weeds and grass produced may be cut or mowed off from time to time to feed cattle till about Michaelmas, at which time the heads will begin to spear within the ground.—Then hoe, pare and rake the ground clean as before, for a second crop. And the like directions are to be observed the next year for a third crop.

The Midsummer following dig up all the saffron-heads, and plant them again in another new ground, ordered as aforesaid, wherein no saffron has been planted, at least not within seven years.—The flowers are to be gathered as soon as they come up, before they are full blown, whether wet or dry.—Pick out the sheaves clean from the shells or flowers, and sprinkle them two or three fingers thick, very equally on a double saffron-paper. Lay this on the hair-

* John Baptist Tavernier, a noted French traveller, was born at Paris in 1605. He visited Turkey, Persia, and the East Indies six times; and on the seventh journey, in 1689, died at Moscow, being 84 years of age. He had acquired a great fortune by trading in jewels, and was ennobled by Lewis the 14th. His affairs however, in the latter part of his life, fell into bad condition, by the mismanagement of his nephew, to whom they were entrusted. His travels have been published in many forms and languages. They amount to 6 volumes in 12mo.

cloth of the saffron-kiln, and cover it with two or more saffron-papers, a piece of woollen cloth, or thick baize, and a cushion of canvas or sackcloth filled with barley-straw, on which lay the kiln-board.—Put into the kiln clean, thoroughly kindled charcoal, oven coals, or the like, keeping it so hot that you can hardly endure your fingers between the paper and the hair-cloth.—After an hour or more turn in the edges of the cake with a knife, and loosen it from the paper. If it stick fast wet the outside of the paper with a feather dipped in beer, and then dry the papers. Turn the cake, that both sides may be of a colour.—If it stick again to the paper, loosen it, and then dry it by a very gentle heat, with the addition of 20 or 30 pound weight laid on the kiln-board.—The saffron-cake being sufficiently dried, it is fit for use, and will last a good many years, being wrapped up and kept close.

The best saffron is that which consists of the thickest and shortest chives, of a high red and shining colour, both without and within alike.—Saffron is oftentimes burned, and in knots, spotted and mixed with the yellows that are within the shells.—One acre yields at the last 12 pounds of good Saffron, one year with another, and some years 20 pounds.—Sixteen quarters of saffron-heads are sufficient to plant one acre.

The kiln consists of an oaken-frame, lathed on every side, 12 inches square in the bottom, 2 feet high, and 2 feet square at the top; upon which is nailed a hair-cloth, and strained hard by wedges driven into the sides; a square board and a weight to press it down, weighing about a quarter of a hundred.—The insides of the kiln covered all over with the strongest potter's clay, very well wrought with a little sand, a little more than 2 inches thick.—The bottom must be lined with clay 4 or 5 inches thick, which is the hearth to lay the fire on: and level with which is to be made a little hole to put the fire in. The outside may be plastered all over with lime and hair.

Account of the Tin Mines in Cornwall. By Dr. Christopher Merret. N° 138, p. 949.

The stones, from which tin is wrought, are sometimes found a foot or two below the surface of the earth, but most usually between two walls or rocks, and are commonly of an iron colour, of little or no affinity with the tin, and lying in a vein or load between 4 and 18 inches broad. Sometimes there is a rich and fat metal; sometimes hungry and starved; sometimes nothing but a drossy substance, neither purely earth, nor stone, nor metal; but a little resembling the rejected cinders of a smith's forge: and where this is found the miners judge the metal to be ripe.

The pits are 40, 50, or sometimes 60 fathoms deep and more. The load being very rich and good, above that is about 10 fathoms from the grass. And below that there is a large cavity, containing nothing but air, for many fathoms deep. This cavity lies between hard stony walls, about 6 or 9 inches asunder.

Tin is usually incorporated with the stone, or is found in it. They break every individual stone, and if there be any blackness in the stones, of this black stuff the tin is produced.—Sometimes it is as it were mixed with a small gravelly earth; sometimes white, but for the most part red. From this earth it is easily separated by bare washing; but from the stone not without much stamping.—This gravelly tin they distinguish from that which is gathered from the stones, calling it pryan tin, and is but about half the richness of the other.—They have another sort of ore, called mundic ore. Being mixed together, the mundic may be easily known by its glittering, yet deep brownness.—The mundic is said to nourish the tin; and yet they say where much mundic is found, there is little or no tin; and where there is little or none of that, much and good tin is found. Certain it is, if there be any mundic left in melting the tin, it does it much prejudice, making it less ductile. For tin without it will easily bend any way; but mixed with it, becomes very brittle, and will crack and break.

This mundic seems to be a kind of sulphur. Fire only separates it from the tin, and evaporates it into smoke. Little sprigs or boughs being set in the chimney, the smoke gathers upon them into a substance, which they call poison, and think it is a kind of arsenic; which being put into water, easily dissolves, and produces very good vitriol.—The water in which it is dissolved soon changes small iron rods put into it.—When they burn it to separate it from the tin, it sends forth a very loathsome and dangerous stench.

Besides the forementioned stones, &c. found in tin mines, and incorporated with the tin; there occurs a spar, mixed also with this metal, as it is commonly with lead and copper.—This appears frequently of a shiny whitish substance; and casts a white froth on the water in washing it. When first taken out of the earth it is soft and fattish, but soon after grows somewhat hard. The miners call it white spar.

The Cornish diamonds, so called, lie intermixed with the ore, and sometimes on heaps: some of them large enough to have a coat of arms engraven on them; and are hard enough to cut glass. Some of them are of a transparent red, and have the lustre of a deep ruby. These diamonds seem to me to be but a finer, purer, and harder sort of spar; for they are both found together, as on St. Vincent's rocks near Bristol.

The working of the ore is performed in this manner: The stones, beaten as before said, are brought to the stamping-mill. They are so disposed, as that by degrees they are washed into a latten-box with holes, into which the stampers fall: by which means they are beaten pretty small, and by the water continually passing through the box, the ore through its weight, falls close by the mill, and the parts not metalline, which they call causalty, are washed away by the water. And thus the first separation is made.—They then take that which falls close by the mill, and so dispose it in the said mill, that the water may once more drive it, to make a better separation of the causalty.—Next they dry it in a furnace on iron-plates, and then grind it very fine in a crazing-mill, with stones common in the hills of that country.—After this they re-wash it as before, and then dry it a little, and so carry it to the furnace, called a blowing-house, and there melt and cast it.

There swims on the metal, when it runs out of the furnace, a scum, which they call dross; much like to slag or dross of iron; which being melted down with fresh ore, runs into metal. The causalty they throw in heaps upon banks, which in 6 or 7 years they fetch over again, and make worth their labour. But they observe, that in less time it will not afford metal worth the pains; and at the present none at all.*

Experiments of refining Gold with Antimony.† *By Dr. Jonathan Goddard.*‡
N° 138, p. 953.

I. Having taken 178 grains of crown gold, of 22 carats fine, it was melted down with about 6 times as much antimony. And because the gold was made into plates, for the more certain melting and mixture, the first regulus of gold being separated from the antimony, both were powdered apart, and the regulus in the melting-pot laid on the same antimony, and so both melted down again.

* In some of the future volumes a further account of tin-ores, with their analysis, will be given.

† The mode here described of refining gold by antimony is extremely tedious, and is attended with considerable expence. Gold may be freed from an alloy of the baser metals by cupellation with lead; but not from an alloy of silver. For the separation of this last it is usual to resort to the processes of *quartation* and *parting by aquafortis.*

‡ Jonathan Goddard, a physician of much note in the 17th century, was an early and active member of the Royal Society. He was educated at Oxford, and took his medical degree in that university. After passing some time in foreign travel, he established himself in London, and read anatomical lectures before the College of Physicians. He became a favourite with Cromwell, who appointed him chief physician of the army, made him warden of Merton College, and conferred other honours upon him. These appointments, however, were taken from him on the restoration of Charles II.; and he was only suffered to retain his situation of lecturer on physic, at Gresham College. He died in 1674. He always prepared his medicines himself, the composition of which he kept a secret. An account of them, however, was published in 1691, under the title of Arcana Goddardiana, annexed to Bate's Pharmacopœia. It is said he was the first who made a Telescope in this country. See the 3d Volume of Birch's History of the Royal Society.

In both which meltings such a heat was given, as made all of a clear light red heat. Then the pot was taken off, and all permitted to separate, settle, and cool in it. On breaking the pot the regulus of gold, being very distinct in the bottom, and easily separated from the antimony, weighed 163 grains.

This way of cooling all in the pots was observed in all the following experiments, for the more certain separation and settling of the regulus, without effusion into the antimony-horn, or hollow iron-cone. Which effusion, by confounding and cooling the mixture, may be some hindrance to a more perfect separation. Note also, that borax was used in every pot, for preventing the sticking of the regulus to the bottom, and the antimony to the sides of it.

A piece was broken off the regulus, which weighed 38½ grains, and was kept to be refined on the cupel apart. The weight of the remainder was therefore 124½ grains. This remainder being powdered, and put on an equal quantity of fresh antimony, as at first, and melted down, the regulus weighed 74 grains.

The other piece, of 38½ grains, being refined on a cupel from the antimonial substance mixed with it, by exhalation, promoted sometime with a blast upon it, especially toward the latter end, as in all the following experiments of refining upon the cupel, weighed 30½ grains: and upon melting with borax in a crucible, lost not above half a grain. So that the weight of the whole to the gold it held, was as 38½ to 30½, or the gold almost ⅘ of the whole.

The latter regulus, weighing 74 grains, being refined in the same manner, weighed 63 grains: the gold holding in proportion to the whole, as 63 to 74, that is near upon $\frac{6}{7}$ of the whole. So that the same regulus of gold and antimony, in passing through new antimony, though it lose much in weight, yet there is not a proportionable loss of gold: but it is richer in gold, as is proved by this and many other trials.

Both the parcels of antimony being saved, for separating the gold remaining behind in them; they were severally mixed with equal weight both of tartar and nitre, and then fired, and so reduced to a regulus. Then the regulus of each, exhaled and blown off upon cupels. Of the first parcel of antimony, with which the gold was first melted, the regulus being exhaled, there remained in gold 36 grains. Which, upon melting in a crucible, lost scarcely half a grain. Of the second parcel of antimony, with which the first regulus of gold and antimony, weighing 124½ grains, was melted, there remained in gold 27 grains.

All the other parcels were fine gold to sense, on the touch. Only that out of the first antimony, was apparently not fine, and pale, from the silver in the original alloy mixed with it, and not from any remainder of antimony, as appeared by the inconsiderable waste upon melting it in a great heat with a blast upon it: and also by the toughness and malleability: and by comparing it on the touchstone, with sovereign gold allayed with silver, to which it agreed, but was somewhat paler; holding, as judged, about a fourth part of silver, as the sovereign gold holds a sixth. Neither was it quite free from copper; because, upon annealing, it always turned black on the surface.

But for more exact discovery, it was first refined with lead on a cupel, for separation of any copper that might be in it. On which operation it came forth $33\frac{1}{2}$ grains; which was $2\frac{1}{2}$ grains less than it was before. Afterwards this last was melted with between 2 and 3 parts of silver, and so wrought in aquafortis for separation of the silver: and there remained in gold $28\frac{1}{2}$ grains, being 5 grains short of the former. And yet it appeared, on the touch, not fine, but paler than fine gold, and deeper than crown gold allayed with silver. So that what remained in it was necessarily of silver; and it might be estimated about 23 carats fine; or to hold in fine gold about 27 grains.

The loss of gold, on this refining with antimony, may easily be computed. First, $\frac{1}{12}$ is to be deducted from the first quantity of crown gold, being 178 grains, for alloy; which is $14\frac{1}{6}$ grains. So the remainder is $163\frac{5}{6}$ grains. Then the several parcels of fine gold, recovered and separated from the regulus of antimony and gold, and also from the parcels of the crude antimony reduced to regulus, are to be added together: that is to say, 30 grains, 63 grains, 27 grains, and 27 grains (the 27 grains last mentioned;) all which amount to 147 grains. Which being deducted from the first quantity of 163 grains, the difference is 16 grains, which is $\frac{1}{10}$ and $\frac{3}{16}$ of $\frac{1}{10}$.

The piece of regulus weighing 124 grains, melted with the second parcel of antimony (in proportion to the former piece broke off, weighing 38 grains, and upon refining yielded 30 grains of pure gold) must contain 98 grains of the like gold; and so much this second parcel of antimony must be charged with. Toward which, the regulus weighing 74 grains, being refined, produced 63 grains. And that gold separated from the same antimony, being 27 grains, added to the former, make 90 grains: short of the first quantity charged on this parcel of antimony, by 8 grains.

Some loss of gold may happen on powdering the regulus in an iron mortar, as also by the papers necessarily used. But it is most probable, that the greatest loss was by small sparks, which continually fly up while the antimony is in a boiling heat with the gold; which is always given it for the better melting and mixture. These sparks appear heavy, by their rising not very high, and most of them falling down again on the metal and within the pot: but many fly over into the fire. It is not improbable also, that some loss of gold may take place on the firing of the antimony, for reducing it to a regulus with tartar and nitre, which make a vehement conflagration with much sparkling.

It has been suspected, that some gold may be dissipated by the blast on the cupels, in refining it from the antimony remaining in it. But this is not so probable; because gold has been melted several times with a greater proportion of regulus of antimony simple, than is contained in the golden regulus, and refined from it with the greatest heat and blast that could be given, without any loss. And it is the constant practice of some refiners, to give their fine gold a higher colour for gilding, to put a third or fourth part of crude antimony, or of regulus of antimony, and with a great heat and strong blast work it off; in which operation, in some ounces of gold, they lose not one grain.

II. Again, there was taken 141½ grains of crown gold, which was melted with 1 ounce and ¼ of antimony. The regulus weighed 123 grains. From this, a piece weighing 30 grains was broken off, and reserved for refining by itself; the remainder, being 93 grains, was melted down again with the same antimony, being powdered and put on the top: and then the regulus came forth, weighing 91 grains: so that here was no considerable loss. And there is ground to suspect, that it might be upon some accidental difference in the managing, that the regulus did not so perfectly separate and settle: for in all other experiments of melting the same regulus again with the same antimony, the regulus gained weight; as in the next following:—

From this second regulus, a piece broken off, and reserved for refining apart, weighing 36 grains; the remainder, being 55 grains, was melted down as the former, and in the same antimony. Whereupon the regulus came forth in weight 72 grains; 17 grains being here gained to 55 grains, making the whole 72 grains, i. e. between ¼ and ⅓.

The first piece of 1 pennyweight and 6 grains, being refined on the cupel, produced of fine gold 1 pennyweight just: which holds in proportion as 24 to 30. So that it contains ⅘ of gold, and but ⅕ of antimonial substance in it.

The second piece, weighing 36 grains, being refined on the cupel, produced of fine gold 28 grains, in proportion of 28 to 36, which is rather less than ⅘, as in the former; but the difference is inconsiderable for quantity.

The regulus, upon the third melting, weighing 72 grains, refined on the cupel, produced of fine gold 55 grains. This holds in the proportion of ⅘: but somewhat short of the former.

On these comparisons, in this experiment of repeating the melting of the regulus with the same antimony, the regulus gains weight each time, but is in proportion less rich in gold: both which are contrary, in repeating the melting of the regulus with fresh antimony, as in the former experiments.

The remaining antimony being reduced to a regulus by firing with nitre and tartar, of each equal weight to itself, and that regulus exhaled on the cupel, there remained 19 grains of gold. This was less fine than that obtained from the first antimony, in the former experiment of passing gold through several parcels of antimony; though losing little sensibly in weight, upon melting with a strong heat and blast upon it. So that the impurity was not from any remaining antimonial substance in it; but from the silver and copper mixed with it in the first alloy.

III. A parcel of crown gold, weighing 82½ grains, was melted down with an ounce of antimony; and the antimony exhaled in the crucible to a regulus. Then the antimonial part of that regulus was exhaled on a cupel. Whereupon there remained 84 grains: which was more than the first gold by 1½ grain. This must happen for want of a heat strong enough at last to force off all the antimonial substance. Whence afterward, upon melting in a crucible, it came short 4 grains; 80 grains, which was but 2½ grains short of the first quantity, and is the least part of the proportion of copper that must be in it, according to the usual alloy of crown gold: which is generally two parts to one of silver, or at least the half. So that antimony, in a far greater proportion, does not so much as lead in exhaling or separating copper from gold, if the work be done merely by exhalation; but only retains it with itself, whilst the gold separates and settles in a regulus at the bottom. Neither is it so destroyed, but that it may, in part at least, be united to the gold again. That there remained copper in this gold, appeared farther by the black complexion of it on annealing. As also by the loss on working it with lead on a cupel: whereupon it came forth 76 grains, i. e. 4 grains short.

Account of a monstrous Birth, communicated by Dr. S. Morris. N° 138, p. 961.

This monstrous female birth had two heads, both the faces very well shaped. The left face looked swarthy and never breathed; that head was also the larger. The right head was perceived to breathe; but not heard to cry. Between the heads was a protuberance, like another shoulder. The breast and clavicles very large; about 7 inches broad. It had only 2 hands and 2 feet.

The brain, in each head, was very large. The spina dorsi, from the neck to the loins, was double. There were also two hearts, one on each side the thorax. The left heart the larger. And two pair of lungs; one infolding each heart. Those in the left side were blackish; the other looked well. The mediastinum parted the two hearts from each other. The aorta and vena cava, below the diaphragm were single; the diaphragm having only three perforations, as is usual. But a little above it they were each divided into two branches, distributed to the two hearts, in the figure of a Greek Υ. The œsophagus in like manner, a little above the diaphragm, viz. about the fifth vertebra, was divided into two branches, one ascending up into each throat. There were also two stomachs or ventriculi. One shaped as in a natural birth; the other a kind of great bag, larger than the natural ventricle. In which respect it answered to the paunch in a cow or sheep; but in regard of its place, rather to the reticulus, or else to the abomasum; being at the one orifice continuous with the true pylorus, and at the other with the duodenum. Within it was contained a substance like meconium, as is usual in children newly born. The liver was single, but very large, and the cystis fellea proportionable. The spleen also, one, but large. So were the intestines, and all the parts of the lower ventricle, especially the left kidney. The uterus of the usual size; but the clitoris large. The secundine extraordinarily great, weighing about 8 pounds.

An Account of three Books. N° 138, p. 963.

I. The Royal Pharmacopœia, Galeno Chemical, according to the Practice of the most eminent and learned Physicians of France, and published with their several approbations. By Moses Charras, the King's chief Operator in his royal garden of plants. In English.

A translation of Charras's Pharmacopœia was noticed in the first volume of this Abridgement.

II. Decameron Physiologicum; or, Ten Dialogues of Natural Philosophy. To which is added the Proportion of a straight Line, equal to half the Arch of a Quadrant. By Thomas Hobbs, of Malmsbury.

The first of these dialogues is on the origin of natural philosophy. 2. The principles and method. 3. Of a vacuum, which he denies. 4. On the system of the world. 5. On the motions of water and air, on clouds, springs, &c. 6. Of heat and cold. 7. On hardness, softness, elasticity, &c. 8. Gravitation. 9. Magnetism. 10. Transparency and refraction. Lastly, a supposed demonstration of a right line, equal to the quradrantal arc of a circle.

III. Mechanic Exercises; or, The Doctrine of Handy Works. Began Jan. 1, prosecuted in two other Essays, Feb. 1, and March 1, 1677; and intended to be continued monthly. By Joseph Moxon, Hydrographer to the King.

A curious and ingenious performance, in which most handycraft works have been very usefully explained, and illustrated with appropriate figures of the various tools and instruments.

An Occultation of Saturn by the Moon, Feb. 27, 1678, N. S. Observed at Paris by M. Bulliald. N° 139, p. 969.

Bulliald observed the beginning of this occultation at Paris, at a time when the observed altitude of the star in the head of Andromeda, above the western horizon, was 18° 11′; whence it follows that the astronomical time from noon was given equal to $7^h\ 20^m$; but the mean time was $7^h\ 29^m\ 55^s$. And he saw the end when the more southern star of Andromeda's girdle, of the second magnitude, was 21° 17′ high to the west. Whence the astronomical time from noon is found to be $8^h\ 30^m\ 22^s$.

M. Bulliald gave calculations of this occultation from several astronomical tables then in practice, not of any use now to be reprinted. These calculations commonly show a considerable difference from the real observed times. Particularly, the philolaic tables make Saturn 19′ farther advanced in longitude than he really is by observation; so that Saturn was then in ♊ 3° 28′, with 1° 38′ south latitude.—Further, in this observation by having recourse to the description of the lunar disk, as given by M. Hevelius, we found Saturn to emerge in that part of the limb, which is situated in a right line drawn from the middle of Mount Berosus through the Riphæan mountains, a little above Mount Alanus, and below the southern limits of the hyperborean marshes.

Of Red Snow seen at Genoa. By Sig. Sarotti, the Venetian Resident here, and by him communicated to the Honourable Mr. Boyle. N° 139, p. 976.

On St. Joseph's day, on the mountains called Le Langhe, there fell on the white snow, that lay there before, a great quantity of red, or if you please of bloody snow. From which, being squeezed, there came a water of the same colour.

*Anatomical Observations on the Structure of the Nose, made by Mons. du Verney.** N° 139, p. 976.

Mons. du Verney observes, that the cavities of the nose are filled with many cartilaginous laminæ distinct from each other; every lamina being divided into

* Guichard Joseph du Verney was born at Feurs in Forez, 1648. He prosecuted his medical studies at Avignon, and afterwards removed to Paris; where he was chosen member of the Academy of Sciences, and was appointed to demonstrate the structure of the different parts of the human body before the Dauphin. He was also elected to the anatomical chair in the king's garden. He died at Paris in 1730, aged 82. His principal work is on the organ of hearing, published in French, and afterwards translated into Latin, English, and German. Besides this treatise, Du Verney wrote se-

many others, all folded almost into a spiral line; and that the os cribrosum is made up of the extremities of these laminæ, which terminate in the root of the nose, the holes with which it is pierced, being the intervals between the laminæ. That they are designed to support the inner tunic of the nose, which being a principal organ of smelling, has received from nature a very great expansion; for the commodious placing of which nature has folded it round about together with these laminæ, that by this economical mechanism she may employ all its length in a very little room. This tunic is filled with innumerable branches of arteries, veins, and nerves; by which it has a most exquisite sense. Yet because the particles of odorous bodies are so subtile, that they can but very gently glance on the organ, nature has therefore provided by this great expansion, that the greater number of these particles may strike it at the same time, and so render their impression the stronger. And that these odorous particles which rush with the air into the nose in smelling, might not all forthwith pass off from thence into the breast; nature, by this labyrinth, made by the windings of the lamellæ, has given them an arrest and longer stay. And for the same reason, she has furnished the said tunic of the nose with a great many small glands, which open into it, and so moisten it with a thick and slimy exudation, the better to entangle the dry odorous particles. This tunic examined and compared in several animals, shows also much of the reason of the delicacy of smelling in some, above what it is in others. For by how much a finer nose it is that animals have, by so much likewise is there a greater number of these lamellæ, wherewith the said tunic is rolled up in so many more folds. So the nose of a hound is better furnished with them than that of any other animal. The hare, fox, cat, wild boar, have a considerable number of them. Those animals that chew the cud have fewer. And man is less provided for than any of the rest. And not only the number, but also the length of the lamellæ is of great use for the strength of smelling. For which purpose most quadrupeds which either hunt, as the carnivorous, or at least want reason otherwise to distinguish their food than by the smell, as the graminivorous, have their nose not placed in the middle of the face, as in man, but prolonged to the very end.

veral memoirs, chiefly on anatomical subjects, of which mention is made in the History of the Academy of Sciences, and in the Journal des Sçavans; from which it will be seen that he bestowed no inconsiderable pains on comparative anatomy. He left by his will his large and valuable collection of anatomical preparations to the Academy of Sciences. His nephew, James Francis du Verney, was likewise a celebrated demonstrator of anatomy. He undertook a splendid work on the muscles, the plates of which exhibit those parts as large as in the real subject.

Observations in Congo and Brazil. By Michael Angelo de Guattini and Dionysius of Placenza, Missionaries thither. N° 139, p. 977.

In Brazil there are certain little animals called poux de pharaon,* Pharoah's lice, which enter into the feet between the skin and the flesh. They grow in one day as large as beans, and if not presently drawn out they cause an intolerable ulcer, and the whole foot corrupts.

Amongst other fair fruit trees in Brazil there is one, whose fruit is called niceffo, which has only two leaves; each of which is large enough to cover a man.

In the kingdom of Congo there are serpents† 25 feet long, which will swallow at once a whole sheep. The manner of taking them is thus: when they lie to digest what they have eaten, they stretch themselves out in the sun, which the blacks seeing kill them. And having cut off their head and tail, and embowelled them, they eat them; and usually find them as fat as hogs. There are here a great number of ants, and so large, that the author reports that being one day sick in his bed he was forced to order himself to be carried out of his room for fear of being devoured by them; as it often happens to those of Angola, where may be seen in the morning the skeletons of cows devoured by these animals in one night.

On the Sorbus Pyriformis. By Mr. Edmund Pitt. N° 139, p. 978.

Last year I found a rarity growing wild in a forest of Worcester. It is described by L'Obelius under the name of sorbus pyriformis,‡ also by Mathiolus upon Dioscorides, and by Bauhinus, under the name of sorbus procera. And they agree, that in France, Germany, and Italy, they are commonly found. But neither these, nor any of our own countrymen, as Gerard, Parkinson, Johnson, How, nor those learned authors Merret or Ray, have taken notice of its being a native of England. Nor have any of our English writers so much as mentioned it. Only Mr. Lyte, in his Translation of Dodonæus, describes it under the name of the sorb-apple. But says no more of the place, but that it grows in Holland.

It resembles the ornus or quicken tree; only the ornus bears the flowers and fruit at the end, this, on the sides of the branch. Next the sun, the fruit has a dark red blush, and is about the size of a small jeneting pear. In September,

* Chigger or chego. Pulex penetrans. *Linn.*

† Boa constrictor. *Linn.*

‡ Sorbus domestica. *Linn.*——Pyrus domestica. *Smith Engl. Bot.* 350.

so rough as to be ready to strangle one. But being then gathered and kept till October, they eat as well as any medlar.

Of a Child which remained Twenty-six Years in the Mother's Belly, out of the Uterus. By Mons. Bayle, M.D.* N° 139, p. 979.

Margaret Mathew, wife of John Puget, shearman, being with child 1652, perceived about the end of the ninth month of her bearing such pains as women usually have when about to fall in labour. Her waters also broke, but no child followed. For the space of 20 years she perceived this child to stir, with many troublesome symptoms. Which made her from time to time to desire the surgeon to open her belly, and take out this grievous burthen. But for the last six years she perceived not the child to move. Being lately fallen sick she requested the surgeon to open her when she should be dead. She died June 18, this year 1678. She was opened the next day, and a child was found in her belly, out of the womb, no way attached to it. The head was downward; and the buttocks hanging toward the left side. All the back part of this child was covered with the omentum, which was about two fingers thick, and stuck hard to divers parts of the body of it, not to be separated without a knife; which being done, very little blood issued. This infant weighed 8 pounds avoirdupois. The skull was broken into several pieces. The brain of the colour and consistence of ointment of roses. The flesh red where the omentum adhered, other parts whitish, yellowish, and somewhat livid, except the tongue, which had the natural softness and colour. All the inward parts were discoloured with a blackishness, except the heart, which was red, and without any issuing blood. The forehead, ears, eyes, and nose, were covered with a callous substance, as thick as the breadth of a finger. The gums being cut, the teeth appeared in the adultness of those in grown persons. The body had no bad smell, though kept 3 days out of the mother's belly. The length of the body from the buttocks to the top of the head about 11 inches. The mother died about the 64th year of her age.

An Account of some Books. N° 139, p. 980.

I. Johannis Wallisii, S.T.D. in Celeberr. Academia Oxoniensi Geometriæ Professoris Saviliani, Exercitationes Tres. 1. De Cometarum Distantiis investi-

* This physician, Francis Bayle, held a medical professorship in the university of Toulouse, and was much esteemed for his learning and probity. He died in 1709 in the 87th year of his age. He was a man of a philosophical turn of mind, and was author of several other tracts besides the above-mentioned; which he republished collectively in 1700, in 4 vols. 4to.

gandis. 2. De Rationum et Fractionum Reductione. 3. De Periodo Juliana. Lond. 1678.

The first of these three problems, viz. on investigating the distances of comets, it seems, was undertaken at the request of Sir Chr. Wren, and which the author states in this problem, viz. "Four right lines being given in position in the same plane, to find a fifth, which shall be cut by the four given, so that the interposed segments shall have a given ratio." The solution of which he gives.—The second problem shows ingenious methods of reducing ratios and fractions of large terms to other equivalent ones, or nearly so, in much smaller terms, particularly exemplified in the ratio between the diameter and circumference of a circle. The third tract teaches how to find what year of the Julian period answers to any given years of these three, viz. the solar and lunar cycles, and the Roman indiction.

II. Martini Lister è Societate Regia, Lond. Historiæ Animalium Angliæ tres Tractatus. Unus, de Araneis. Alter, de Cochleis tum Terrestribus, tum Fluviatilibus. Tertius, de Cochleis Marinis. Quibus adjectus est quartus, de Lapidibus ejusdem Insulæ ad Cochlearum quandam imaginem figuratis. Lond. 1678.

The first tract contains two books. The former treats of spiders in general. As a description of their several parts, both outward and inward; of their generation; the nature and emission of their thread; casting their cuticle; of their food; their venom; several either false or dubious traditions concerning them; medicines made of them.—The second book contains a distribution of spiders into their several species, arranged in curious and compendious tables of divisions and subdivisions. He likewise exhibits the figures; sets down the descriptions, place, time of laying, manner of coition. Describes their eggs, nests, nets, threads. Speaks of their food and manner of living; their very high ascent into the air, &c.

The second tract has three parts. The first of snails in general. As, of their shells, and other parts both outward and inward; their saliva, eggs, food; use in medicine; diet, &c.—The second of land snails.—The third of river snails. The several sorts being figured, described, and comprised by the author in tables of subdivisions.

The third tract is of sea snails, which by the author are figured and distributed into tables, like the former.

The last book, de Cochlitis Angliæ, presents the figures and descriptions of many of this kind, classed in tables like the others.

The author is rather of opinion that these figured bodies are not petrified shells, but bred in the earth, like other stones.

III. Lectures and Collections made by Robert Hooke, Secretary of the Royal Society.* 1678.

This work is divided by the learned author into two parts. The first is called Cometa: containing, besides observations of the comets of 1664, 1665, and 1677, a discourse also on comets in general. Also several other astronomical tracts, relating to the spots on the sun, and the transits of the planet Mercury, &c.—The second part is called Microscopium; containing a description of microscopes, and their employment in viewing several minute objects.

Anatomical Observations on the Body of a Woman about 50 *Years old, who died Hydropical in her left Testicle,* [*ovarium*], *Dec.* 30, 1677. *By Dr. Henry Sampson.* N° 140, p. 1000.

The woman had been married, but had never borne a child. Had been a widow for about 10 years before her death. In which time she was much oppressed with grief; and her belly by degrees began to swell; yet not much, till about 4 years before she died. In the year 1678, at which time she weighed 216 pounds, I advised her to the use of cathartic hydragogues, and diuretics, after the use of which for some time, she weighed but 200 pounds. But still the morbific matter was reaccumulated to the diseased part. So that resolving to forbear further medicines, within half a year after she weighed 250 pounds; her belly being at last so distended as to hang down, as she sat, below her knees.

On opening her, there issued about 20 pounds of a brownish water or serum from one of the vesicles of the left ovarium. Having separated the muscles of the abdomen, I found no serum or hydropic water, but a heap of bladders of several sizes presented themselves. From the largest of which there issued above 20 pounds more of a brown and thickish serum, tinctured with a sediment of the colour of umber. Some of the smaller were about the size of a child's head, and yielded a slimy serum, in consistence and colour like the mucilage of quince seeds. Others were much less, from the size of a man's fist, to that of a walnut: most of which contained a serum like the white of an egg; in some it was less viscous, like starch newly boiled.

At length I perceived that all these bladders were parts some way relating to the womb. Wherefore having separated the ossa pubis, I took out the womb with the parts appendant altogether. And then, amongst other parti-

* Mr. Hooke succeeded Mr. Oldenburg as secretary to the Royal Society in 1677. But it was Dr. Grew, elected the other secretary at the same time, who then commenced the editorship of the Philosophical Transactions in the same year, with No. 137 of that work; and published 6 Nos. ending with No. 142. After this, Mr. Hooke published 7 Nos. of what he called Philosophical Collections; and after that Dr. Plot recommenced the Philosophical Transactions in January 1682-3, with No. 143, and continued them till No. 166.

culars, observed, that the right testicle or ovary was but small, white, and its vesicles in a manner dried up. But the left swelled to a vast bulk: the aforesaid bladders, in one of which were contained so many pounds of liquor, being nothing else originally but the ova belonging to this left ovary. Imagine you saw about 40 bladders, some of a small pig, others of a hog, or a calf, and some of an ox, all distended with liquor, and tied like a reeve of onions together, and you have the appearance of this ovary.—The testicle or ovary itself, all the serum being exhausted, weighed together with the womb, which was but light, 25 pounds. Out of all the said vesicles or bladders together, were exhausted above 112 pounds of serum.

Microscopical Observations on the Structure of Teeth and other Bones, also of Hair. By Mr. Antony Leuwenhoeck. N° 140, p. 1002.

Having some time since applied a glass, esteemed a good one, to observe the structure of the teeth and other bones; they then seemed to consist of globules. But since then having drawn out one of my teeth, and for further observation applied better glasses than the former; it has plainly appeared that the whole tooth was made up of very small, straight and transparent pipes. Six or 700 of these pipes put together exceed not the thickness of one hair of a man's beard. In the teeth of a cow, the same pipes appear somewhat larger, and in those of a haddock somewhat less.

I have also observed part of the shin-bone of a calf, 6 or 8 weeks old; in which the pipes are less straight than in a tooth; and sometimes there seemed to be several lesser pipes joined together, so as to constitute a larger one.

Of the Grain of Ivory.

The author of these Observations has often taken notice of the grain of ivory; and is that which upon a due position to the falling light is visible to a naked eye. The several pieces whereof it is composed, appearing like the fibres or threads of a muscle, running in parcels, decussatim, and under and over each other reciprocally; and so making up one piece of platted work.

I have formerly also with others examined the structure of hair; and we agreed that it consisted wholly of globules; as also the hoof of an elk. But not being satisfied without further inquiry; I took the hair of my beard after it had been shaved the first, second, third, and fourth days, and observed, that the little particles which we saw through the common microscopes, which yet were very good, and which appeared round, were indeed irregular, and lay very closely pressed one upon another. Of these particles consist the outer parts or cuticle of the hair. One of these hairs I met with, which seemed rare, being on the one side convex, on the other somewhat concave, and looking like two hairs continuous or growing together.

I examined the roots of several hairs plucked out of my hand, nostrils, eye-lid, eye-brow, &c. and clearly saw that the whole root, except the cuticle, consisted of little strings, which I suppose to be veins or vessels, appearing like a common tree with all its roots.

A New Invention of a Clock ascending on an inclined Plane. By M. de Gennes. N° 140, p. 1006.

We have formerly seen clocks that never go but when they are applied on an inclined plane. But we never yet saw any clocks that wind up again of themselves on the same plane. There is to be seen in M. Cospi's study, a wooden wheel, which has the same effect on an inclined plane, invented by M. Bondoni, a Florentine secretary to the said Marquis. But M. Legati does not unfold this secret; and M. de Gennes, having found out the same, has successfully applied it to a clock, on the following principle.—Fig. 4, pl. 12, represents the inside of the machine placed on an inclined plane. The whole invention consists of a weight, which causes the machine to act after the following manner:—

The circle FGH being placed on an inclined plane AB, is divided into two unequal parts by the line GI. To restore to the least segment its equilibrium, there is fastened to the extremity of the radius DF, a weight F, which is sufficiently heavy to recover what the lesser segment loses by its situation. That a wheel or clock may thus stand not only in equilibrium, but also ascend upward, there is placed in the middle of the clock a drum, which encloses the spring of the pendulum, on which drum is fastened the radius DF. For thus the spring, being mounted, forces the drum to turn, and so raise the weight, which it cannot do without its becoming heavier, because that coming to the point E, it is farther from the centre than when it was at F; and thus the wheel turns on that side as the spring gives way.

A New Engine to make Linen-Cloth without the help of an Artificer. By M. de Gennes. N° 140, p. 1007.

This engine is no other than a mill to which are applied all the parts of a weaver's ordinary loom. It consists of four principal parts, viz. the serpent AA, two footsteps or treddles BB, one clapper C, and two arms DD, DD, fig. 5, pl. 12.—The serpent, or iron bar AA, has two elbows EE, to which the two ends of the ropes are fixed that raise and depress the treddles BB: FF are two fourths of a circle, that successively rest on two arches or bows of iron GG, which are above the clapper C, in order to raise it. HH are two teeth of iron, added to the serpent, making an angle of 25 degrees with FF and KK, which serve to depress a bascule or sweep, which is in the arm that carries the shuttle. The treddles differ in nothing from those which are commonly used, only the cords that hold them pendent from the ground are fixed in the elbows of the serpent, which in turning raises and depresses them by the help of two little pulleys on which the ropes turn.—The clapper is supported between two pillars, with a rope double twisted, which causes it to make a kind of a spring, and naturally to yield forwards, to beat the cloth.—LM is one of the arms which pass freely into the canal or pipe NN, supported by four pillars of wood OOOO. Its motion proceeds from the following parts: PQ is a bascule which, though unequally divided by its supporter R, is yet in equilibrio, the end PR being made to weigh exactly as much as RQ.—At the extremity of this bascule is tied a cord, which passes through the pulley S, and terminates at the extremity of the arm, where it is fastened to a little bowl M. At the other extremity of the same arm, towards L is also fastened underneath a cord, which passes through the pulley T, and carries the weight V.—At the same end of the arm is added a little nich Z, about the size of half the shuttle: then over a little bar XY, which passes athwart the arm, there are two other little pieces of wood, having at the end of them two teeth, which enter into the nich Z through two holes, which are on both the sides.

To the ends of these little pieces of wood there is a small bow of whalebone or steel, which keeps the two ends asunder, and forces the teeth, which are at the other end, to enter into the nich before the said pieces themselves. At the points II are two ropes, that pass through the pulley 22, fastened to the pillars 0 3 0 4, having each of them a little weight at the end large enough to keep it from passing through a little bowl which is under each pulley.

This arm so disposed goes and comes in the hole NN in the following manner. One tooth of the serpent already described, strikes on the extremity of the bascule PQ, and so causes the end Q to rise up, which drawing the cord fastened to the point Q, makes the arm LM to advance forward. But when afterwards the tooth of the serpent is come out, then the weight V tied to the other end of the same arm by a cord, that passes through the pulley T, forces the said arm by its own weight to return again.—When the arm LM is in its ordinary place, the two little pieces of wood into which the bar XY enters, enclose the shuttle by means of the whalebone spring. But when the said arm approaches the other opposite arm, then the cords tied to the points II being a little too short, and the weight which is at the end of them not being able to pass through, the spring gives way a little, and so the shuttle is no longer enclosed by the arm which carries it, but is wholly received and grasped by the other; which likewise in its turn delivers it back again in the same manner.

The motion of the whole machine is made at the rate as you move the handle of the serpent; for then the arms cause the threads to open, and immediately one of the arms begins to slide in towards the opposite arm, to which it carries the shuttle, and retires immediately. At the same time one of the quarters of a circle which held the clapper elevated quits it, and leaves it to flap, and then the opposite quarter of a circle elevating itself, the other elbow changes the threads, and the other arm retires, and so on successively.

The advantages of this machine are these; 1. That one mill alone will set 10 or 12 of these looms at work. 2. The cloth may be made of what breadth you please, or at least much broader than any which has been hitherto made.—3. There will be fewer knots in the cloth, since the threads will not break so fast as in other looms, because the shuttle that breaks the greater part can never touch them. In short the work will be carried on quicker and at less expence, since instead of several workmen which are required in making of very large cloths, one boy will serve to tie the threads of several looms as fast as they break, and to order the quills in the shuttle.

Of a Worm Voided by Urine. Communicated by Mr. Ent; to whom it was sent by Mr. Matthew Milford. N° 140, p. 1009.

The worm when I voided it was then alive. It was snake-headed, of indifferent substance in the middle, and small at the tail. In length above half a yard. I was very ill before it came away; and ever since the urine seems mixed with blood.

This relation is here set down in the patient's own words. It is most probable he had had a suppression of urine for sometime, at the first making whereof the worm was voided from one of the kidneys, wherein it was bred, into the bladder; and at the second from thence into the pot.—The worm when dead and dry was of a dull red colour, and in thickness about the 12th of an inch.

To make a probable Conjecture of Tempers and Dispositions by the Modulations of the Voice in ordinary Discourse. Communicated also by Mr. Ent. N° 140, p. 1010.

Sitting in some company, and having been but a little before musical, I chanced to take notice that in ordinary discourse words were spoken in perfect notes; and that some of the company used eighths, some fifths, some thirds; and that those were most pleasing, whose words, as to their tone, consisted most of concords; and where of discords, of such as constituted harmony; and the same person was the most affable, pleasant, and the best natured in the company. And this suggests a reason why many discourses which one hears with much pleasure, when they come to be read scarcely seem the same things.

From this difference of music in speech, we may also conjecture that of tempers. We know the Doric mood sounds gravity and sobriety; the Lydian freedom; the Æolic sweet stillness and composure; the Phrygian jollity and youthful levity; the Ionic sooths the storms and disturbances arising from passion. And why may we not reasonably suppose that those whose speech naturally runs into the notes peculiar to any of these moods, are likewise in disposition.

So also from the cliff; as he that speaks in gamut to be manly; C Fa Ut may show one to be of an ordinary capacity, though good disposition. G Sol Re Ut to be peevish and effeminate, and of a weak and timorous spirit. Sharps an effeminate sadness; flats, a manly or melancholic sadness. He who has a voice in some measure agreeing with all cliffs, seems to be of good parts and fit for variety of employments, yet somewhat of an inconstant nature. Likewise from the times; so semibriefs may bespeak a temper dull and phlegmatic; minims grave and serious; crochets a prompt wit; quavers vehemency of passion, and used by scolds. Semibrief-rest may denote one either stupid or fuller of thoughts than he can utter; minim-rest one that deliberates; chrochet-rest, one in a passion; so that from the natural use of mood, note and time, we may collect dispositions.

Account of some Books. N° 140, p. 1011.

I. Museo Cospiano annesso a quello del famoso Ulisse Aldrovandi * et donato alla sua Patria dall' Illustrissimo Signore Ferdinando Cospi Patricio di Bologna

* Ulysses Aldrovandus, professor of philosophy and physic at Bologna, the place of his nativity, was a most curious inquirer into natural history, and travelled into the most distant countries on purpose to inform himself of their natural productions, whether animal, vegetable, or mineral; but his principal attention seems to have been devoted to zoology, and in particular to ornithology, in the pursuit of which branch he expended great sums, having according to Aubert le Mire, engaged at a yearly salary of two hundred crowns, continued for the space of thirty years together, an eminent artist to execute his figures of birds. He is also said to have employed, at his own expence, Lorenzo Bennini, Cornelius Swintus, and the eminent engraver Christopher Coriolanus. These expences ruined his fortune, and at length reduced him to the utmost necessity, and it is said that he died blind in a hospital at Bologna, at a great age, in the year 1605. Bayle observes, that antiquity does not furnish us with an instance of a design so extensive and laborious as that of Aldrovandus, with regard to natural history. Pliny indeed has treated of more subjects, but only touches them lightly, whereas Aldrovandus has collected all he could. His compilation, or at least what was compiled upon his plan, consists of several volumes in folio, some of which were printed after his death. He himself

et Senatore, &c. Descrizzione di Lorenzo Legati Cremonese, in fol. In Bol. 1678.

Mr. Ferdinand Cospi, marquis of Petreoli, equally illustrious for his merit, the employments wherewith he is honoured in the court of Tuscany, and for his extraordinary learning, which has raised him to one of the highest degrees in the academy of the Gelati in Bononia, having with extraordinary care and expence made a collection of whatever he saw there that was curious and rare, and bestowed it upon his country, the Senate of Bononia has added the same to that of Aldrovandus. An ample and learned description whereof is here made by M. Lorenzo Legati, philosopher, physician, and Greek professor in the university of Bononia.

He divides the work into five books.

The first contains a description of whatever this museum contains of rarity concerning mummies, beasts, serpents, birds and human monsters.—The second book contains descriptions and remarks of several rarities concerning aquatiles, as of the flying fish, &c. also of corals, pearl, &c.—The subjects of the third book are works of art.—The fourth and fifth books are concerning the medals and gods of the ancients.

II. Systema Bibliothecæ Collegii Parisiensis Soc. Jesu. In 4to. A Par. 1678.

Of which library the author of the said journal says, that it contains above 32,000 volumes.

III. Glossarium ad Scriptores mediæ et infimæ Latinitatis, in quo Latina Vocabula novatæ significationis explicantur; complures ævi medii Ritus et Mores; Legum, Consuetudinum Municipalium, et Jurisprudentiæ recentioris formulæ et obsoletæ voces; utriusque Ordinis Ecclesiastici et Laici Dignitates et Officia, &c. enucleantur et illustrantur; innumera denique Scriptorum loca Græcorum, Gal. Lat. Ital. Hispan. German. Anglo-Sax. expenduntur, emendantur, elucidantur. In fol. 3 vol. Autore Carolo du Fresne Domino du Cangi * Regi a Cons. et Franciæ apud Ambianos Questore. A Par. 1678.

published his Ornithology, in three volumes, in 1599, and his seven books of Insects, which make another volume of the same size. The volume of Serpents, three of Quadrupeds, one of Fishes, that of Exsanguious Animals, the History of Monsters, with the Supplement to that of Animals, the treatise on Metals, and the Dendrology or History of Trees, were published at several times, after his death, by the care of different persons, viz. The volume of Serpents by Bartholomæus Ambrosinus; that of Bisulcous Quadrupeds by John Cornelius Uterverius, and afterwards by Thomas Dempster; that of Digitated Quadrupeds by Ambrosinus; the History of Monsters by the same author; the Dendrology by Ovidius Montalbanus; and the History of Metals by Ambrosinus.

* Du Cange, a learned French lawyer, was born at Amiens in 1610. He became advocate in the parliament of Paris, and treasurer of France. He was well acquainted with history, both sacred and profane, and left many works, both printed and manuscript, at his death, which happened in

This glossary of M. du Cange is now completed, and posseses great merit. The work contains above two thousand observables; with several learned dissertations on divers curious and profitable subjects.

IV. Explication Nouvelle et Mechanique des Actions Animales, ou il est traitè des fonctions de l'Ame, &c. Par. M. Duncan, D. en Med. In 12mo. a Par. 1678.

Because the knowledge of the functions of the soul, and animal motions, depends much on that of the construction of the brain; this author therefore teaches the dissection hereof, after such a manner, as seems more natural than that of Sylvius, Bartholine, or Willis, although they have all done excellently well.

An Eclipse of the Moon of Oct. 29, N. S. 1678, observed at Paris. By M. Cassini. N° 141, p. 1015.

	h	m	s
The beginning of the shadow was at	6	43	35
The total immersion	7	40	20
Beginning of the emersion	9	21	30
End of the eclipse	10	20	5

Mons. Gallet's Observation of the Solar Eclipse on the 11th of June, N. S. 1676, at Avignone. N° 141, p. 1020.

In observing this eclipse, the image of the sun, through the telescope, was received on a whiter paper, and the appearances carefully noted. The times were corrected by the altitudes of the sun, taken by the shadow of a gnomon, erected for the purpose.

The following were the principal phases and times.

Digits eclipsed.						
0°	27′	at	7^h	50^m	34^s	
6	0	....	8	44	44	the horns were vertical.
4	35	....	9	42	3	the horns were horizontal.
0	20	....	10	25	12	western horn vertical to the sun's centre.
0	0	....	10	28	50	end of the eclipse.

The proportion of the diameters appeared equal at the eclipse of 6 digits; for then the sun's vertical horns were distant on each side from his vertical

1688, at 78 years of age. Besides the above Latin Glossary, which has been often printed, his Greek Glossary of the middle age, in 2 vol. fol. is also an esteemed work. He published also a history of Constantinople, and various other books, showing extensive reading and deep contemplation.

about 30° : whence it appears that the moon's centre was then in the periphery of the sun, and that the line through the centre was equal to the sun's semi-diameter. But after the middle of the eclipse there was found some change in the diameter of the shadow; for the shadow appeared a little more convex, so that the diameter was shorter, though almost insensibly.

The making of Microscopes. By Mr. Butterfield, Instrument maker to the French King. N° 141, p. 1026.

I doubt not but you may be as busy at London, as we are here, in making of microscopes of the kind lately brought out of Holland by Mr. Huygens, of which I have several sorts ready made. I have tried several ways for the making of glasses of the size of a large pin's head, and less; as in the flame of a tallow-candle, and of one of wax. But the best way of all I have yet found, to make them clear and without specks, is with the flame of spirit of wine well rectified, and burned in a lamp. Instead of cotton, I make use of very small silver wire, doubled up and down like a skein of thread; which being wet with the spirit of wine, and made to burn in the lamp, gives through the veril of the lamp a very ardent flame. Then take the beaten glass, being first washed very clean, on the point of a silver needle filed very small, and wet with spittle. Hold it thus in the flame till it be quite round, and no longer for fear of burning it; and if the side of the glass next the needle be not melted, you may put it off and take it up with the needle on the round side, presenting the rough side to the flame till it be every where very round and smooth; then wipe and rub one or several of them together with soft leather, which makes them much the better. Then put them between two pieces of thin brass, the apertures very round, and that towards the eye almost as large as the diameter of the glass; and so place it in a frame with the object conveniently for observation.

Improvement of Sir Samuel Moreland's Speaking Trumpet, &c. By Mr. John Conyers. N° 141, p. 1027.

Having some years since tried to make one of Sir S. Moreland's speaking trumpets of tin, that is, tinned iron plate; and finding it to serve, as well as copper or glass; I next thought of several ways for reducing it to a more contracted form, without abating its power. In consequence Dr. Goddard presented to the Royal Society, at one of their meetings, held at Arundel House, the reflecting trumpet here described. It consists of two parts. The utmost Bb (fig. 6. pl. 12.) is a large concave pyramid, about a yard long, or of any manageable length, open at the base b, and closed, not with a flat, but a concave head, at the cone B. Within this is fastened a bended tube Aa, as in the figure. On trial before the Royal Society it was found, that this trumpet distinctly delivered certain words from the said house across the garden and the river Thames, and that against the wind, which was then strong: and the words were written down by a person sent over for that purpose. By which it appeared, that a reflecting trumpet after this, or some other like manner, of wood, tin, pewter, stone or earth, or which may be best, of bell metal, will send the voice as far as the long one invented by Sir Sam. Moreland, or farther. Besides, that it seems to take off from the great noise near at hand, which happens in the use of the long trumpet; so that it may be used within doors, with advantage, on several occasions.

Some other trials were made to effect the above mentioned contraction, which were found not to answer. Yet because they may serve, in part, to shew the motion of sound, I have added two examples hereof. The first is Sir Samuel Moreland's trumpet angularly arched in the middle, fig. 7; the second, with three large angular arches reaching almost from one end to the other, as in fig. 8: by the former of which the delivery of sound, to any distant or remote place, is much shortened; but by the latter almost wholly obstructed.

An Account of two Books. N° 141, p. 1030.

I. A Discourse of the State of Health in the Island of Jamaica, with a Provision calculated for the same, from the Air, the Place, and the Water; the Customs and Manner of Living, &c. By Thomas Trapham, M. D. Coll. Med. Lond. Soc. Hon.

This treatise on the climate, diseases, and natural productions of Jamaica, is superseded by the later and more ample accounts of Sloane and Browne.

II. Catalogus Stellarum Australium: sive Supplementum Catalogi Tychonici, exhibens Longitudines et Latitudines Stellarum fixarum, quæ prope Polum Antarcticum sitæ, in Horizonte Uraniburgico Tychoni inconspicuæ fuere, accurato Calculo, ex Distantiis supputatas, et ad Annum 1677, completum correctas. Cum ipsis Observationibus in Insula S. Helenæ (cujus Latitudo 15 gr. 55 m. Austr. et Longit. 7 gr. 00 m. ad Occasum à Londino) summâ Curâ et Sextante satis magno de Cœlo depromptis. Opus ab Astronomicis hactenus desideratum. Accedit Appendicula de Rebus quibusdam Astronomicis, notatu non indignis. Authore Edmundo Halleio, è Coll. Reg. Oxon.

The author having found that the astronomical tables in use, are defective in calculating the motions of celestial bodies; so that Saturn for instance moves much slower, and Jupiter swifter, than appears by those tables. Hence he had thoughts of correcting them; but presently foresaw that could never be well done, without a more correct catalogue of the fixed stars, a task which was already undertaken by other excellent hands. He therefore chose rather to take upon himself the stating of the places of the fixed stars near the south pole, and out of our horizon. And being approved and encouraged by such respectable persons as Lord Brouncker, Sir Joseph Williamson, Sir Jonas Moore, and others, and even by the King also, he furnished himself with such instruments as were necessary for his purpose: which he particularly mentions and describes. Of these, he says, he made the utmost and most assiduous use that could be, in a place of so thick and cloudy a sky as that of St. Helena, contrary to common report, proved to be; having restored about 350 fixed stars, which were omitted in Tycho's catalogue.

To his own observations the author has added an ancient catalogue out of Clavius's Commentaries in Sphæram Jo. de Sacrobosco; and that of Bartschius è Tabulis Rudolphinis Kepleri: that being compared with these his observations, it might evidently appear how very much the ancient globes almost every where differ from the heavens. From these observations, as he proceeds, he also proposes some conjectures of the corruptibility, or at least the mutability of the fixed stars. Next follows a table of the right ascensions of the southern fixed stars, and their distances from the pole: for the use of navigators. To which is subjoined an observation of Mercury by our author, scil.

Mercurii Transitus sub Solis Disco. Oct. 28. Anno 1677. Cum Tentamine pro Solis Parallaxi.

Of his conjectures here made about the sun's parallaxis, in his preface he says, that were the place of Mercury's node once found, from this his observation of Mercury, the sun's parallaxis might be deduced.

Hereto are added, by our author,

Modi quidam penè Geometrici pro Parallaxi Lunæ investiganda.

Of which there are three proposed; of which it is remarked that the best way of finding the same, would be, by comparing the meridian altitudes of the moon, observed both in St. Helena and in Europe at the same time.

The concluding chapter is entitled,

Quædam Lunaris Theoriæ Emendationem spectantia.

Wherein it is observed that astronomy is at present most of all defective. And that the discovery hereof would lead us to the most exact way of finding the longitude of places.

Anatomical Observations on an Abscess in the Liver; also a great Number of Stones in the Gall-Bag and Bilious Vessels; an unusual Conformation of the Emulgents and Pelvis; a strange Conjunction of both Kidnies; and great Dilatation of the Vena Cava. By Edw. Tyson, A. M. and M. S. Oxon.* N° 142, p. 1035.

The anatomy of morbid bodies, as Dr. Harvey has observed, is most instructive; thereby we become acquainted not only with the many causes that oppress nature, but with the liberty she often takes in forming the parts different from her usual rule; our present subject affords both. For on lately opening the body of a clergyman of this city, we observed the liver to be very large and fastened to the diaphragm more than usually; the colon so firmly joined to the liver near the gall-bladder, that I could not separate it without incision. The gibbous part of the liver towards the right side, appeared discoloured, where making an incision there plentifully issued out a perfect pus, very fœtid; and the same from a wound I made in its cavous part near the fissure. This purulent matter I found not contained in any particular cystis or bag, but in several sinuses in that part of the liver; whereas the other parts seemed sound and well coloured. Nor did I any where meet with any tubercules, glandules, or scirrhus.

This abscess may well be presumed the cause of that lurking fever that ended in death. The patient laboured under it about six weeks, but without much sickness, though troubled with irregular heats, yet sometimes such as were imperceptible to himself: twice or thrice, but at great distances, he had paroxysms of chill fits like an intermittent fever, but such a fœtor and dryness in his throat as proved obstinate to all medicines. His approaching death was attended with other symptoms that usually follow the affection of the brain and genus nervosum. Formerly he had been often subject to the yellow jaundice; and it is well worth the enquiry, why at present nothing thereof appeared? since the gall bladder was not only filled with stones, but likewise the meatus cysticus and ductus communis even to the duodenum were very much distended with them; in the porus bilarius also I met with several small ones. No fluid

* Edward Tyson was a celebrated Physician and Anatomist of the 17th century, and a great contributor to the Philosophical Transactions, especially on subjects relative to natural history and comparative anatomy. He read lectures at Gresham College. Besides his numerous communications to the Royal Society, he published the following works: Phocœna, or an Anatomy of a Porpus, 1680. Carigueya seu Marsupiale Americanum, or the Anatomy of an Opossum dissected at Gresham College, 1698, (of which an account is also inserted in the Philosophical Transactions.) The Anatomy of a Pigmy, compared with a monkey, an ape, and man, &c. 1699.

gall was contained in the bladder, but some that was soft of a deep yellow ochre colour that filled up the interstices of the stones. These stones were of various sizes, from that of a nutmeg, to a pepper corn: their colour was of a darkish yellow ochre, although in some there appeared laminæ of a browner colour: to the touch, when a little dry, they seemed soapy; their weight was light, and their scent very fetid, resembling that of the purulent matter in the liver. Their consistence was friable; their figure for the most part triangular, or inclining to that figure, but all angular; that side towards the gall bag was protuberant and convex, the other two sides were flat; so that having the lesser angle towards the centre of the cavity of the gall bag, like so many wedges, they more completely filled it: I numbered I think above thirty.

Our inquiry thus far had informed us of the cause of the patient's death, as well as of his former illness, and frequent disposition to the jaundice. But prosecuting our search, we were more surprised to observe the unusual structure and conjunction of both kidneys, the parenchyma of the one being continued over the spine unto the other, so that they both made but one continued semilunary body. This although rare, yet hath been sometimes observed by former authors. The kidneys here were large; that part that conjoins them, and lies over the spine, was something less than the true kidneys, and in its outward tunicle or membrane had three seams, although that parenchyma inwardly seemed not to observe such a division, but was the same with the substance of the kidneys. The emulgent vessels were very numerous: for besides two larger veins that were subdivided into several lesser ramifications, there were divers other that were single, even to their insertion into the vena cava. The middle part likewise, by which both kidneys were conjoined, was plentifully provided with blood vessels, for it received from the aorta two arteries, which before their insertion, were each subdivided into three branches; and it sent out two veins, which being joined afterward into one, entered the vena cava. Besides at the seam at the lower part of the left kidney, it had a vein and artery, which afterwards inserted themselves into the iliac branches of the aorta and cava; so that nature, though erring from her wonted rule in forming this part, yet was provident in furnishing it with vessels. But to the whole compages of the kidneys, there belonged only two ureters, but the great dilatation of the pelvis in each was remarkable; for that of the left kidney when blown up, was larger than it is represented in the figure, and had a triple origin. The right had but a single one, and was less.

Whether this conformation and structure of the kidneys and its vessels were of much inconvenience to the patient, I shall not define; but am apt to think, that it might occasion as well the great dilatation of the vena cava, as also of the pelvis: for the middle part conjoining both the kidneys lying over the vena cava, by its weight pressing thereon, would hinder the free return of the blood, which yet would make room for itself, by enlarging its own channel, which was so capacious as to contain three or four of my fingers. So likewise the ureters running over that part that conjoins the kidneys, like strings over the bridge of a viol, in some position of the body they might have their passage so straightened, that the urine being impeded and regurgitating, might swell and stretch the membrane of the pelvis to this size.

In fig. 1. pl. 13, A is the right kidney, B the left; C the middle part conjoining both kidneys; D, E, F, three seams in the coat of the kidneys; G the aorta; H two arteries from the aorta, which afterwards are ramified into three, and so inserted into the said middle part; I the vena cava; KK two veins arising from the middle part, which, uniting into one, entered the vena cava; LM a vein and artery arising at the seam f. which at last are both inserted into the iliac branches of ne aorta and vena cava; NN the emulgent artery of both kidneys, tne ramifications of which are not here represented; OO the emulgent veins; whereof some are single, others variously ramified; PQ pelvis of both kidneys, that of the left being extremely large; RR the two ureters.

Four Ureters in a Child; and on the Glandulæ Renales. By Dr. Edw. Tyson. N° 142, p. 1039.

Having in the former observation given some remarks of the unusual structure of the kidneys, the emulgent veins and pelvis; I shall here mention what occurred to me lately on opening the body of an infant, relating to those parts, particularly of the ureters; which here I found double to both kidneys, their origination from the kidneys being at some distance from each other; but afterwards both of the same side were inclosed in a capsula or membrane, even to the bladder, where those of the right side were inserted severally, yet near each other, but on the left they seemed to enter at the same orifice. As far as I have hitherto observed, the glandulæ renales in embrios, and infants, are greater, at least proportionably, than in adults. They have a large cavity, which by blowing into them I found emptied themselves into two veins; whereof the right immediately passed into the vena cava, the left into the emulgent: besides these, they had other lesser ones from the neighbouring vessels.

In fig. 2, pl. 13, A is the right kidney, whose surface seemed to be variously divided; B the emulgent vein; C the emulgent artery; DD two ureters belonging to this kidney.

Fig. 3, represents the two ureters of the left kidney, which a little below the kidney are both inclosed in a common capsula or case, and so continued to the bladder.

Fig. 4, represents the glandulæ renales. A the glandula renalis of the right side; B that of the left side; C the vena cava; D a vein or ductus opening from the cavity of this gland and entering the vena cava; E a vein from the left glandula renalis, and is inserted into a branch of the left emulgent.

Observationes D. Anthonii Leuwenhoeck, de Natis è semine genitali Animalculis. Nec non Auctoris harum Transactionum Responsa.* N° 142, p. 1040.

Observatoris Epistola Honoratiss. D. D. Vice-comiti Brouncker, Latinè conscripta; Dat. Nov. 1677, quam ipsissimis huc transmissis verbis inserendam Auctor censuit.

Postquam Exc. Dominus Professor Cranen, me visitatione sua sæpius honorarat, literis rogavit, Domino Ham cognato suo, quasdam observationum mearum videndas darem. Hic Dominus Ham me secundo invisens, secum in lagunculâ vitreâ semen viri, Gonorrhæâ laborantis, spontè destillatum, attulit, dicens, se post paucissimas temporis minutias (cum materia illa jam in tantum esset resoluta, ut fistulæ vitreæ immitti posset) animalcula viva in eo observasse, quæ caudata, et ultra 24 horas non viventia judicabat: Idem referebat se animalcula observasse mortua post sumtam ab ægroto terebinthinam. Materiam prædictam fistulæ vitreæ immissam, præsente Domino Ham, observavi, quasdamque in ea creaturas viventes; at post decursum 2 aut 3 horarum, eandem solus materiam observans, mortuas vidi.

Eandem materiam (semen virile) non ægroti alicujus, non diuturna conservatione corruptam, vel post aliquot momenta fluidiorem factam, sed sani viri statim post ejectionem, ne interlabentibus quidem sex arteriæ pulsibus, sæpiuscule observavi, tantamque in ea viventium animalculorum multitudinem vidi, ut interdum plura quam 1000, in magnitudine arenæ sese moverent. Non in toto semine, sed in materia fluida crassiori adhærente, ingentem illam animalculorum multitudinem observavi; in crassiori vero seminis materia, quasi sine motu jacebant; quod inde provenire mihi imaginabar, quod materia illa crassa ex tam variis cohæreat partibus ut animalcula in ea se movere nequirent. Minora globulis sanguini ruborem adferentibus hæc animalcula erant: ut judicem millena millia arenam grandiorem magnitudine non æquatura. Corpora eorum rotunda, ante-

* The discovery of the spermatic animalcules has always been considered as one of the most extraordinary, which the microscope is capable of exhibiting. They are observable in the seminal fluid of almost all animals; and though the Leuwenhoeckian theory of generation be now exploded, yet the existence of such legions of animated beings affords a wide field for philosophical speculation. The subject however being now sufficiently familiar to the naturalist, it seems unnecessary to preserve more of Mr. Leuwenhoeck's paper than what is here printed; and which it seems more proper to give in the original Latin translation from the Dutch, than in English.

riora obtusa, posteriora fermè in aculeum desinentia habebant; caudâ tenui longitudine corpus quinquies sexiesve excedente, et pellucidâ; crassitiem vero ad 25, partem corporis habente prædita erant, adeo ut ea quoad figuram cum cyclaminis minoribus longam caudam habentibus optimè comparare queam; motu caudæ serpentino, aut ut anguillæ in aqua natantes progrediebantur: in materia vero aliquantulum crassiori, caudam octies deciesve quidem evibrabant, antequam latitudinem capilli procedebant. Interdum mihi imaginabar, me internoscere posse ad huc varias in corpore horum animalculorum partes, quia vero continuo eas videre nequibam, de iis tacebo. His animalculis minora adhuc animalcula, quibus non nisi globuli figuram attribuere possum, permista erant.

Siquando canes coeunt, marem à fæmina statim seponas, materia quædam tenuis et aquosa, (lympham spermaticam intelligit) è pene solet paulatim exstillare. Hanc materiam numerosissimis animalculis repletam aliquoties vidi; eorum magnitudine quæ in semine virili conspiciuntur. Quibus particulæ globulares aliquot quinquagies majores permiscebantur.

Quod ad vasorum in crassiori seminis virilis portione spectabilium observationem attinet, denuo non semel iteratam, saltem mihimet ipsi comprobasse videor. Meque omnino persuasum habeo, cuniculi, canis, felis arterias venasve fuisse à peritissimo anatomico haud unquam magis perspicuè observatas, quam mihi vasa in semine virili, ope perspicilli, in conspectum venêre.

Cùm mihi prædicta vasa primùm innotuere, statim etiam pituitam, tum et salivam perspicillo applicavi. Verùm hic minimè existentia animalia frustra quæsivi.

A cuniculorum coitu, lymphæ spermaticæ guttulam unam et alteram è fæmella extillantem examini subjeci; ubi animalia prædictorum similia, sed longè pauciora comparuêre. Globuli item quam plurimi, plerique magnitudine animalium, iisdem permisti sunt.

Horum animalium aliquot etiam delineationes transmisi. Figura 5, pl. 13. exprimit eorum aliquod vivum, (in semine cuniculorum arbitror) eaque formâ quâ videbatur, dum aspicientem me versus tendit. ABC capitulum cum trunco indicant; CD ejusdem caudam; quam, pariter ut suam anguilla, inter natandum vibrat. Horum millena millia, quantum conjectare est, arenulæ majoris molem vix superant. Fig. 6, 7, 8, sunt ejusdem generis animalia, sed jam emortua.

Fig. 9 delineatur vivum animalculum, quemadmodum in semine canino sese aliquoties mihi attentius intuenti exhibuit. EFG caput cum trunco indigitant; GH, ejusdem caudam. Fig. 10, 11, 12, alia sunt in semine canino, quæ motu et vitâ privantur. Qualium etiam vivorum, numerum adeò ingentem vidi, ut judicarem, portionem lymphæ spermaticæ arenulæ mediocri respondentem, eorum, ut minimum, decena millia continere.

Ex aliis Observatoris Literis, dat. 31, 78, etiam teutonicè conscriptis, aliquot huc spectantia excerpta.

Seminis canini tantillum microscopio applicatum iterum contemplatus sum, in eoque antea descripta animalia numerosissima conspexi. Aqua pluvialis pari quantitate adjecta, iisdem confestim mortem accersit.

Ejusdem seminis canini portiunculâ in vitreo tubulo unciæ partem duodecimalem crasso servatâ sex et triginta horarum spatio contenta animalia vitâ destituta pleraque, reliqua moribunda videbantur.

Quo de vasorum in semine genitali existentiâ magis constaret, delineationem eorum aliqualem mitto; ut in fig. ABCDE. Quibus literis circumscriptum spatium arenulam modiocrem vix superat.

De vasis, quoniam auctor dubiis, ex observationibus anatomicis oriundis, quarum antea ex parte meminit, immoratur; ideo sequentia regerenda judicavit.

——Quæ videntur vasa sive partes organicæ et tubulares, revera seminis cocti et coagulati filamenta viscosa è vasis testiculorum propriis ejaculata judicamus. Quorsum autem vasa, si fæminæ ova hæc suppeditent? Et si ova gallinacea, quidni et muliebria? At qui muliebria, ubi, inquis, inventa sunt? In ovariis. Quæ, quàm insulsè testicula nuncupantur, vel exinde patet, quòd vasa duntaxat sanguinea, nulla sibi propria obtineant. E contra, ovaria quam appositè? utpote ovorum, seu vesicularum, lymphâ viscosa, instar albuminis ovi, distentarum, duplex congeries. Adeò autem pertinaciter sibi invicem adhærent, quòd immature conspiciantur. Quin neque vel avium ova, prius quam matura, absque violenta divulsione ab ovario solvuntur. Pariter ut videmus glandes nucesve avellanas adhuc minusculas, caliculis suis firmâ continuitate infixas teneri: quæ tamen æstivo tempore, tactu excutiuntur mollissimo. Deinde, si filicula ista viscosa, quæ pro vasis ostendis, verè talia sint, ut supervacanea essent, ita etiam generationi prorsus inepta. Adeò enim in transitu è mari in fæminam implicarentur (quod etiam ostendunt a te exaratæ figuræ) ut natura longè facilius opus moliretur extruendo nova vasa quam hæc, si vasa, in ordinem regularem et generationi idoneam restituendo. Observationes demum quas Transactionibus proximè editis et edendis (N° 139 et 140) inserui, altera de fœtu non matris in utero, sed abdomine invento, altera de testiculo s. potius ovario cujusdam mulieris hydropico, rem omni dubio forsan extricabunt.

The Art of Refining. By Dr. Chr. Merrit. N° 142, p. 1046.

The design of this art is the separation of all other bodies from gold and silver, which is performed in four ways, viz. by parting, by the test, by the almond furnace or the sweep, and by mercury.

Parting is done with aquafortis, which the refiners make thus: take of saltpetre 3 lb. Dantzic vitriol 2 lb. Let them be well bruised and mixed in a mortar, and then put into a long-neck, an earthen vessel so named from its figure. Then six or eight of these long-necks so filled, are placed in each side of the furnace, on a range built with iron bars, of the form of a parabola, at above 9 inches distance from each other, and closed at the sides with bricks. The upper arches are left open to put in and take out the pots. Over the said arches are laid large bars of iron, and then all the top of the furnace covered with loam, the body of each long-neck being exposed to the fire, the neck outward, to which the receivers, whether glass or German pots, are well luted. Note that if the vitriol be not Dantzic, which is made with copper, but English, which is made with old iron, the water will be weaker, and make a dirty coloured verditer, and wholly spoil it; besides, the silver will not gather so well to the copper after solution, and thereby becomes black. The lute is made of good loam, with some horse-dung and a little colcothar, though the two former do well. The luting being well worked and applied, they make a gentle charcoal fire under the pots for 3 hours, and then increase it for 3 hours more; about the 7th hour they make a vehement hot fire for 4 hours, and cast in at last well dried billets of the length of the furnace, whose flame surrounds all the pots, and finishes the work. Next morning the receivers are carefully separated from the long-necks. Usually this work is performed but once in 24 hours, though sometimes twice.

Some refiners distil 100lb. of the materials put into a cast-iron pot, which is the best way, especially being performed after this latest invention, viz. Build a furnace 2 yards high or more, and at the top place in the iron pot; to which fit a head of earth, like the head of a large distilling alembic for chemical oils, with a large belly, about 8 inches from the iron pot, into 3 branches; dividing the middle one going straight forwards, the 2 other lateral ones obliquely; all which branches are 4 or 5 inches in diameter, and 5 or 6 long. To these branches are fitted glass bodies, narrow and hollow at both ends, large and globular in the midst; which must be well luted on with colcothar, rags, flour, and whites of eggs. To this first glass body is luted on another glass of the same figure and size, and 8 ranged in order till they come to the receiver, which is an ordinary gallon glass. All these rows of glasses lie on boards shelving from the head to the receiver. The two upper receivers or glass bodies require exceeding good luting, for the rest, ordinary lute will serve. The convenience of this way is, that a little fire, and that of Newcastle coal, will do the work, so that you save a long-neck for each 5 pounds of materials, and you need never break or unlute any of the receivers but the lowermost.

The aquafortis being distilled off, it is put into a large earthen pot, and there is added of fine silver 1 or 2 penny-weights, called fixes, to every pound of aquafortis, which in 4 hours will purge it from all dirt and impurity, and make it fit for parting, which is thus done. If the silver gilt be fine enough for wire, they only melt it in a wind-furnace, and cast it melted into a large tub of water, that they may have it in small pieces; but if it be only standard, they first fine it on the test. These small pieces taken out of the water, and well dried, are put into a tapering glass, a foot high, and 7 inches at the bottom; then the glasses are about $\frac{2}{3}$ charged with aquafortis, and set in a range of iron, covered 2 inches deep with sand, with a gentle charcoal fire under it. Small bubbles will soon rise, and the water also run over. If so, they take off the glasses till they cease boiling, or else pour some of it into a vessel. If lead be mixed with it they cannot keep it from running over. When the water has once ceased from this ebullition, it will rise no more. The greenness of the water shows the quantity of copper contained in it. If the water boil over, it will penetrate the bricks and wood. They commonly let it stand a night on the iron range, with a gentle heat under it, and in the morning gently pour off the water impregnated with all the silver, the gold lying like black dirt at the bottom, which being washed out is put into small parting glasses, and set over the sand with fair conduit-water for an hour, and then the water poured off. This is repeated 5 or 6 times, to separate the salt from the gold, which is now fit to be melted, and cast into an ingot.

To regain the silver, they have large round washing-bowls, lined within with melted rosin and pitch, for otherwise the water would heat the wood and penetrate the sides of the bowl, covered with copper plates 10 inches long, 6 wide, and half or more thick. Into these bowls they pour plenty of water, the more the better the verditer, and then the silver-water; which working on the softer metal of copper leaves all the silver in very fine sand, at the bottom and sides of the bowl, and on the plates of copper; which being taken out is washed, dried, and melted for any use. Concerning the plates it is observable, that if any brass or shruff metal be in them, they gather very little of the silver, the latter mixing with it.

Verditer is made from the copper-water poured off from the silver and whiting, in this manner: they put into a tub 100 pound weight of whiting, on which they pour the copper-water, and stir them together every day for some hours together. When the water grows pale, they take it out and set it by for further use, and pour on more of the green-water, and so continue till the verditer be made. Which being taken out, is laid on large pieces of chalk in the sun, till it be dry for the market. The water taken from the verditer is put into a copper, and boiled till it comes to the thickness of water-gruel, now principally consisting of saltpetre reduced, most of the spirit of vitriol being gone with the copper into the verditer. A dish full whereof being put into the other materials for aquafortis, is redistilled, and makes a double water, almost twice as good as that without it.

I come next to the second way of refining, viz. by the test. This separates all metals from silver, except gold, because they swim over it when they are all melted together. The test is thus made: they have an iron mould of an oval form, and 2 inches deep, with 3 arches of iron at bottom, set at equal distances, 2 fingers wide, if the great diameter of it be 14 inches long, and so proportionably in greater or less tests. This cavity they fill with fine powder of bone-ashes, moistened with lixivium made with soap-ashes. Some use cakes of pot-ashes, or other ashes well cleansed, and so pressed well together with a muller, that it becomes very close and smooth at the top. A cavity is left above in the midst of it, to contain the melted silver; this cavity being longest in the middle: for the bone-ashes come up parallel to the circumference of the mould, only a small channel left in that end which is most remote from the blast, for the running off of the baser metals, and so is made to decline to the centre of the test, where it is not above half an inch deep. The test thus made, is set annealing 24 hours, and then it is set in a chimney a yard high, parallel almost to the nose of a pair of large bellows; then the silver is put in, which being covered all over with billets of barked oak, the blast begins, and continues strongly all the while. The lead purified from all silver, which they call the soap of metals, is first put in, and melts down with the silver; then the lead and copper swim at the top, and run over the test; the motion the refiner helps with a long rod of iron, drawn along the surface of the silver towards the forementioned slit, and often stirring all the metal, that the impurer may rise the better: and by thus continuing, the separation is made in 2 or 3 hours. The greatest part of the lead flies away in fumes.

If the lead be gone before all the copper, the metal will rise in small red fiery bubbles; and then they say it drives, and more lead must be added. The force of the blast drives the upper metal to the lower side of the test, and promotes its running over. When the silver is fully refined, it looks like very pure quicksilver; then they take off their fogs and let it cool. In the cooling, the silver will frequently from the middle spring up in small rays, and fall down again. If moist silver be put into that which is melted, it will spring into the fire. A good test will serve two or three firings. As soon as the silver will hold together, they take it out of the test, and beat it on an anvil into a round figure, for the melting pot; which being set in a wind furnace, surrounded with coal, and covered with an iron cap, that no charcoal fall into it, it is then melted. If any dross or filth be in the melting-pot, they throw in some tincal, which collects the dross together, that it may be separated from it. These melting-pots are never burned, but only dried, and will last a whole day, if they be not suffered to cool: but if they once cool, they infallibly crack.

The next is the almond furnace or sweep. Here are separated all sorts of metals from cinders, parts of melting-pots, tests, brick, and all other harder bodies, which must be first beaten into small pieces with a hammer, and an iron plate. Those which stick but superficially to the silver, they wash off thus: they have a wooden round instrument, 2 feet wide, somewhat hollow in the middle, with a handle on each side. On this they put the materials, and hold them in a tub of water below the surface, and so waving it to and fro, all the lighter and looser matter is separated from the metal.

The furnace is 6 feet high, 4 feet wide, and 2 feet thick. It is made of brick, having a hole in the middle of the top, 8 inches over, growing narrower towards the bottom, where on the fore part it ends in a small hole, encompassed with a semicircle of iron, to keep the melted metal. About the middle of the back is another hole, to receive the nose of a pair of great bellows. The night before they begin, charcoal is kindled in the furnace to anneal it: and when hot, they throw 2 or 3 shovels of coal to one of the fore-mentioned stuff, and so proceed during the whole work, which continues 3 days and nights without intermission. After 8 or 10 hours the metal begins to run, and when the receiver below is pretty full, they lade it out with an iron ladle, and cast it into sows in cavities or forms made with ashes. They frequently stop the passage-hole with cinders to keep in the heat; and when they think a quantity of metal is melted, they unstop the hole to pass it off. If the matter be hard to flux, they throw in some slag, which is the recrement of iron, to give it fusion. A stinking blue smoke proceeds from the furnace, and all by-standers look like dead men. To get the silver from these metals, they now use no other art than that of the test; and the same to refine the copper from the litharge.

The last way of separation is by quicksilver. And this is for filings of small workers and goldsmiths, wherein gold and silver are mixed with dust, &c. This dust is put into a hand-mill with quicksilver, and being continually turned upon that and the metals, an amalgama is made of them; then fair water poured in carries off the dust, as it runs out again by a small quill. This amalgama is put into an iron vessel with a bolt-head, set in the fire, having an iron-neck 3 feet long, to which a receiver is fitted. The mercury distils off into the receiver, and the gold and silver remain in the bolt-head.

On the English Alum Works. By Daniel Colwall, Esq. N° 142, p. 1052.

Alum is made of a stone dug out of a mine, of a sea-weed and urine.*—The stone is found in most of the hills between Scarborough and the river Tees in the county of York. As also near Preston in Lancashire. It is of a bluish colour, and will cleave like Cornish-slate. That mine which lies deep in the earth, and is indifferently well moistened with springs is the best. The dry mine is not good: and too much moisture cankers and corrupts the stone; making it nitrous. In this mine are found several veins of stone called doggers; of the same colour, but not so good. Here are also found those which are commonly called snake-stones. For the more convenient working of the mine, which sometimes lies 20 yards under a surface, they begin the work on the declivity of a hill, where they may also be well furnished with water. They dig down the mine by stages, to save carriage; and so throw the mineral down near the places where they calcine it; being exposed to the air before it is calcined, it will moulder in pieces, and yield a liquor of which copperas may be made; but being calcined, is fit for alum. As long as it continues in the earth or in water, it remains a hard stone. Sometimes a liquor will issue out of the side of the mine, which by the heat of the sun becomes natural alum. The mineral is calcined with cinders of Newcastle coal, wood, and furzes. The fire is made about 2½ feet thick, 2 yards broad, and 10 yards long. Between the fires are stops made with wet rubbish; so that any one or more of them may be kindled without prejudice to the rest.

After that 8 or 10 yards thickness of broken mineral are laid on this fuel, and five or six of them are so covered; they then begin to kindle the fuel; and as the fires rise towards the top, they lay on fresh mineral. So that, to what height soever the heap be raised, which is oftentimes about 20 yards, the fires without any more fuel will burn to the top, and stronger than at the first kindling, as long as any sulphur remains in the stones.

* Alum is a triple salt composed of vitriolic acid, alumina and potash or ammonia. The potash obtained by the incineration of the sea weed here mentioned (or from the ashes of other vegetables) serves (like the ammonia contained in urine) to combine with the superabundant acid, a circumstance essential to the crystallization of the aluminous salt, and to its purification from iron or any other metallic or earthy ingredient. The stone or ore of alum above described is termed by mineralogists schistus aluminaris, and contains besides the earth of alum, sulphur and iron, a portion of silica and magnesia, and generally some bituminous matter. By the process of calcination, the sulphur is converted into sulphuric or vitriolic acid, and combines with the aluminous earth. By the processes of lixiviation and boiling this union is further promoted, and the insoluble parts (consisting chiefly of silica and oxyd of iron) are separated; and on the addition of the ashes of the sea weed (kelp) and urine, the crystallization (after sufficient evaporation of the liquor) is effected—The terms "nitre and nitrous" which frequently occur in this paper are improper.

In calcining these stones, the wind often does hurt, by forcing the fire in some places too quickly through the mineral, leaving it black and half burned; and in others burning it too much, leaving it red. But where the fire passes slowly, and of its own accord, it leaves the mineral white, which yields the best and greatest quantity of liquor.

The mineral thus calcined, is put into pits of water, supported with frames of wood, and rammed on all sides with clay, about 10 yards long, 5 yards broad, and 5 feet deep; set with a current, that turns the liquor into a receptacle, from whence it is pumped into another pit of mineral. And thus every pit of liquor before it comes to boiling, is pumped into four several pits of mineral; and every pit of mineral is steeped in four several liquors, before it be thrown away; the last pit being always fresh mineral. The mineral thus steeped in each of the several liquors about 24 hours, is of course four days in passing the four several pits, from whence the liquors pass to the boiling-house.

The water, or virgin-liquor, often gains in the first pit 2 pounds weight; in the second it increases to 5 pounds weight; in the third to 8 pounds weight; and in the last pit, which is always fresh mineral, to 12 pounds weight; and so in this proportion, according to the goodness of the mineral, and the proper calcining: for sometimes the liquors passing the four several pits, will not be above 6 or 7 pounds weight. Yet often the liquor of 7 or 8 pounds weight produces more alum than that of 10 or 12 pounds, either from the badness of the mineral, or, as usually, the bad calcining of it. And if, by passing the weak liquor through another pit of fresh mineral, you bring it to 10 or 12 pounds weight, yet there will be less alum with it, than when it was but 8 pounds weight. For what it gains from the last pit of mineral, will be mostly nitre and slam, which spoil the good liquors, and disorder the whole house, until the slam be wrought out. What they call slam is first perceived by the redness of the liquor when it comes from the pit, occasioned either by the badness of the mineral, or more commonly the over or under calcining it; which in the settler sinks to the bottom, and there becomes of a muddy consistence and a dark colour. That liquor which comes whitest from the pits is the best. When a work is first begun, they make alum of the liquor only that comes from the pits of mineral, without any other ingredients. And so it might continue, but that it would spend so much liquor as not to quit cost.

Kelp is made of a sea-weed, called tangle. It grows on rocks by the sea side, between high and low water mark. Being dried it will burn and run like pitch; when cold and hard it is beaten to ashes, steeped in water, and the lees drawn off to about 2 pounds weight.

Because the country people who furnish the work with urine, sometimes mix it with sea-water, which cannot be discovered by weight: they try it by putting it to some of the boiling liquor: for if the urine be good, it will work like yest put to beer or ale; but if mixed, it will stir no more than so much water. It is observed, that the best urine is that which comes from poor labouring people, who drink but little strong liquor.*

The boiling pans are made of lead, 9 feet long, 5 feet broad, and 2½ deep, set upon iron plates, about 2 inches thick; which pans are commonly new cast, and the plates repaired 5 times in 2 years.—When the work is begun, and alum once made, then they save the liquor which comes from the alum, or wherein the alum shoots, which they call mothers. With this they fill two thirds of the boilers, and put in one third part of fresh liquor, which comes from the pits. Being thus filled up with cold liquor, the fires having never been drawn out, they will boil again in less than 2 hours time; and in every two hours the liquor will waste 4 inches, and then the boilers are filled up again with green liquor.—The liquor, if good, will in boiling seem greasy at the top: if nitrous it will be thick, muddy, and red.† In boiling 24 hours, it will be 36 pounds weight; then about a hogshead of the lees of kelp is put into the boiler of about two penny weight, which will reduce the whole boiler to about 27 pounds weight.—If the liquor be good, as soon as the lees of kelp are put into the boiler, they work like yest put to beer: but if the liquor in the boiler be nitrous, the kelp-lees will stir it but very little; and in that case they put in the more and stronger lees.—Presently after the kelp lees are put into the boiler, all the liquor is drawn into a settler as large as the boiler, made of lead, in which it stands about two hours; in which time most of the nitre and slam sink to the bottom.—This separation is made by means of the kelp-lees. For when the whole boiler consists of green liquor drawn from the pits, it is of power strong enough to cast off the slam and nitre: but when mothers are used, the kelp-lees are needful to make the said separation.—Then the liquor is scooped out of the settler into a cooler, made of deal-boards, and rammed with clay. Into this is put 20 gallons or more of urine, more or less, according to the goodness of the liquor; for if the liquor be red,† and consequently nitrous, the more urine is required. In temperate weather the liquor stands 4 days in the cooler. The second day the alum begins to strike, gather and harden about the sides, and at the bottom of the cooler. If the liquor should stand in the cooler above 4 days, they say it would turn to copperas—The use of urine is both to cast off the slam, and to keep the kelp-lees from hardening the alum too much. In hot weather the liquors will be a day longer in cooling, and the alum in gathering,

* This alum consists of 4 ingredients.

† This redness is owing not to nitre but iron.

than when the weather is temperate. In frosty weather the cold strikes the alum too soon, not giving time for the nitre and slam to sink to the bottom, and thus they are mixed with the alum. This produces double the quantity; but being foul it is consumed in the washing.

When the liquor has stood 4 days in the cooler: then that called mothers is scooped into a cistern, the alum remaining on the sides and at the bottom; and from thence the mothers are pumped back into the boiler again. So that every 5 days the liquor is boiled again till it evaporate, or turn into alum and slam.—The alum taken from the sides and bottom of the cooler is put into a cistern, and washed with water that has been used for the same purpose, being about 12 pounds weight. After which it is roached in the following manner:—Being washed, it is put into another pan with a quantity of water, where it melts and boils a little; it is then scooped into a large cask, where it commonly stands 10 days, and is then fit to take down for the market. The liquors are weighed by troy-weight. So that half a pint of liquor must weigh more than so much water, by so many penny weights.

The English Green Copperas. By Dan. Colwall, Esq. N° 142, p. 1056.

Copperas-stones,* which some call gold-stones, are found on the sea-shore in Essex, Hampshire, and so westward. There are great quantities on the cliffs; but not so good as those on the shore, where the tides ebb and flow over them. The best of them are of a bright shining silver colour: the next such as are of a rusty deep yellow: the worst, such as have gravel and dirt in them, of a dull or umber colour. In the midst of these stones, are sometimes found the shells of cockles, and other small shell fishes; also small pieces of the planks of ships and of seacoal. The brightest of these stones are used for wheel-lock pistols and fusees.—To make the copperas, they prepare beds according as the ground will permit. Those at Deptford are about 100 feet long, 15 feet broad at the top, and 12 feet deep, shelving all the way to the bottom. They ram the bed very well first with strong clay, and then with the rubbish of chalk; whereby the liquor which drains out of the solution of the stones, is conveyed into a wooden shallow trough, laid in the middle of the bed, and covered with a board; being also boarded on all sides, and laid lower at one end than the other, the liquor is conveyed into a cistern under the boiling-house.—When the beds are indifferently well dried, they lay on the stones about 2 feet thick.—These stones will be 5 or 6 years before they yield any considerable quantity of liquor; and before that, the liquor they yield is but weak. They ripen by the sun and rain: yet

* Martial pyrites; from which by the process here described is extracted sulphate of iron, or green vitriol.

experience proves, that watering the stones, although with water prepared by lying in the sun, and poured through very small holes of a watering-pot, only retards the work. In time these stones turn into a kind of vitriolic earth, which swells and ferments like leavened dough.—When the bed is come to perfection, then once in 4 years they refresh it, by laying new stones on the top.—When they make a new bed, they take a good quantity of the old fermented earth, and mingle with new stones, whereby the work is hastened. Thus the old earth never becomes useless.

The cistern before mentioned is made of strong oaken boards, well joined and chalked. That at Deptford will contain 100 tuns of liquor. Great care is to be taken, that the liquor does not drain through the beds or out of the cistern. The best way to prevent this, is to divide the cistern in the middle by oaken boards, chalked as before, whereby any one of them may be mended in case of a defect. The more rain falls, then the more, but the weaker, will be the liquor. The goodness of it is tried by weights prepared for that purpose: 14 penny weight is rich: or an egg being put into the liquor, the higher it swims above the liquor, the stronger it is: sometimes the egg will swim near half above the liquor. Within one minute after an egg is put in, the ambient liquor will boil and froth; and in 3 minutes the shell will be quite worn off. A drop of this liquor falling on the manufactures of hemp, flax, or cotton-wool, will presently burn a hole through it; as also in woollen and leather.

The liquor is pumped out of the aforesaid cistern into a boiler of lead, about 8 feet square, containing about 12 tuns, which is thus ordered: first they lay long pieces of cast iron, 12 inches square, as long as the breadth of the boiler, about 12 inches from each other, and 24 inches above the surface of the fire. Then crosswise they lay ordinary flat iron bars, as close as they can lie, the sides being made up with brick-work. In the middle of the bottom of this boiler is laid a trough of lead, wherein they put at first 100 pounds weight of old iron.

The fuel for boiling is Newcastle coal. By degrees in the boiling, they put in more iron, amounting in all to 1500 pounds weight in a boiling; and as the liquor wastes in boiling, they pump in fresh liquor into the boiler. Hence, and by a defect in ordering the fire, they were formerly above 20 days before it was finished. When done, they try by taking up a small quantity of liquor into a shallow earthen pan, and observing how soon it will gather and crust about the sides of it. But of late, by the ingenious contrivance of Sir Nicolas Crisp, the work is much facilitated. For at his work at Deptford, they boil off 3 boilers of ordinary liquor in one week. Which is done, first by ordering the furnace so as that the heat is conveyed to all parts of the bottom and sides of the furnace. Then, instead of pumping cold liquor into the boiler to supply the waste in boiling, which checked the work sometimes 10 hours, Sir Nicolas has a vessel of lead, which he calls a heater, placed at the end of the boiler, and a little higher, supported by bars of iron as before, and filled with liquor, which by a conveyance of heat from the furnace is kept near boiling hot; and hence he continually supplies the waste of the boiler, without hindering the boiling. Thirdly, by putting in due proportions of iron from time to time into the boiler. As soon as they perceive the liquor to boil slowly, they put in more iron, which will soon quicken it. If they do not continually supply the boiling liquor with iron, the copperas will gather to the bottom of the boiler and melt. And so it will do if the liquor be not presently drawn off from the boiler into a cooler, so soon as it is sufficiently done.

The cooler, made of tarras, is oblong, 20 feet long, 9 feet over at the top, and 5 feet deep, tapered towards the bottom. Into this they let the liquor run, as soon as it is boiled enough. The copperas herein will be gathering or shooting 14 or 15 days; and it gathers as much on the sides as in the bottom, viz. above 5 inches thick. Some put bushes into the cooler, about which the copperas will gather. But at Deptford they make no use of them. That which sticks to the sides and to the bushes is of a bright green; that in the bottom of a foul and dirty colour.—At the end of 14 days, they convey the liquor into another cooler, and reserve it to be boiled again with new liquor.—The copperas they shovel on a floor adjoining, so that the liquor may drain from it into a cooler. The steam which comes from the boiling is of an acrimonious smell.

Copperas may be boiled without iron, but with difficulty.* Without it the boiler will be in danger of melting. Sometimes in stirring the earth on the beds, they find pieces of copperas produced by lying in the sun.

Account of the Salt Springs and Salt making at Droitwich in Worcestershire. By Dr. Thomas Rastell. N° 142, p. 1059.

The country has no great hills, but many small risings. On the other side the river Severn are Aberly Hills, at about 7 miles distance from us. There are many salt springs about the town, which is seated by a brook-side called Salwark-Brook, which arise both in the brook and in the ground near it, though there are but 3 pits that are made use of. The plants growing about the springs I find much the same as in other places; but where the springs are saltest there grows nothing at all; only by the brackish ditches there grows aster atticus, with a pale flower, which I find no where else with us.

* The unsaturated vitriolic acid acts upon the iron thus added, and consequently the quantity of vitriolic salt (green copperas) is increased.

The depth of the springs is various; some rise on the top of the ground which are not so salt as others: those that are in the pits we make use of, are various also. The great pit, which is called Upwich Pit, is 30 feet deep, in which are 3 distinct springs rising in the bottom. The pit is about 10 feet square; the sides are made with square elms, jointed in at the full length, which I suppose is occasioned by the saltness of the ground, which appears to me to have been a bog, the surface of it is made of ashes. That it was originally a bog I am induced to believe; for, not many years since digging to try the foundation of a seal, for so we call the houses we make salt in, I thrust a long staff over head.

There are no hot springs near us: and the brine is generally colder than other water; yet it never freezes, but the rain water that lies on the brine in extreme hard frosts will freeze, though not much.

I never observed nor heard of any shells in the earth. For the nature of the soil about the town on the lower side, it is a black rich earth, under which 2 or 3 feet is a stiff gravelly clay; then marl. Those that make wells for fresh water, if they find springs in the marl, they are generally fresh; but if they sink through the marl, they come to a whitish clay mixed with gravel, in which the springs are more or less brackish.

In the great pit at Upwich, we have at once 3 sorts of brine, which we call by the names of first-man, middle-man, and last-man, these sorts being of different strengths. The brine is drawn by a pump; that which is in the bottom is first pumped out, which is that we call first-man, &c. A quart measure of this brine weighs 29 ounces troy, but of distilled water only 24 ounces. This brine yields above a fourth part salt; so that 4 tuns of brine make above 1 tun of salt. The two other sorts less, or 28 ounces. And the pit yields 450 bushels of salt per day. In the best pit at Netherwich a quart of brine weighs 28 ounces and a half; this pit is 18 feet deep, and 4 feet broad, and yields as much brine every 24 hours, as makes about 40 bushels of salt. The worst pit at Netherwich is of the same breadth and depth as the former; a quart of brine out of which weighs 27 ounces, and yields as much brine daily as makes about 30 bushels of salt.

The fuel which was heretofore used was all wood; which, since the iron works, is destroyed, that all the wood at any reasonable distance will not supply the works one quarter of the year; so that now we use almost all pit-coal, which is brought to us by land, from 13 or 14 miles distance.

The vats we boil the brine in are made of lead, cast into a flat plate, 5 feet and a half long, and 3 feet over; then the sides and ends beaten up, and a little raised in the middle, which are set upon brick-work, called ovens, in which is a grate to make the fire on, and an ash-hole which we call a trunk. In some seals are 6 of these pans, in some 5, some 4, some 3, some 2. In each of these pans is boiled at a time as much brine as makes 3 pecks of white salt. For clarifying the salt we should have little need, were it not for dust accidentally falling into the brine. The brine of itself being so clear that nothing can be clearer. For clarifying it, we use nothing but the whites of eggs; of which we take a quarter of a white, and put it into a gallon or two of brine, which being beaten with the hand, lathers as if it were soap, a small quantity of which froth put into each vat, raises all the scum, the white of one egg clarifying 20 bushels of salt; by which means our salt is as white as any thing can be; neither has it any ill savour, as that salt has that is clarified with blood. For granulating it we use nothing at all; for the brine is so strong of itself, that unless it be often stirred, it will make salt as large grained as bay-salt. I have boiled brine to a candy height, and it has produced clods of salt as clear as the clearest alum, like isle of May salt; so that we are necessitated to put a small quantity of rosin into the brine, to make the grain of the salt small.

Besides the white salt above spoken of, we have another sort, called clod-salt, which adheres to the bottoms of the vats, and which after the white salt is laded out, is digged up with a steel picker. This is the strongest salt I have seen, and is most used for salting bacon and neats tongues; it makes the bacon redder than other salt, and makes the fat eat firm: if the swine are fed with mast, it hardens the fat almost as much as if fed with pease, and salted with white salt. It is very much used by country-women, to put into their runnet-pots, esteeming it better for their cheese. These clods are used to broil meat with, being laid on coals; but we account it too strong to salt beef with, as it takes away too much of its sweetness. There is a third sort of salt, called knockings, which candies on the stales of the barrow, as the brine runs from the salt after it is laded out of the vats: this salt is most used for the same purposes as the clod salt, though it is not altogether so strong. There is also a fourth sort, called scrapings, being a coarse sort of salt that is mixed with dross and dust, that cleaves to the tops of the sides of the vats; this salt is scraped off the vats when we reach them, that is, when we take the vats off the fires to beat up the bottom; and is bought by the poor sort of people to salt meat with. A fifth sort is pigeon salt; which is nothing but the brine running out through the crack of a vat, and hardens to a clod on the outside over the fire. Lastly, the salt loaves are the finest of the white salt, the grain of which is made something finer than ordinary, that it may the better adhere together, which is done by adding a little more rosin, and is beaten into the barrows when it is laded out of the vat.

Our salt is not so apt to dissolve as Cheshire salt, nor as that salt that is made by dissolving bay-salt and clarifying it, which is called salt upon salt; which appears by our long keeping it without any fire.

If it is asked why we use not iron-pans as in Cheshire and other places? I answer, there have been trials made both of forged iron-pans and cast-iron. The former the strength of the brine so corrodes, that it quickly wears them out; the latter the brine breaks.

On the Culture, and Use of Maize. By Mr. Winthorp.* N° 142, p. 1065.

The corn used in New England, before the English planted there, is called by the natives, Weachin, known by the name of Maize, in some southern parts of America; where, and even in the northern parts, amongst the English and Dutch, who have plenty of wheat and grain, this sort of corn is still much in use both for bread and other kind of food. The ear is for the most part about a span long, composed of several rows of grains, 8 or more, according to the goodness of the ground; and in each row, usually above 30 grains, of various colours, as red, white, yellow, blue, olive, greenish, black, speckled, striped, &c. sometimes in the same field, and the same ear. But the white and yellow are the most common. The ear is clothed and armed with several strong thick husks. Not only defending it from the cold of the night, and from unseasonable rains, but also from the birds. The stalk grows to the height of 6 or 8 feet; more or less, according to the condition of the ground, or kind of seed. The Virginian grows taller than that of New England. And there is another sort used by the Northern Indians far up in the country, that grows much shorter than that of New England. It is jointed like a cane, and is full of sweet juice, like the sugar-cane. And a syrup as sweet as sugar may be made of it. Also meats sweetened with it, have not been distinguished from the like sweetened with sugar. At every joint there are long leaves, almost like flags, and at the top a bunch of flowers, like the blossoms of rye. It is planted between the middle of March and the beginning of June. But most commonly from the middle of April to the middle of May. In the pure northerly parts they have a peculiar kind called Mohawks corn, which though planted in June, will be ripe in season. The stalks of this kind are shorter, and the ears grow nearer the bottom of the stalk, and are generally of divers colours.

The manner of planting is in rows, at equal distance every way, about 5 or 6 feet. They open the earth with a hoe, taking away the surface 3 or 4 inches deep, and the breadth of the hoe; and so throw in 4 or 5 grains, a little distant one from another, and cover them with earth. If two or three grow, it is well; for some of them are usually destroyed by birds, or mouse-squirrels. The corn grown up a hand's length, they cut up the weeds, and loosen the earth about it, with a broad hoe: repeating this labour, as the weeds grow. When the stalk begins to grow high, they draw a little earth about it: and upon the putting forth of

* Zea Mays. Linn.

the ear, so much as to make a little hill, like a hop-hill; after this, they have no other business about it, till harvest. After it is gathered, it must, except laid very thin, be presently stripped from the husks; otherwise it will heat, grow mouldy, and sometimes sprout. The common way is to weave the ears together in long traces by some parts of the husk left thereon. These traces they hang upon stages or other bearers within doors, or without; for, hung in that manner, they will keep good and sweet all the winter after, though exposed to all weathers. The natives commonly thresh it as they gather it, and dry it well on mats in the sun; then bestow it in holes in the ground, well lined with withered grass and mats, and then covered with the like, and over all with earth: and thus it is kept very well till they use it. The English have now taken to a better way of planting by the help of the plough, in this manner: in the planting time they plough single furrows through the whole field, about 6 feet distant, more or less, as they see convenient. To these they plough others across at the same distance. Where these meet they throw in the corn, and cover it either with the hoe, or by running another furrow with the plough. When the weeds begin to overtop the corn, then they plough over the rest of the field between the planted furrows, and so turn in the weeds. This is repeated once, when they begin to hill the corn with the hoe; and thus the ground is better loosened than with the hoe, and the roots of the corn have more liberty to spread. Where any weeds escape the plough, they use the hoe. Where the ground is bad or worn out, the Indians used to put two or three of the fishes called aloofes under or adjacent to each corn-hill; whereby they had many times a crop double to what the ground would otherwise have produced. The English have learned the like husbandry, where these aloofes come up in great plenty, or where they are near the fishing-stages; having there the heads and garbage of cod-fish, at no charge but the fetching.

The fields thus ploughed for this corn, after the crop is off, are almost as well fitted for English corn, especially summer grain, as pease, or summer wheat, as if lying fallow, they had had a very good summer tilth. The Indians and some English, at every corn-hill, plant with the corn, a kind of French or Turkey beans: the stalks of the corn serving instead of poles for the beans to climb up with. And in the vacant places between the hills they plant squashes and pompions; loading the ground with as much as it will bear. And many, after the last weeding, sprinkle turnep-seed between the hills, and so after harvest have a good crop of turneps.

The stalks of this corn, cut up before too much dried, and so laid up, are good winter-fodder for cattle. But they usually leave them on the ground for the cattle to feed on. The husks about the ear are good fodder, given for change sometimes after hay. The Indian women slit them into narrow parts, and so weave them artificially into baskets of several fashions. This corn the Indians dressed several ways for their food. Sometimes boiling it whole till it swelled and became tender, and so either eating it alone, or with their fish or venison instead of bread. Sometimes bruising in mortars, and so boiling it. But commonly this way, viz. by parching it in ashes, or embers, so artificially stirring it, as without burning, to be very tender, and turned almost inside outward, and also white and floury. This they sift very well from the ashes, and beat it in their wooden mortars, with a long stone for a pestle, into fine meal. This is a constant food at home, and especially when they travel, being put up in a bag, and so at all times ready for eating, either dry or mixed with water. They find it very wholesome diet, and is that their soldiers carry with them in time of war. The Indians have another sort of provision out of this corn, which they call sweet-corn. When the corn in the ear is full, while it is yet green, it has a very sweet taste. This they gather, boil, and then dry, and so put it up into bags or baskets, for their use: boiling it again, either whole or grossly beaten, when they eat it, either by itself, or among their fish or venison, or beavers, or other flesh, accounting it a principal dish. These green and sweet ears they sometimes roast before the fire or in the embers, and so eat the corn; by which means, they have sufficient supply of food, though their old store be done. The English, of the full ripe corn ground make very good bread. But it is not ordered as other corn; for if it be mixed into stiff paste, it will not be so good, as if made only a little stiffer than for puddings; and so baked in a very hot oven, standing therein all day or all night. Because on the first pouring of it on the oven floor, it spreads abroad; they pour a second layer or heap upon every first, and thereby make so many loaves. It is also sometimes mixed with half or a third part of rye or wheat meal, and so with leaven or yest made into loaves of very good bread.

Before they had mills, having first watered and husked the corn, and then beaten it in wooden mortars, the coarser part sifted from the meal, and separated from the loose hulls by the wind, they boiled to a thick batter: to which being cold, they added so much of the fine meal, as would serve to stiffen it into paste, whereof they made very good bread. But the best sort of food which the English make of this corn, is that they call samp. Having first watered it about half an hour, and then beaten it in a mortar, or else ground it in a hand or other mill, into the size of rice, they next sift the flour, and winnow the hulls from it. Then they boil it gently till it be tender, and so with milk or butter and sugar, make it into a very pleasant and wholesome dish. This was the most usual diet of the first planters in these parts, and is still in use amongt them, as well in fevers, as in health: and was often prescribed by the learned Dr. Wilson to his patients in London. And of the Indians that live much upon this corn, the English have been informed by them, that the disease of the stone is very seldom known among them. The English have also found out a way to make very good beer of grain: that is, either of bread made hereof, or else by malting it. The way of making beer of bread, is by breaking or cutting it into great lumps, about as large as a man's fist, to be mashed, and so proceeded with as malt, and the impregnated liquor, as wort, either adding or omitting hops, as is desired.

To make good malt of this corn, a particular way must be taken. The barley malt-masters have used all their skill to make good malt of it the ordinary way, but cannot effect it; that is, that the whole grain be malted, and tender and floury, as in other malt. For it is found by experience, that this corn, before it be fully malted, must sprout out both ways, i. e. both root and blade, to a great length, of a finger at least: if more, the better; for which it must be laid on a heap a convenient time.

To avoid all difficulties, this way was tried and found effectual. Take away the top of the earth in a garden or field 2 or 3 inches, throwing it up half one way and half the other. Then lay the corn for malt all over the ground, so as to cover it. Then cover the corn with the earth that was pared off, and there is no more to do, till you see all the plot of ground like a green field covered over with the sprouts of the corn, which will be within 10 or 14 days, according to the time of the year. Then take it up, and shake the earth from it and dry it. This way every grain that is good will grow, and be mellow, floury, and very sweet; and the beer made of it be wholesome, pleasant, and of a good brown colour. Yet beer made of the bread, as aforesaid, being as well coloured, wholesome, pleasant, and more durable, is most in use; because the way of malting this corn, last described, is as yet but little known among them.

Manner of making Malt in Scotland. By Sir Robert Moray. N° 142, p. 1069.

Malt is there made of no other grain but barley; of which there are two kinds: one, which has four rows of grains on the ear, the other, two rows. The first is the more commonly used, but the other makes the best malt. The more recently barley has been threshed it makes the better malt; but if it has been threshed six weeks or upwards, it proves not good malt, unless it be kept in an equal temper, of which it easily fails, especially if it be kept up against a wall; for that which lies in the middle of the heap is freshest, that which lies on the outsides and at top is over-dried, that which is next the wall shoots forth, and that which is at the bottom rots. The best way to preserve threshed barley long in good temper, is not to separate the chaff from it. But as long as it is unthreshed it is always good. Brewers use to keep their barley in large rooms on boarded floors, laid about a foot in depth, and so turned over now and then with scoops. Barley that has been over heated in the stacks or barns, before it be separated from the straw, will never prove good for malt, nor any other use.

But though it heat a little after it is threshed, and be kept in the chaff, it will not be worse, but rather better for it; as it will come sooner, and more equally.

Take then good barley, newly threshed, and well purged from the chaff, and put hereof 8 bolls, that is, about 6 English quarters, in a stone trough. Where let it infuse, till the water be of a bright reddish colour, which will be in about 3 days, more or less, according to the moistness or dryness, the size of the grain, the season of the year, or temper of the weather. In summer malt never makes well. In winter it will need longer infusion than in the spring or autumn. It may be known when steeped enough, by other marks besides the colour of the water; as the excessive swelling of the grain, or, if over steeped, by two much softness; being, when in the right temper, like that barley which is prepared to make broth of, or the barley called by some, urge wonder. When the barley is sufficiently steeped, take it out of the trough, and lay it on heaps, to let the water drain from it. Then after 2 or 3 hours turn it over with a scoop, and lay it in a new heap about 20 or 24 inches deep. This heap they call the coming heap. And in the managing of this heap aright lies the great skill. In this heap it will lie 40 hours, more or less, according to the forementioned qualities of the grain, &c. before it come to the right temper of malt. Whilst it lies in this heap it is to be carefully looked to during the first 15 or 16 hours. For about that time, the grains will begin to put forth the root, which when they have equally and fully done, the malt must, within an hour after, be turned over with a scoop, otherwise the grains will begin to put forth the blade or spire also, which by all means must be prevented; for hereby the malt will be utterly spoiled, both as to pleasantness of taste and strength.

If all the malt come not equally, for that which lies in the middle, being warmest, will usually come first; turn it over, so as the outmost may lie inmost, and so leave it till all come alike. As soon as the malt is sufficiently come, turn it over, and spread it to a depth not exceeding 5 or 6 inches. And by the time it is all spread out, begin and turn it over and over again, 3 or 4 times. Afterwards turn it over in like manner, once in 4 or 5 hours, making the heap thicker by degrees, and continuing so to do constantly for the space of 48 hours at least. This frequent turning of it over cools, dries, and deads the grain; whereby it becomes mellow, melts easily in brewing, and then separates entirely from the husk. Then throw up the malt into a heap as high as you can. Where let it lie, till it grows as hot as your hand can endure it: which usually is in about 30 hours space. This perfects the sweetness and mellowness of the malt. After the malt is sufficiently heated, throw it abroad to cool, and turn it over again about 6 or 8 hours after, and then dry it on the kiln. Where, after one fire, which must serve for 24 hours, give it another more slow, and if need be, a third. For if the malt be not thoroughly dried it cannot be well ground, neither will it dissolve well in the brewing, and the ale it makes will be red, bitter, and will not keep.

The best fuel is peat. The next, charcoal, made of pit-coal or cinders. Heath, broom, or furzes, are bad. If there be not enough of one kind, burn the best first, for that gives the strongest impression as to the taste.

Αντιδιατριβη, *Sive Animadversiones in Malachiæ Thrustoni, M.D. Diatribam de Respirationis Usu primario. Auctore Georgio Entio,* Eq. Aur. M.D. et Col. Lond. Soc.* 1679. N° 142, p. 1072.

In this book, besides the anatomical observations, several opinions are proposed and defended with the known elegancy and learning of the author.

It seems probable, says our author, that the finer part of the alimentary juice is transmitted from the stomach and guts, by the mediation of small concave fibres thereto annexed, to the several parts for their nourishment. That the same alimentary juice is that which, in the use of vomitories and cathartics, is by the same concave fibres disgorged into the stomach and guts, and not by lacteal veins, or the arteries. That the water or serum, which is extravasated in hydropic persons, issues not from the sanguiferous vessels; but that it is the nutritious juice itself, which either by an ulcer in some mesenteric gland, which is not unusual, or an aperture in some lymphæduct, oozes into the cavity of the abdomen. That the febrific matter in intermittents is not lodged originally in the blood. That the pituita, supposed by Dr. Glisson and others to be spued out of the arteries into the coats of the stomach, is this very nutritious juice, tending to other parts of the body, but upon the death of the animal, by cold and slower motion condensed, and there arrested in its way. That after the same manner milk is also transferred to the breasts. That neither in abscesses, nor in any other case, it is the extravasated blood that suppurates, but only the nutritious or alimentary juice. That accordingly in the small pox, the purulent matter is not derived or bred out of the blood, but out of the aforesaid juice. So that if a woman with child has the small pox, the child is found to have them too, though not one drop of the mother's blood passes into the child.

* This was one of the most accomplished physicians of the 17th century. He was born at Sandwich in Kent, in 1604; and after prosecuting his studies at Cambridge, he went to Padua, where he took his medical degree. On his return from his travels he settled in London; became a member of the Royal Society, and was elected president of the College of Physicians. The honour of knighthood was conferred upon him by Charles II. He died at an advanced age in 1689. Sir George Ent was one of the most strenuous defenders of the Harveian circulation of the blood, as may be seen by consulting his Apologia pro Circulatione Sanguinis, quâ respondetur Æmilio Parisano 1641, and afterwards republished, 1685, with considerable additions. The abovementioned tract, entitled Antidiatriba, and his Apologia were reprinted at Leyden in 1687, under the title of Entii Opera Physico-medica.

That the membranes and nerves suck in their nourishment from the glands of the mouth and throat, while we chew our meat. That after it is concocted in the stomach, part of it is filtrated and transferred by the œsophagus or gullet to the brain. From whence it is also derived to the nerves and membranes, especially the membrana carnosa originated of the pia mater. That the colliquamentum, which first appears in a setting egg, is the ground work or beginning of the brain. That the blood serves not to nourish the body, but only to foment it, as it were, or keep it warm. That generation is opus ideale, and the semen not to be taken for an extract from the several parts. For viviparous animals have a placenta, to which there is nothing analogous in either parent. That the pulse is rather the vibration, by a continuation of the motion from the heart, than the intumescence of the artery. That urine is not derived to the reins by the emulgent arteries, but by the nerves. That what are called the lungs of a frog are only wind-bladders, analogous not to lungs, which in a frog are no where found, but to that part, which in fishes is commonly called the swim. That the primary use of respiration, is not to carry off fuliginous steams from the blood, but for the ventilation of the vital flame in the heart or blood, and supplying it with proper fuel. That it is a vulgar error, that the action of exspiration is performed more slowly than of inspiration. That in the tip of an Indian naked dog's ear, there are no muscles found, although he command it into various and nimble motions. That the only use of the diaphragm, is to facilitate respiration by guarding the heart and lungs, that the lower viscera do not throng in upon them. That respiration is not needful to the motion or circulation of the blood. That although heretofore our author thought the air in inspiration to be mixed with the blood; yet he says, that after several experiments made, he could not by any good argument evince the same.

He has made the experiment, that whey tinged with saffron, being injected into the pulmonary artery, immediately runs into the left ventricle of the heart, without the assistance of inspiration. Neither does any blood at the same time break forth into the lungs. He supposes, that animal motions are not made by the influence of the animal spirits. But that in each part is seated a private sense, which is under the command of the soul. And that therefore there are no animal spirits, but those in the blood, called by the name of the calor nativus. That the suspension or intermission of inspiration for a certain time does not alter the pulse.*

* It is scarcely necessary to remark that this treatise of Dr. Ent's abounds in false physiology.

END OF VOLUME TWELFTH OF THE ORIGINAL.

PHILOSOPHICAL COLLECTIONS,*

Containing an account of such Physical, Anatomical, Chemical, Mechanical, Astronomical, Optical, or other Mathematical and Philosophical Experiments and Observations as have lately come to the publisher's hands. As also an account of some books of this kind.

A Letter of Mr. Leuwenhoeck to Dr. G.† containing an Account of his Observations lately made of vast Numbers of Animals in Semine Animalium. Philos. Collect. N° 1, p. 3.

Viewing the melt of a live codfish, I found the juices which ran from it full of small live animals, incessantly moving to and fro. I have also viewed the melt of pikes or jacks, and found an incredible number of small animals; and I judge that there were at least 10,000 of these creatures in the size of a small sand. These were smaller than those I observed in beasts, but their tails longer and thinner. I viewed also the matter in the vasa deferentia of a male hare 4 days after it was killed, and found it full of these small tadpoles swimming in a clear liquor, but they were without motion: the same I found also in the testicle. I examined also the matter in the vasa deferentia of birds, such as cocks and turkey cocks, and found it full of oblong bodies, larger in the middle than at each end, which I conceived to be animals. I viewed also the testicle of a dog taken out of the second skin, and discovered a vast number of small creatures. The semen of a cock about a year old, which had been kept alone in a coop for 5 days, I found full of those animals, at least 50,000 in the

* These Philosophical Collections, seven in number, were published by Mr. Rob. Hooke, while one of the secretaries to the Royal Society, the occasion of which was this: after the death of Mr. Oldenburg, who had compiled the Philosophical Transactions from the beginning to N° 136, inclusive, Mr. Hooke was elected one of the secretaries Nov. 30, 1677, and Dr. Grew at the same time the other secretary. In consequence, Dr. Grew continued the publishing the Philosophical Transactions in six additional numbers, ending with N° 142; after which, the publication was discontinued for four years, when they were recommenced in 1683 by Dr. Plot, who continued them from N° 143 to N° 166, inclusive. During the interval, however, Mr. Hooke, thinking to continue the work, or begin another like it, published these Numbers exactly in a similar manner and form, but changed the name from Transactions to Collections. These numbers of Mr. Hooke's then having always been considered, as it were, a part of the general work, and having been as such inserted in former abridgements, it has been thought fit to include them also in the present abridgement; and they are inserted here between N° 142 and N° 143, being their proper place in respect of the order of time of publication; distinguishing each paper in these seven numbers by printing in the titles the words, Philosophical Collections.

† Probably Dr. Grew.

size of a sand. Those were like river eels, which wriggled very much; these closing together would make a cloud, and separating again make it seem to disperse. I found them also in the vas deferens, in the epididymis, and in the vas præparans. By these observations you may judge that the testicle is made on purpose for the production of these animals, and to keep them till sent off.

How vast and almost incredible the number of these creatures is, you may somewhat the better conceive by the calculation which I have hereunto annexed. I have formerly said that in a quantity of the juice of the melt of a male codfish, of the size of a small sand, there are contained more than 10,000 small living creatures with long tails; and considering how many such quantities, viz. of the size of a sand, might be contained in the whole melt, I was of opinion, that the melt of one single codfish contained more living animals than there were living men at one time upon the face of the earth. That which induced me to be of this belief, was this following calculation. I conceive that 100 sands in length will make an inch, therefore in a cubic inch there will be 1,000,000 of such sands. And I have found the melt of a codfish to be about the quantity of 15 cubic inches, it must therefore contain 15,000,000 of quantities as large as a sand, now if there be 10,000 animals in each of those quantities, as I have computed there will be in the whole 150,000,000,000. To reckon now the number of men which may be on the face of the earth at once by guess. There are in a great circle, or in the compass of the earth 5,400 Dutch miles, thence I collect there must be 9,276,218 square Dutch miles for the earth's superficies. It is said $\frac{2}{3}$ of the superficies of the earth are water, and $\frac{1}{3}$ only is land; $\frac{1}{3}$ therefore of the last number is 3,092,072, which is the number of square miles of dry land on the surface of the earth: I suppose $\frac{1}{3}$ of this uninhabitable, and the other $\frac{2}{3}$ only inhabited, which $\frac{2}{3}$ contain 2,061,382 square miles. According to the computation of N. N. the number of people in Holland and West Friezland may be about 1,000,000. And if all the rest of the habitable parts of the world were as populous as these, there would be 13,385,000,000 of men at once on the face of the whole earth; but in the melt of the codfish I have computed that there are 150,000,000,000 of animals; the number of these therefore will exceed the number of men more than ten times.*

On Firy Damps in Mines. By Mr. J. Beaumont. Philos. Collect. N° 1, p. 6.

About two miles on the south-east of Stony Easton, at a place near Mendip-

* This very computation ought to have made the author aware of the extreme improbability of his opinion respecting the share which he ascribed to these his animalcula in the work of generation.

Hills, begins a running of coal, consisting of several veins, which extends itself in length towards the east about 4 miles: there is much working in this running, and fire-damps continually happen there: so that many men of late years have been there killed, many others maimed and burnt; some have been blown up at the works mouth, the turn-beam which hangs over the shaft has been thrown off its frame by the force of it, and those other effects whereof you had an account from other places are generally found. The middle and more easterly parts of this running are so very subject to these fire-damps, that scarcely a pit fails of them; notwithstanding which, our colliers still pursue their work: but to prevent mischief, they keep their air very quick, and use no candles in their works but of a single wick, and those of 60 or 70 to the pound, which nevertheless give as great a light there, as others of 10 or 12 to the pound in other places, and they always place them behind them, and never present them to the breast of the work.

When any are burned, the usual method they observe in their cure is this: they presently betake themselves to a good fire, and sending for some cows hot milk, they first bathe the burned places with that; when they have done this a while, they make use of an ointment proper for burns.*

On the great Over-flowing of some Rivers in Gascony. Philos. Collect. N° 1, p. 9.

In the beginning of the month of July 1678, after some gentle rainy days, which had not swelled the waters of the Garonne more than usual, in one night this river swelled so much, that all the bridges and mills above Tolouse were carried away by it. In the plains below this town, the inhabitants who had built in places, which by long experience they had found safe enough from any former inundations, were surprized by this; some were drowned, together with their cattle; others only saved themselves by climbing into trees, and getting to the tops of houses; and some others, who were looking after their cattle in the field, warned by the noise which this horrible and furious torrent of water made, rolling towards them like a sea, could not escape, but were overtaken, though they fled with much precipitation: this however did not last many hours with this violence.

* In slight and partial burns happening in this or other ways, cold water, applied before the skin becomes blistered, produces an immediate good effect; but in the more violent and extensive injuries of this kind, it has been found that a contrary method is proper, and that ulceration, deep sloughs, and mortification are best prevented by the application of spirit of wine, oil of turpentine, and other stimulants. The common oily applications are improper.

At the same time exactly, the two rivers only of Adour and Gaue, which fall from the Pyrenean hills, as well as the Garonne, and some other little rivers of Gascony, which have their source in the plain, overflowed after the same manner, and caused the same devastations.

The inhabitants of the Lower-Pyrenees observed, that the water flowed with violence from the entrails of the mountains, about which there were opened several channels, which, forming so many furious torrents, tore up the trees, the earth, and great rocks in such narrow places where they found not a passage large enough. The water also which spouted from all the sides of the mountain in innumerable jets, which lasted all the time of the greatest overflowing, had the taste of minerals.—In some of these passages, the waters were stinking, as when one stirs the mud at the bottom of mineral water, so that the cattle refused to drink of it, which was more particularly taken notice of at Lomber, where the horses were 8 hours thirsty before they would endure to drink it. So that M. Martell believes he had found out the true cause of this overflowing to be nothing else but the subterraneous waters: for if the heavens have not supplied this prodigious quantity of waters, neither by the rain, nor the melting of the snow; it cannot come elsewhere than from the bowels of the earth, from whence passing through divers channels, it had contracted and carried along with it that stinking and pungent quality.

An Account of the Sieur Bernier's Way of Flying. Philos. Collect. N° 1, p. 14.

The art of flying has been attempted by several persons, in all ages. Not to notice more remote and fabulous accounts, we may observe that it was believed possible by our countryman Friar Bacon, who assures us that he himself knew how to make an engine, in which a man sitting might be able to carry himself through the air like a bird: and affirms, that there was then another person who had actually tried it with good success. And it has been credibly reported that about the year 1640, one Mr. Gascoyn tried it with good effect; though he since dying, the thing also died with him.—The following method it is said was practised in France, between 1670 and 1680, and thus described.

The Sieur Besnier, a smith of Sable in the county of Maine, had invented an engine for flying, consisting of 4 wings, to be moved by the strength of the arms and legs of the man that flies.

It consists of 2 poles or rods, which have at each end of them an oblong chassie or wing of taffety, which folds from above downwards, as the frame of a folding window. In fig. 1, pl. 14, A represents the right wing before, B the left wing behind, C the left wing before, and D the right wing behind; E is a small string tied from the wing B to the left foot, by which it is moved downwards, at the same time that the wing A is moved downwards by the right hand. F is a string fastened to the right leg, by which the right wing D behind is made to descend at the same time that the left hand draws down C the left wing before. When the person designs to fly, he fits these poles upon each shoulder, so that 2 of the chassies may be before him, and the other 2 behind him. Those chassies or wings which are before him, are moved or struck downwards by the strength of his arms and hands, by which the hinder chassies are lifted up, and those hinder wings are pulled or struck downwards by the legs, which pull them down by the 2 strings which are fastened both to the legs and hinder wings.—The manner and order of moving these wings is thus: when the right hand strikes down the right wing before, the left leg by means of the string E pulls downward the left wing behind B; then immediately after, the left hand moves or strikes downwards the left wing before C; and at the same time, the right foot by the string E, moves or pulls down the right wing behind D, and so successively, and alternately, the diagonally opposite wings always moving downwards, or striking the air together.

Two things seem to be wanting to this machine. First, to find out a substance that shall be exceedingly light, and yet of great extension, which being applied to any part of the body, it shall be able to suspend the weight of the man in the air: the second is the fitting and adjusting of a tail, because this will help both to support, and also to steer or guide the flier. But from divers experiments that have been made by several persons without effect, it is conceived that these motions and directions to the tail cannot be given without much difficulty.

But he pretends not nevertheless to be able to raise himself from the earth by this his machine, nor to sustain himself any long time in the air, by reason of the want of strength and quickness in his arms and legs, which is necessary to move these kind of wings frequently and efficaciously enough; but yet he is confident that from a place pretty high elevated into the air, he shall be able to pass over a river of a considerable breadth, having already done as much from several heights, and at several distances.—He began his trials first by springing out himself from a stool, then from the top of a table, then from a pretty high window, then from a window in a second story, and at last from a garret, from whence he flew over the houses of the neighbours; practising thus with it by little and little, till he had brought it to the present perfection.

*Of a Flying Ship.** *By Fr. Lana.* *Philos. Collect.* N° 1, p. 18.

But no one yet thought it possible to make a ship which should pass through the air, as if it were sustained by the water, because they have judged it impossible to make an engine which should be lighter than the air, which is, nevertheless, necessary to be done in order to produce this effect. But I hope at length I have fallen on a way of making such an engine as shall not only, by its being lighter than the air raise itself in the air, but together with itself buoy up and carry into the air men, or any other weight; which I confirm both by certain experiments, and by geometrical demonstration.

I suppose first, that the air has weight, because of the vapours and exhalations, which are raised from and incompass our terraqueous globe to the height of many miles. And the proof of it may be made by evacuating, if not all, yet a great part of the air out of a glass-vessel, which having been first weighed, and after the extracting of the air weighed again, will be found notably lessened in weight. Now how much the weight of the air is, I have found in this manner: I took a large glass-vessel, the neck of which could be shut and opened by a stop-cock; and being open I heated it at the fire, so that the air in it being rarefied, issued out of it in great part: then I suddenly shut it, so that the air could not re-enter, and weighed it: which done, I sunk the neck under water, the body of the glass remaining all above the water; and opening it, the water ascended into the glass, and filled the greater part of it. Then I opened it again, and let out the water, which I weighed, and measured the bulk and quantity thereof. Whence I inferred, that so much air had issued out of the glass, as there was water that had entered to fill the part left by the air. I again weighed the vessel, first well wiped dry, and I found that it weighed an ounce more whilst it was full of air, than it did when the greater part of it was evacuated. So that that surplus of weight was a quantity of air, equal in bulk to the water that had entered into the place of it. Now that water weighed 640 ounces: whence I conclude, that the weight of the air, compared with that of the water, is, as one to 640, that is, if the water which fills a vessel weighs 640 ounces, the air filling the same vessel weighs one ounce.† I suppose, secondly, that a cubic foot of water weighs 80 pounds, or 960 ounces; for I found that that water which weighed 640 ounces, was little less than $\frac{2}{3}$ of

* This account is extracted and abridged from the 6th chapter of Fr. Lana's book called Prodromo.

† This method of finding the relative weight of air, in respect of that of water, is ingenious enough; though the experiment here described has not been accurately performed, since instead of 640 times, it is now well known that water is 830 times heavier than air.

a cubic foot; whence it follows, that if $\frac{2}{3}$ of a foot of air weighs an ounce, a whole foot will weigh $1\frac{1}{2}$ ounce.—I suppose, thirdly, that any great vessel may be altogether evacuated of air, or at least of the greatest part of the air, in the following manner. Let any great glass-vessel be taken, that is round and has a neck, and let to the neck be fastened a brass or tin tube, the longer the surer the effect will be. Let there be near the said vessel a stop-cock, so closing the glass that no air can enter. Fill the whole glass, and the whole pipe full of water; then shutting the latter in the extreme part, let the vessel be inverted, so that it stand on its upper part, and let the extreme part of the tube be immersed in water; and whilst it is immersed in the water, let it be opened, that the water may issue out of the vessel; which will all go out of it, except part in the tube, and the remaining space above will be empty, there being no way for the air to enter: then shut the neck of the vessel with the stop-cock, and the vessel will be empty.—I suppose, fourthly, it proved that the superficies of spheres increase in a duplicate proportion to their diameters, and their solidity in a triplicate.—I suppose, fifthly, that where one body is lighter in specie than another, the lighter ascends in the other that is heavier, if the heavier be a liquid body; as a ball of ordinary wood on water, because it is lighter in specie than water; so also a ball of glass full of air will swim at the top of water, because though glass be heavier than water, yet taking the whole compound of the ball, glass and air together, it is lighter than that, which is only a body of water.

These things being supposed, it follows, that if we could make a vessel of glass or other matter, that might weigh less than the air that is in it, and should draw out all its air, this vessel would be lighter in specie than air itself, so that it would swim on the top of the air, and ascend on high.—Let the capacity of the vessel be increased more and more, and we shall arrive to such a size, that although it be made of a dense and heavy matter, yet the weight of the air it may contain shall exceed the weight of the matter that makes up the surface of that vessel.—Let us see then of what determinate size a brass vessel may be made; and let us suppose the thinness of the brass to be such, that a plate of it a foot broad and long may weigh 3 ounces; which is not difficult to make. Let us make of such thin plate a round vessel of the diameter of 14 feet: then such a vessel will weigh less than the air in it. So that the air being exhausted out of it, and the bare vessel remaining lighter than an equal bulk of air, must needs of itself mount up into the air. For the surface of the globe will be 616 square feet of brass-plate, each of which we have supposed to weigh 3 ounces, and the whole 1848 ounces, or 154 pounds. But the capacity of the vessel will be $1437\frac{1}{3}$ feet; and the weight of the whole air contained in that vessel will be 2155$\frac{1}{3}$ ounces, or 179 pounds and 7$\frac{2}{3}$ ounces: so that by drawing out that air, the vessel is 25 pounds and 7$\frac{2}{3}$ ounces lighter than air; so that that air being evacuated, the vessel will not only ascend into the air, but also carry with it on high a weight of 25 pounds and 7$\frac{2}{3}$ ounces. And thus by increasing the bulk of the vessel, without increasing the thickness of the brass plates, he supposes a kind of ship or other vessel may be made to rise up in the air, and carry men up with it.*

An Occultation of Jupiter by the Moon, June 5, New Style, in the Morning, Observed at Dantzic, by M. Hevelius. Translated from the Latin. Philos. Collect. N° 1, p. 29.

Although I have watched and observed the heavens for 50 years past, yet I have only once in all that time seen Jupiter covered by the moon, which was in the year 1646, Dec. 24, New Style, in the evening, when the sun was under the horizon. So that I very much congratulate myself that I could make this observation, not only with a very clear sky, but also according to my wish, in the presence of my very welcome guest, the learned and celebrated Mr. Edmund Halley.†

Jupiter entered behind the moon at mount Audus; and, as far as could be expected from its exit, it passed through the Loca Paludosa of the island Carcinna, over mount Actua, through the island Besbica, through Byzanticum, the island Apollonia, and the upper part of Palus Mæotis; so that he went a little above the moon's centre; the moon having then some south latitude. Then, which is very uncommon, we measured, I think very accurately, the apparent diameter of Jupiter in this observation. I remember that formerly I have several times measured that diameter by means of the lunar spots, and found

* Father Lana next proceeds to explain and direct the methods of making the machine, of exhausting it, and obviating some difficulties which he conceives may occur in that process, and in the managing of it, when floating in the air, by endeavouring to give it any particular direction; also to cause it to rise higher, he directs to throw out ballast or weights, and in order to descend lower when needful, open a stop-cock to let out more air, &c. much in the manner as practised by the aeronauts of the present day. And although the business could not be accomplished in his way of managing it, nor the sides of so large and thin a vessel be sustained from crushing by the pressure of the atmosphere, for want of the knowledge of another kind of air, as elastic as the atmosphere, and yet many times lighter, to fill the globe with, thereby rendering it at once both light and strong; yet the paper is very ingenious and curious, and probably may have furnished hints to the more modern philosophers to bring to perfection, in our time, what Father Lana and others had only surmised and conceived as possible.

† Who had been sent to him to settle a dispute with Mr. Hook, concerning the difference in observing, between plain and telescopic sights.

that it came to about 55″; but now the same diameter came out much less: for, the whole duration of this occultation being known, viz. $58^m\ 10^s$, and at the same time the diameter of the moon being 32′ 40″; it presently becomes known, from that moment of time when the first limb of Jupiter touched that of the moon, and when again his body was just all hid, which took place in the space of 55^s, that the diameter of Jupiter comes out 30″ 53‴.

The principal observations were the following, the times of the pendulum clock being corrected by observed altitudes:—The moon rose at $1^h\ 54^m\ 25^s$; the sun rose at $3^h\ 28^m\ 25^s$; Jupiter touched the eastern limb of the moon at $4^h\ 18^m\ 5^s$; the planet was wholly covered at $4^h\ 19^m\ 0^s$; a considerable part of Jupiter was emerged at $5^h\ 16^m\ 25^s$; and the whole of the planet was emerged at $5^h\ 17^m\ 10^s$.

The Observation of the Eclipse of Jupiter and his Satellites by the Moon, the 5th of June 1679, *N. S. By M. Cassini. Philos. Collect.* N° 1, p. 33.

Having found by calculation, that the moon ought to eclipse the planet Jupiter the 5th of June this year 1679; I was ready with a 20-foot telescope to observe after the rising of the moon, with which I discovered 3 satellites: the 1st was distant westward of the limb of Jupiter a little less than a diameter; the 2d was distant on the east side a little more than a diameter; the 3d was more eastward than the 2d, by somewhat less than a diameter of Jupiter; and the 4th being less than the other, which according to calculation, should have been near its greatest digression eastward, appeared not by reason of the crepusculum.

At $3^h\ 0^m\ 11^s$, the first satellite was hid by the east limb of the moon. At $3^h\ 2^m\ 0\frac{1}{2}^s$ the east side of the moon touched the west side of Jupiter: then I took the height of Jupiter, which was 8° 1′, at $3^h\ 2^m\ 51^s$. At $3^h\ 2^m\ 57^s$ Jupiter was entirely hid by the moon. He entered at equal distance from the two spots Grimaldi and Aristarchus; the last of which was in the section of the moon, which distinguished the light from the dark part. At $3^h\ 5^m\ 1^s$, the 2d satellite was hid by the east side of the moon. At $3^h\ 5^m\ 48^s$ the 3d satellite was hid, &c.

Then followed a multitude of small clouds of the colour of shining fire, which were formed near the horizon. Whilst we were looking at these, without losing sight of the moon, we perceived by the eye that Jupiter was parted from the obscure side of the moon at $3^h\ 56^m$.

M. De la Hire, who observed near the gate of Montmartre, 2 minutes after the parting, took the height of Jupiter, and found it 17° 17′. We afterwards made divers observations for determining the parallax and the diameter of the moon, and for adjusting the time, having observed Jupiter till $6^h\ 3^m$ in the morn, at the height of 36°, as well with a telescope of 20 feet as with one of 3, which is remarkable, because Jupiter is wont to disappear in the crepuscule some minutes after sun-rising.

Twelve Problems in compound Interest and Annuities, resolved by Adam Martindale. Philos. Collect. N° 1, p. 34.

Twelve problems in compound interest and annuities, expressed in symbols, to be resolved by logarithms, and distinguished into three ranks, whose symbols are thus to be understood:

p	Signifies	Principal	Common to all the 3 ranks.
r		Rate, viz. 1 l. with its rate	
t		Time	
a		Amount or aggregate	proper to 1 Rank.
s		Sum of principal and arrearages	proper to 2 Rank.
d		Difference of principal and worth	proper to 3 Rank.

Their capitals stand for the logarithms of the number signified by the small letters.

D signifies data, or given; Q quæstitum, Prob. problem, Res. resolution. The first rank touching compound interest for a single sum of money.

1 prob. D. *p, r, t.* Q. *a*? Res. $R^t \times P = A$.

2 prob. D. *a, r, t.* Q. *p*? Res. $A - R^t = P$.

3 prob. D. *p, a, t.* Q. *r*? Res. $\frac{A - P}{t} = R$.

4 prob. D. *p, a, r.* Q. *t.* Res. $\frac{A - P}{R} = t$.

The second Rank, concerning Annuities in Arrear at Compound Interest, grounded upon these two Axioms.

1. The annuity and rate of interest being given, the principal correspondent to the annuity is in effect given also, being easily found out by the Rule of Three, thus;

As the Interest of any principal, ex. gr. of 1, 10, 100, &c. is to that principal :: so the annuity or pension, to its principal.

2. The sum of the principal and the arrearage of all the payments being found, the arrearages alone may be obtained, by subtracting the principal from that sum.

The Problems are these that follow.

1 prob. D. *p, r, t.* Q. *s*? Res. $R^t \times p = S$.
2 prob. D. *s, r, t.* Q. *p*? Res. $S - R^t = P$.
3 prob. D. *p, s, t.* Q. *r*? Res. $\frac{S-P}{t} = R$.
4 prob. D. *p, s, r.* Q. *t*? Res. $\frac{S-P}{R} = t$.

The third rank concerning annuities anticipated, or bought for a sum in hand, or equivalent thereto, at compound interest discounted, founded on the former of the two axioms above mentioned, and this that follows:

If the difference and worth be once found, the worth is easily obtained, by subtracting that difference out of the principal, which is ever greater, being the worth of the annuity at that rate for ever.

1 prob. D. *p, r, t.* Q. *d*? Res. $P - R^t = D$.
2 prob. D. *d, r, t.* Q. *p*? Res. $D \times R^t = p$.
3 prob. D. *p, d, t.* Q. *r*? Res. $\frac{P-D}{t} = R$.
4 prob. D. *p, d, r.* Q. *t*? Res. $\frac{P-D}{R} = t$.

An Account of Books. Philos. Collect. N° 1, p. 38.

Marcelli Malpighii Phil. et Med. Bononiensis, è Societate Regia Anatomes Plantarum, Pars Altera. Lond. 1679.

This ingenious author having, in the first part of his anatomy of plants, given us the description of the several parts a plant consists of, together with their uses, and the method and order of vegetative motion; in this second part, he explains the causes and descriptions of the usual accessaries and accidents of vegetables: as, the method of growing, from the first motion of the seed, and what change it daily undergoes, &c. Next he shows, that galls, or preternatural tumors of plants, are receptacles or nurseries of the eggs of insects. Then he treats of the excrescences and other morbid tumors from some fault of the plant itself. Then of the hairs or thorns which plants are armed with. And in like manner of the tendrils, or the hands wherewith weak plants lay hold upon some firmer support; and with no less exactness of the plants that grow upon others. Of roots, and in what manner buds are shot out: and herein of bulbous plants that are but annual, and yet renew and propagate from themselves without the mediation of seeds. And all these things are so illustrated by figures well graven after the author's own exact delineations, that there is hardly any room left for addition.

Memoires pour servir a l' Histoire des Plantes dressés par M. Dodart, de l' Academie des Sciences, Docteur en Medicine de la Facultè de Paris.* 2d *Edition, à Par.* 1679. *Philos. Collect.* N° 1, p. 39.

This book contains the designs of the Royal Academy of France for perfecting the knowledge of vegetables, and that design likewise in some manner pursued. It consists of five chapters; in the first of which they tell us, how they intend to describe plants.

In the 2d chap. we learn, that the figures will be as large as one leaf of the noble French paper will admit. If the plant be too large, it will be represented in pieces, all, or some at least, of the natural size, with their seeds, flowers, &c. and some parts and small plants as they appear in the microscope. The 3d chap. promises a trial of several means to ascertain the nature of soils, the causes of their particular disposition to certain plants, &c. &c. The 4th chap. treats of the virtues of plants, and in particular of their analysis, or the products obtained from them by subjecting them to the action of fire; showing that by this method they yield the following results: 1. Acid spirits. 2. Essential oils. 3. Sulphureous spirit. 4. Simple waters. 5. Waters with an acidity mixed,

* Denis Dodart was born at Paris in 1634. After going through the usual preliminary studies in that university, he determined to embrace the medical profession, to which he accordingly applied with extraordinary assiduity; and at length became physician to the King, as well as to the Dowager Princess of Conti, and the young Prince of Conti. Although the history of plants occupied a large portion of his time, he nevertheless engaged in a laborious set of experiments (conducted after the manner and example of Sanctorius) on insensible perspiration; and moreover made many practical observations on venesection, on diet, &c.; on all which subjects some manuscripts were found after his death, which, however, have never yet been published. It should also be mentioned, that he once thought of writing histories of medicine and music; in the last of which he was well skilled, both theoretically and practically. He died in 1707, aged 73; leaving a son whom he had brought up in his own profession; and who, like the father, had the honour of being appointed physician to the King.

In private life M. Dodart was esteemed for his mild but dignified demeanour, for his unreserved and instructive conversation, for his benevolent and charitable disposition, for the purity of his moral principles, and for his sincere belief in the christian religion.

Notwithstanding his engagements at Court, and among the higher orders of society, he never neglected the poor. He was at the same time the physician and companion of the great, and the friend and comforter of the needy. It was his felicity (as Fontenelle has remarked) to receive the stroke of death while he was occupied in performing charitable actions, being seized with a fever (which terminated fatally in a few days), in consequence of the fatigue he underwent and a cold he caught in visiting and prescribing for the indigent sick. It is by such men that the great purposes of the medical profession are fulfilled, and that its dignity and utility are rendered conspicuous. Such characters, whether they belong to our own country or to foreign nations, can never be contemplated without pleasure, nor mentioned without praise.

which though not tastable, is yet found and distinguished by other means. 6. Waters with sulphur in like manner insensible, and to be found by other scrutinies. 7. Acid spirits. 8. Mixed spirits of acid and sulphur. 9. Urinous spirits. 10. Acid urinous spirits. 11. Volatile salts. 12. Black burnt oils. 13. Salts fixed, saline or lixiviate. 14 Earth.*

These are supposed alterations by fire, rather than separations. But for distinguishing, several ways have been thought of, as, the manner of process, ascertaining the degrees of heat by a thermometer, &c. That culinary heat, and the heat of the stomach may make the same alterations and separations.

For rectifying these substances, little is used. The particulars of the process are remarked and registered, as the time, degrees of fire, sensible qualities of the parts, &c. And these according to their proportionable quantities and degrees, and that with great care, exactness, and judgment, collected from different ways of examination: taking an account of their weight, gravity, (of which many instances are given). Insensible qualities, found by divers mixtures; as of acids, sulphurs, &c. with turnsole. Volatile spirits, &c. make solution of sublimate white. Sea-salt whitens solution of sacharum saturni. Mixed spirits redden solution of vitriol, &c.

Next the trying of these extracts in vacuo is propounded to find their air. And several observations of the differences of volatile and fixed salts related. Also ways are shown how to find the degrees of composition of these extracts: particular ways for particular plants. Lastly, the way of analysing juices is mentioned; and the advantages of this process.

In the fifth and last chapter is stated the method which the Royal Academy intend to take for completing and publishing the History of Plants.

Osservationi intorno alle Torpedini fatte da Stephano Lorenzini Fiorentino. In Firenze, 1678. *Philos. Collect.* N° 1, p. 42.

In the present treatise, the author gives a description of the torpedo. Noticing, 1. That this fish is of the flat cartilaginous kind;† that there are two

* Great expectations were once entertained by the French academicians, as to the possibility of ascertaining the virtues of plants by this mode of decomposition; and an extensive set of experiments was accordingly instituted. But after much time and expence, it was found that this object was not thus attainable. In fact, vegetables which produce the most opposite effects upon the human body, yield, when subjected to destructive analysis by fire, similar results. These experiments however were by no means useless in a chemical point of view.

† It belongs to the Linnæan genus Raja, and much light has within these few years been thrown upon the structure and use of some of its parts, by the late Mr. John Hunter. See Phil. Trans. vol. 63.

kinds, a greater and a less; some of the one weighing 25 lbs. and of the other 6 ozs. 2. That in the skin there are several pores, which are the ends of so many pipes or ducts that bring slime to make the skin slippery, and which are accompanied with so many others which go from the skin to the spina dorsi, the rise of the former being in the glands, between the eyes and the edge of the fish, which swim in a liquor in a cavity made by appendices of the cranium, which glands separate this slime from the blood. And the latter or smaller pipes carry back the superfluous. 3. That the brain touches the pia mater in the base only, being separated elsewhere by water in which the brain swims. 4. That the heart has but one auricle, and that it will move 8, 9, or 10 hours after it is separated from the body. 5. It has an ovarium near the liver, and a double oviduct and womb (for it is viviparous) wherein the eggs swim, till from a cicatricula a perfect fœtus is formed. The conception, during its formation, having no communication with the dam, by vessel, membrane, or otherwise. And here he describes the genitals of other creatures, and amongst them of a lobster and crab, wherein he says Dr. Willis has mistaken penultim for ultim. 6. That the chief wonder of this animal, and that which gives it its name, is the benumbing faculty which is seated in the two semicircular or falcated muscles on each side of the thorax, which consist of fibres, irregular, but as large as a goose-quill, and made up of bladders filled with a kind of water; one end of these fibres being fixed to the skin of the belly, and the other end to that of the back, on which may be plainly seen the vestigia of the fibres ends. Now when the fish contracts those fibres, there issue out corpuscles fitted to the pores of a man's skin, so as to enter upon immediate contact, but not otherwise, and disturb the posture of the parts, and to cause pain, as when one's elbow is hit or knocked; and this comes most by the fingers ends, because there are ends of tendons. And this pain is more or less, as the contraction of the fibres have immitted more or less. 7. The fish takes in water through several holes near the stomach, and puts it out at the other end, forcing it in the passage to scour the bronchia or lungs, which in creatures that have a thick fuligo to be separated (as this) are convex, in others are concave. 8. He seems to deny a tongue to this as well as to most other fishes, that which is called so being devoid of the papillary instrument of taste, which is the primary use of a tongue. 9. He observes that the stomach, guts, &c. are short, large, and turn very little, and consist but of one sort of fibres, namely circular, whereas others have a pair of cross winding screws. The weak peristaltic motion being made amends for by a copious dissolvent, which oozes out of small teats on the inside of the stomach, and soon reduces fishes, which this animal swallows alive of all sorts, to a liquor fit enough for the lacteals.

An Account of Okey-hole, and several other Subterraneous Grottos and Caverns in Mendip-hills, Somersetshire, &c. By Mr. John Beaumont, Jun. Philos. Collect. N° 2, p. 1.

On the south side of Mendip-hills, within a mile of Wells, is a famous grotto, known by the name of Okey-hole, much resorted to by travellers. The entrance of it is in the declivity of those hills, which is there environed with rocks, having near it a precipitous descent about 10 or 12 fathoms deep, at the bottom of which there always issues from the rock a considerable current of water. The naked rocks above the entrance show themselves for about 30 fathoms in height, though the whole ascent of the hill above it is about a mile, and is very steep.

At first entering this vault, you go upon a level; but advancing farther into it, you find the way rocky and uneven, sometimes ascending and sometimes descending. The roof in the highest part is about 8 fathoms from the floor, and in some places it is so low that a man must stoop to pass through. Its width is also various, in some parts it is about 5 or 6 fathoms, in others not above 1 or 2; it extends itself in length about 200 yards.

At the farther part of this cavern there rises a good stream of water, large enough to drive a mill, which passes all along one side of the cavern, and at length slides down about 6 or 8 fathoms between the rocks, and then pressing through the clefts of them, discharges itself into a valley. This river within the cavern is well stored with eels, and has some trouts in it, which must of necessity have been engendered there, and not come from without, there being so great a fall near the entrance. In a dry summer I have seen a number of frogs all along the cavern, to the farthest part of it, and other little animals in some small cisterns of water there. Before arriving at the middle of this vault, you find a bed of very fine sand, which is much used by artists to cast metals in. On the roof, at certain places, hang multitudes of bats, as usual in all caverns whose entrance is upon a level, or somewhat ascending or descending.

The next cavern of note lies about 5 miles from this, on the southwest part of Mendip-hills, near a place called Chedder, famous for cheese; from this place you may pass up a narrow valley about a mile in length, being bounded with precipitous rocks on the east and west, some of a very considerable height. To enter into this cavern, before you reach half way this valley, you must ascend about 15 fathoms on those rocks which bound it to the east. This cavern is not of so large extent as the former, neither has it any thing peculiar in it. These two caverns have no communication with the mines.

It is generally observed, that wherever mines of lead-ore are, there caverns of various kinds and situations are found. The most considerable in Mendip-hills is a cavern in a hill called Lamb. First a perpendicular shaft descends about 10 fathoms, then you come into a leading vault, which extends itself in length about 40 fathoms; it runs not on a level, but descending, so that when you come to the end of it you are 23 fathoms deep by a perpendicular line; the floor of it is full of loose rocks: its roof is firmly vaulted with lime-stone rocks, having flowers of all colours hanging from them, which present a most beautiful object to the eye, being always kept moist by the distilling waters. In some parts the roof is about 5 fathoms in height, in others so low that a man has much ado to pass by creeping; the width is mostly about 3 fathoms. This cavern crosses many veins of ore. About its middle, on the east side, lies a narrow passage into another cavern, which runs between 40 and 50 fathoms in length. At the end of the first cavern there opens another large one.

I have been in many other caverns upon Mendip-hills. The frequency of caverns on those hills may be easily guessed at, by the frequency of swallow-pits, which occur there in all parts, and are made by the falling in of the roofs of caverns; some of these pits being of a large extent, and very deep; and sometimes our miners, sinking in the bottom of these swallows, have found oaks 15 fathoms deep in the earth.

On the Variation of the Needle; and of the Phosphorus of Dr. Kunkel. By J. Chr. Sturmius. Philos. Collect. N° 2, p. 8.

After many trials repeated several days one after another, and with various ways of examination, still in every one of them with the same success, he certainly found that the north end of the magnetic needle, which the former age always reported to us to vary from the north, and to direct or point more towards the east by several degrees, did now decline towards the west near 5 degrees. On the renewing of the Society of the Curiosi in Germany the third time, I had occasion the last summer to make many experiments with the pneumatic engine: among the rest, having prepared a large receiver of copper of 2 feet diameter, made by joining two hemispheres together, so as that they could at pleasure be separated again; I made one trial not unpleasant: I sealed up a round glass hermetically, and covered it with a double bladder very carefully, and including it in the spherical receiver, I found that after about 200 exhaustions had been made, it broke all in pieces in the receiver with a very great noise, by the elasticity of the air inclosed in the glass globe.

Concerning the noctiluca or phosphorus of Dr. Kunckel,* I suppose you may before this have received some account, but whether you have heard of his pills which he calls miraculous, I much doubt: I thought therefore it might not be unacceptable to you to receive this following account of them. This chemist, Dr. Kunckel, prepares out of the same condensed light, which by his skill he knows how to extract out of any kind of terrestrial body whatever, as if it were there naturally placed, certain pills about the size of peas, to which he ascribes very strange comforting and medicinal virtues; these being moistened a little, and in the dark scraped with one's nail, or a knife, or the like, do yield a very considerable light, with a smoke also; but they afford a light yet much more pleasant and strange, if about 8 or 10 of them be put into a glass of water, and therewith shook in the dark; for thereby all the water and the cavity of the glass will seem perfectly filled with light, flashing by turns very briskly. Dr. Kunckel has also reduced the same lucid matter into the form of larger stones,

* John Kunckel, one of the most celebrated chemists of the 17th century, was a native of Holstein, and was born in 1630. After being instructed in pharmacy, he turned his attention to metallurgy, glass-making, and other branches of technical chemistry. Having seen a specimen of phosphorus that had been made by a German chemist named Brandt, he determined to prepare some himself, though Brandt kept the process a secret. It was known, however, that the last mentioned chemist had long been busied in making experiments on urine; accordingly Kunckel conjectured that it was from this excretory fluid that the phosphorus was extracted. Though he repeatedly failed, he still persevered, and after 4 years labour he succeeded in accomplishing his object. In his situation as director of the glass works near Potsdam, he had excellent opportunities of trying experiments on the coloration of glass, and accordingly brought the method of making ruby-glass to great perfection. He manufactured large quantities of it, which he sold (as Lewis mentions in his Commercium Philosophico-technicum) for about 40 shillings an ounce. A chalice of this ruby-coloured glass which he made for the Elector of Cologne, weighed not less than 24lbs, was a full inch thick, and of an uniform fine colour throughout. The principal tinging material which he employed for this purpose appears to have been Cassius's precipitate of gold. He hints, however, that he used other additions; that much depends on the degree and continuance of the fire; that a smoky flame often served to heighten the colour; and that the process after all frequently failed. Kunckel's practical knowledge in this and other chemical arts led him to be employed at different times in the service of the Electors of Saxony and Brandenburg, and of the King of Sweden; by which sovereign he was ennobled, with the title Von Lowenstern. The exact time of his death is not recorded; but he was known to be living when he was between 60 and 70 years of age. His observations on phosphorus; on the art of making glass; on acid and urinous salts, &c. besides his work entitled Laboratorium Chemicum, were written in German; but have been translated into Latin and other languages.

Among foreigners, Kunckel is entitled to the discovery (equally with Brandt) of preparing phosphorus from urine; but from the papers deposited by Mr. Boyle with the secretaries of the Royal Society and opened after his death, it would appear that this indefatigable experimenter, our countryman, had found out the method of obtaining this curious product sometime before. This circumstance we shall barely mention here, postponing the further consideration of it until we come to that part of the Transactions containing Mr. Boyle's communication on this subject.

which being warmed by the hand, but especially if they be a little scraped or rubbed on a paper or table, describe letters very legible in the dark.

Account of a Body, after being long buried, almost wholly converted into Hair. Philos. Collect. N° 2, p. 10.

About 43 years since the body of a woman was buried in this place, (Noremberg), in a dry and yellow earth, as the earth for the most part is near this city. The corps lay the lowest of three in the same grave, there being two other corps over it; the ground, bones, and ashes of which being removed, this coffin began to appear; through the clefts of which much hair was thrust out, and had grown very plentifully, so that it is believed that the whole coffin may for some time have been all covered with hair. The cover of this coffin being removed, the whole corps appeared perfectly resembling a human shape, exhibiting the eyes, nose, mouth, ears, and all the other parts; but from the very crown of the head to the sole of the feet covered over with a very thick set hair, long and much curled. Which strange sight much amazed the sexton and his companions; but after a little viewing of it, going to handle the upper part of the head with his fingers, he found immediately all the shape of the body to fall, and left nothing in his hand but a handful of hair, there being neither skull nor any other bone left, unless it were a very small part of that which they suspect to be the great toe of the right foot. This hair was somewhat rough at first, but afterwards it grew very much harder, and of a brown red colour, of which the enclosed papers present you a sample.

This paper of hair was seen at a meeting of the Royal Society, and remains in their repository at Gresham College, and it is found to be a stiff, red, somewhat curled, but rotten hair.

Some Anatomical Observations on Hair found in several Parts of the Body; as also Teeth, Bones, &c. with parallel Histories of the same observed by others. Communicated by Edward Tyson, M. D. and F. R. S. Philos. Collect, N° 2, p. 11.

It is the opinion of the learned Honoratus Fabri (lib. 3, de Plantis) and others, that hair, wool, feathers, nails, horns, teeth, &c. are but animal vegetables or plants; if so, we may be the less surprized at their growth on the body, even after the decease of the animal, as in the foregoing and many other instances. Petre Borelli (Hist. et Obs. Med. Cent. 1, Obs. 10,) thinks that as plants they may be transplanted, and made to grow in a soil they did not at first. What he relates concerning teeth being drawn and set again, I know to be true, having tried it formerly in myself, and have heard of the like done by others. As for hair, the outward surface of the body is the usual place where it grows; and of teeth, the mouth. But of late I have met with instances, that even in the most inward parts of the body I have observed these vegetable excresences. Dr. T. then adds a long account of such cases, part of his own observing on dissections, and many instances extracted from the writings of others; from which it appears that hair, and other matters, have been found in many unusual places; as, on the tongue, on the heart and within it, in the breasts and kidneys, besides other glandular and muscular parts of the body; but that there is scarcely any internal part more subject to it than the ovaria or testicles of females.

Account of a Medicinal Experiment of Johannes Baptista Alprunus, M.D. Physician to the Empress Eleonora, and appointed to take Care of Persons infected with the Sickness [Plague] tried upon one so infected, and published by himself at Prague, 1680. *Philos. Collect.* N° 2, p. 17.

No poison is greater than that of the sickness [plague]; our outward senses are not affected by it, and our understanding does not comprehend it; it is aërial and volatile, and it is fixed and coagulated when it concretes into bubo. Hence I conceived that the way for me to penetrate into the most latent quality of this pestiferous venom was by chemistry, not with knives but glasses, not with iron but fire. Having lanced a pestilential boil of Mr. Godfrey Reshel, I collected the virulent matter, and putting it in a retort, and luting a receiver to it very close, I applied degrees of fire: at first came over a water, then a more fat and oily matter, and at last a salt ascended into the neck of the retort. The fire being removed and the glasses separated, there came forth so great a stench that 1000 wounds exposed to the summer heat could not have equalled it. And though I thought I had sufficiently armed my senses against it, that is, my ears with cotton, my nose with pessaries, my mouth with sponges, all dipped in vinegars and treacles, yet as if touched with a thunderbolt I was struck with a violent trembling of my body. To make short, having broken the glass, I gave some of this horridly stinking salt to Mr. Reshel to taste, and then I tasted it myself, and it was found to have an acrimony as great as aqua regis. To this acrimony the author ascribes all the phænomena which occur in the plague; for the cure of which he employed sudorifics and cordials.*

* Little light is thrown upon the nature of contagion by this chemical observation. Pus taken from a common ulcer or abscess would doubtless yield, when urged by the fire, a product as fetid and as acrimonious as that obtained in the above recited experiment.

The Way by which the Author preserved himself in this Contagion.

Because no alexipharmic is sufficient in this contagion, therefore grounding my judgment on the principles of Harvey about the motion of the heart, and the circulation of the blood, and some other of Bartholine and others, I concluded this pestiferous venom, attracted by the breath or pores by the circulation of the blood, to be carried to the axillary and inguinal glandules, &c. where if it long stagnates it concretes into buboes which tend to maturation; but if it opens itself a way, and passes with the natural motion of the blood, and so is carried to the heart, then death ensues. Therefore not only for myself, but for two other friends, I made incision with a lancet, in inguine dextro et sinistro, and put in a seton, to the end that by this artificial way the venom might find a passage. This I often tried with good success, great quantity of matter always voiding that way, but more notably when I was any ways touched with pestilential strokes or alterations. By the help of which I kept myself in good health.*

An Universal natural Preservative against Infection, published for the common Good. By Jacobus Johannes Wenceslaus Dobrzensky de Nigro Ponte, Doctor and Professor of Philosophy and Physic, Prague, 1680. *Philos. Collect.* N° 2, p. 20.

This universal natural preservative is this: That whoever converses with patients affected with any disease whatever, if he would preserve himself from infection, must be sure so long as he abides within the sphere of their steams never to swallow his spittle, but to spit it out. For he conceives that to be the part which first and most easily imbibes the infection; and by that swallowed, the infection is carried as by a proper vehicle into the stomach, where it works those dismal and fatal effects.

The infection of pestilential fevers (he observes) proceeds from a seminal ferment, which is emitted by the patient in the form of steams into the incompassing air, and so infects all things within a certain sphere or distance. This drawn into the mouth by the breath is apt to infect the saliva or spittle, which being swallowed infects the stomach, and so the rest of the body; but being spit out frees the body from infection. And therefore he conceives that strong smelling and strong tasting substances kept in the mouth, and chewed to pro-

* Issues and setons have been much commended as preservatives from pestilential contagion by other medical writers; but it is peculiar we believe to the author of these observations to make them in the places here mentioned; where they must always be extremely troublesome.

mote spitting, are of very good and necessary use for physicians, surgeons, and apothecaries, &c. that are necessitated to visit infected persons.*

Account of a strange Birth which in May last happened at Hilbrewers in Somersetshire. By Mr. A. P. Philos. Collect. N° 2, p. 21.

Nature here designed and made preparation for twins. For the joining of these two infant bodies beginning at the navel, each has all its parts below to the very toes proper to itself, and not only distinct all along, but separate. Upwards beneath the breasts these bodies part again, and then all is as below, distinct and separate. When laid supine, they seem to have but one body where joined; but when turned there is a deep furrow between them, with each a distinct back bone, &c. Each has nipples in their proper place respecting the several bodies, but one of each is seen before, the other behind with respect to the whole; they do not wake and sleep together: they suck and cry heartily, exonerate apart freely, and are likely to live, if the multitudes that come to see them, sometimes 500 in a day, do not occasion the shortening of their lives. They are christened by the names of Aquila and Priscilla, though both females; they were born by an easy labour to the mother, who had had five children before. The midwife said, the after-burden, though but one, was triple in size to what is usual; that the navel string was very large. It is remembered, that about 40 years since such a birth occurred in Wales, and that the children lived so long as to be able to talk to each other, and that in tears, when the one thought what the other should do when either should happen to die; but that both died together.

An Hypothesis of the Structure of a Muscle, and the Reason of its Contraction; read in the Surgeons Theatre, Anno 1674-5. *By Dr. C. Philos. Collect.* N° 2, p. 22.

Having shown and proved at large, that the motion of a muscle is performed only by the carnous fibres, and that each distinct carnous fibre had a power of contracting itself; I offered an hypothesis of the structure of one carnous fibre,

* There is little doubt that infection is received in this manner in numerous instances. It is however not the only way in which it is received, and therefore the ejection of the spittle will not alone be always a sufficient security against infection. On a too near approach to the sick, the contagious particles may, in the act of inhalation, enter the nostrils, as well as the mouth, and be detained in the mucus secreted from the Schneiderian membrane; hence the propriety of cleansing the nose as well as the mouth. Moreover contagion is absorbed by the pores of the skin; hence the necessity of washing the hands and face after visiting the sick.

since the force of the whole muscle is but an aggregate of the contractions of each particular fibre: and this I did in farther explication of an hypothesis, printed in the year 1664, grounded on an experiment made for that purpose before the Royal Society a little before that time. In that printed paper it was said, a muscle in contraction swelled like a bladder blown up, which would raise a great weight according to the experiment shown to the Society: an objection arose, that we did not see any such conspicuous swelling in the belly of a muscle; to answer which, and explain the hypothesis more clearly, I made this farther addition.

1. I supposed each distinct carnous fibre as AEF (fig. 2, pl. 14) to consist of an infinite number of very small globules or little bladders, which for explanation sake I here express as so many little triangles, e. g. four in this fibre, ALB, BMC, CND, DOE, all opening into each other at the points A, B, C, D, E.

2. I not only supposed, but endeavoured to prove, that from the artery of each particular muscle, the nourishing juice of the muscle was thrown out and extravasated to run at large among the carnous fibres, and insinuating itself by the constant pulse of the heart was driven on, and after mixing with another liquor it meets with between the fibres in the muscles, came to be strained through the coat of each globule or little bladder into the cavity of it: and likewise, that from each ramification of the nerve within the muscle, that second sort of matter much more fluid and active than the former is extravasated, and these mixed together as beforesaid, enter into each little bladder, and by these constant agitations, ebullition, or effervescence, which with the natural heat that is partly the cause, and partly the constant assister of this motion, and makes that which we call the very life of every part, as long as the animal lives, keeps these globules or small vesicles always distended. How it enters in and sends out its effete particles again into the mass of blood to be discharged by transpiration, and every moment takes in fresh, I endeavoured in those lectures to show at large.

3. By the aforesaid agitation each little bladder ALB, BMC, &c. is always as beforesaid distended more or less, as long as the animal has life or warmth.

4. The force or energy of which distention to contract the fibre AF, either into itself from both extremes A and F, or if A be fixed, from F, and so to raise with it any weight, as that at F to E, or any limb as fig. bicip. the arm and hand F, was before demonstrated in the printed paper, and in an experiment entered in the journal of the Society.

5. By these 4 small bladders, I answer the fore-mentioned objection, which was made against the principle laid down in the printed paper, and show that the fibre AF, and so of the whole muscle, does not distend itself as one single

great bladder AKE, from C to K, in raising the weight from F to E, but those innumerable small bladders are each distended but a very little imperceptible space, and yet produce the same effect to raise a weight from F to E. As if we suppose 4 little bladders only for example sake, each of them being distended from Q to L, shall all together raise the weight F to E, as well as a single large one swelling from C to K: the demonstration is manifest from the view of the figure, where the triangles AKE, ALB, BMC, &c., being all similar, the several small sides AL, LB, BM, &c. all taken together, will be equal AK, KE together, and will produce on the whole the same contraction, from F to E, as the two AK, KE, though by only a 4th part of the swelling out, at L, M, N, O, as of that at K. Now if instead of 4 little bladders in a fibre, we substitute 4000, that fibre shall raise the weight through the space EF with the 4000th part of the swelling, and so of the 400,000th part, &c.

6. Hence was at large shown: 1. The manner of the blood's circulation in each particular muscle, what it contributed to this motion. 2. What the perspiration out of each muscle did towards it. 3. That there was some swelling in the contraction of each muscle. 4. How every muscle in contraction grows more hard, dense, and compact. 5. The necessary cause of every muscle having its antagonist. 6. Why on a paralysis or wound of one side, the antagonist presently contracts. 7. Why upon fractures it runs up. 8. Why in animals killed, while the flesh is warm, there appear so many jerks and contractions of muscles. 9. Why a muscle cut in the middle runs up to each end, &c. These and several other particulars I endeavoured to make out at large in those lectures; yet only in the way of an hypothesis, not as if I did presume to believe I had found out the true secret of animal motion, when I am almost persuaded, no man ever did or will be able to explain either this or any other phenomenon in nature's true way and method.

A Specimen of a New Al-mon-ac* *for Ever: or, A Rectified Account of Time, by a Luni-Solar Year. By Dr. Robert Wood. Philos. Collect.* N° 2, p. 26.

The garter, fig. 3 and 4, pl. 14, shows the number of days, 30, 29, 30, 29, &c. which each of the 12 months of the year is to contain: and in the buckle is discovered, through a hole, the days of a 13th month, 31, 30, 31, 30, &c. to be added to the years 2, 5, 7, 10, 13, 15, 18, 21, 24, 26, 29, 32, 34, 37, in all to 14 of 38, as in a cycle behind; which being supposed move-

* *Al-mon-ac.*—Our ancestors used to carve the courses of the moon of the whole year upon a square stick, which they called an *Al-mon-aght,* that is, *Al-moon-heed.* Verstegan, p. 58. The Dutch *Al-maen-acht* imports the same.

able the 38th part every year backwards, or contrary to the months in the garter, its revolution is completed in 38 years: and the 38th year current, or the year of the period or cycle, is shown through another hole. The new year and account may best begin with the vernal equinox; or rather with the day of the new moon happening near midnight about that time, as March 10, 1680. Once every century, or to one year in 100, add one day, viz. to the first 50th year, or middle of the century, or rather to the first 70th year, because the first new moon, March 10, 1680, happens a little after midnight, and after the vernal equinox, and then to every 100th year following; which additional day being omitted every 230th or 240th century, if any one shall require such great exactness, will balance the account for ever.

The Egyptian hieroglyphic of the year, was a serpent curved into a circle or ring, with the tail in its mouth; but the emblematical garter will be much more proper for England, &c. and is so exactly fitted to the moon's motion, that one day will not be lost or gained in millions of years.

This *Al-mon-ac* measures time principally by the moon, but with a great and near respect to the annual motion of the sun or earth. The unit or least measure is a day; and the garter or luni-solar year will be at a medium, within about a week, or a 50th part of the true solar year; that is so near, that the difference will not be discerned by ordinary popular observation, and therefore must needs answer the ends of husbandry, and other civil affairs, well enough and come often near, and be sometimes very near; and at certain periods they balance each other, and have a kind of coincidence, or agreement, much better then the sun's course has with the Julian account.

The ancients generally computed their time by the moon, or by the lunar, or luni-solar year, as some, particularly the eastern people do still; which year was made up of moons, or real months, to the number of 12, for the most part, and some years 13. This appears from their calendars, &c. It is plain, from 1 Sam. c. xx. v. 5, 18, 24, compared with v. 27, 34, that the days of the Jewish month were the same with those of the moon: and the Greeks, particularly the Athenians, according to the institution of their wise legislator Solon, thus reckoned their time; and so did the Romans also, till Julius Cæsar altered rather than mended the year.

Among the Greeks there lived, in 3 not far distant ages, 3 famous astronomers in their several times, Meton, Calippus, and Hipparchus; each of whom still farther improved astronomy, and rectified the accounts of time. Hipparchus the last of them, flourished about 90 or 100 years before Julius Cæsar altered the year; yet Cæsar, or his astronomer Sosigenes followed Calippus in framing the Julian year: for Calippus's period of 76 years consisted of 27,759 days, and so do 76 Julian years. About 3 centuries and a half after Cæsar, the council of Nice first, and about 2 centuries after that, Dionysius Exiguus again introduced the decennoval cycle, called the golden number, for the celebration of Easter; following Meton, the first and least exact of the 3; or rather following both Meton and Cæsar; two masters who were neither of them the best, and who would after some time disagree and remove farther and farther from each other; though both Cæsar and the church might have had much better pattern in Hipparchus: which mistakes of theirs have occasioned those anticipations and differences that have embarrassed the accounts of time. And, though many have proposed laudable methods of rectifying these accounts, yet still building on the old foundations, which were infirm, Pope Gregory XIII, in his Reformation of the Calendar, about 100 years since, was obliged to wave the golden number; and yet he has only palliated the disease; so hard it is to cure an error in the first concoction.

The council of Nice appointed Easter to be kept the first Sunday after the first full moon after the 21st of March; because the vernal equinox, which was on the 25th in our Saviour's time, was then come to be on the said 21st day; and therefore the Gregorian reformation has reduced the vernal equinox back to that day, or brought that day to it again, by losing 10 days, and must omit another day within 20 years: but in this garter-year, Easter will always be the first Sunday after the first full moon of the new year, that is, after the vernal equinox, according to the true intent and meaning of the Nicene council, or as it was in the primitive times; and be but a week moveable, or a month less moveable than now, either after the old or new stile: which might perhaps have prevented the difference, or served as an expedient for reconciling the western and eastern churches.

The 3 cycles of Meton, Calippus, and Hipparchus, were all of them too large for the sun, and primarily intended for the moon, or for a luni-solar year; however that of Hipparchus was nearest to both, and very near the moon's period.

Perhaps at first view, my 38 years period may be considered only as a double Metonic, or semi-Calippic one, &c.; but on farther consideration, it will be found otherwise; for the periods of Meton and Calippus, as also that of Hipparchus, were all of them too great, not only for the circuit of the sun, but for that of the moon also; whereas mine, on the other hand, is too little for either, and requires one additional day almost every 100 years:

Meton's 19 years cycle had 6940 days, its double is 13880
Calippus's period, or 76 years, had 27759 days, its half is . . . . 13879$\frac{1}{2}$

Hipparchus's period of 304 years, had 111035 days, its 8th part is. . $13879\frac{3}{8}$
Whereas my 38 years period has but just days 13879

I have chosen rather to take the revolution of the moon than that of the sun, for my chief and primary measure of time; because, first, the lunar phases, and consequently months, are more easily discerned than the sun's annual period, even by the meanest capacity: next, the observations on the moon have been more numerous, more exact, and of far greater antiquity; astronomers having also had the help of eclipses of very distant ages for their guide, in finding out very nearly the true measure of the moon's mean motion: lastly, keeping an account by the moon, we may reckon by the sun also, that is by months and years too; whereas, on the contrary, reckoning by a solar year, months are only empty names, and in measuring of times and seasons the moon is rendered of no use.

A New Lamp. By Robert Boyle, Esq. Philos. Collect. N° 2, p. 33.

In fig. 5, pl. 14, ABCD is a vessel of latten, well soldered. BC, EF, are two bottoms soldered to the vessel. FG is a pipe soldered to the bottoms aforesaid, the aperture being in the great cavity FA. H is a hole in the pipe FG, opening between the two bottoms BC, EF. I is another hole to which is soldered a pipe IG bent upwards at G. PP is a small vessel fit to receive the wick of the lamp. LM is a slender pipe open at both ends, and soldered to the cover AD at L, and to the bottom EF at M; so that by that pipe the external air may communicate between the two bottoms, without penetrating into the cavity AF. N is a short pipe soldered to a hole in the cover AD, so that thereby oil may be poured into the cavity AF; and stopped afterwards close with cork.

For filling up this machine, stop the aperture G of the pipe IG with a long pin fitted for that purpose, as also the upper end of the pipe LM; then pour in the oil by the aperture N; which done, the aperture N is to be shut up exactly, and both the others, G and L to be opened. Then the oil will run through the pipe IG, and fill the vessel P, till its surface be on the same level with the hole H.

Now it is easy to see that this lamp is free from all the inconveniencies that Cardan's lamp is subject to. For, first, the air does not get into it by starts as it does in Cardan's lamp; but when the oil in PP is wasted, and comes to have its surface lower than the hole H, the oil from the cavity AF runs gently into PP, because its place in the cavity AF is easily supplied by the external air, which gets up into the said cavity AF, through the pipe LM and the hole H. Secondly, when the air contained in the cavity AF comes to be rarefied by heat, it drives out much oil, and so may choak up Cardan's lamp; but in this the oil, being so driven out, gets into the space between the two bottoms, as well as into the vessel PP: now the said space, by reason of its size, receiving 20 or 30 times more oil than the vessel PP, it follows that the surface of the oi therein rises 20 or 30 times less than if all the oil had been driven into the said vessel. Therefore, when we fill the lamp, we must take care that the pipe L be well closed, so that the air between the two bottoms, finding no vent, may keep the oil from filling that space, which by that means, when the hole L is open, will be fit to receive the oil driven by the rarefaction of the air in the cavity AF. Thirdly, the oil being always kept nearly at the same distance from the flame, the wick will not be quickly consumed. Fourthly, you have the conveniency of putting new oil into the lamp without moving or extinguishing it, only closing up G and L, and pouring the oil through N, as abovesaid.

Account of a Book, Jo. Alphons. Borelli de Motu Animalium, published since his Death. Philos. Collect.* N° 2, p. 35.

The work itself is divided into two parts.—The 1st of which discourses largely of the conspicuous or visible motions of animals, namely, of the bending, extension, convolution, and progression, of the external parts and joints; and, in fine, of going, flying, swimming, and the like. The 2d, of the causes of the motion of the muscles, and of the internal motions, v. g. of the humours which are made through the vessels and viscera of animals: demonstrating by what force, and with what mechanic organs, the parts of animals are moved; afterwards, the manner of the muscles acting; then of the motive faculty diffused through the nerves, by which the muscles are actuated; then of the internal motions, which do not depend on the command of the will, as the pulsation or beating of the heart, and the circulation of the blood; of the use of respiration, and of the modes and organs by which it is performed; of the spirits or nervous juices which give motion and sense, and serve also for the nourishment of the parts; of their motion, and locomotive action; of the necessity of eating, and the cause of concoction and digestion of the food; of the depuration of the chyle, and of the manner by which nutrition is performed, and the excrements cast out, by the pores, glandules, and kidneys; of the circulation of the bile or gall in the abdomen; of a certain circulation seminis genitalis; of sleeping and waking; and last of all, of some internal motions that are disturbed and morbid, namely by convulsion, and from lassitudes and fevers.

The whole work, in 2 vols. 4to. is not yet published, only some parts of what is printed are come to hand, in which, among other things, are contained,

* He died Christmas last 1679, at Rome. See account of his life, vol. i. p. 224.

1. A way how a man may swim under water like a fish, as in fig. 6, pl. 14. He breathes by the help of a bag about his head, containing a quantity of air, which also keeps his head buoyant: through this he sees before him by the help of a flat glass, fixed in it just before his face. He moves himself by ducksfoot pattens, applied to the soles of his feet. He buoys up, or sinks himself in the water, by the help of a vacuum pump, wherein, by the help of a rack and winch to be turned with his hand, he can with ease make a greater or less vacuity, and consequently make himself lighter or heavier in the water, &c.—2. Another way of swimming under water, and breathing by the help of a leathern pipe, kept open by spiral wires, and extending from the swimmer's head to the top of the water; a description of which you have in fig. 7.—3. A way to make a submarine vessel, by which several persons may pass together from place to place under water, accommodated with ways to row and move it to and fro, and to make it rise and sink in the water, &c. It is supposed it may be much like that which Mersenne long since published from the shape and make of it, fig. 8.—4. A net-like form of the carnous fibres of a muscle; by which he supposes the contraction or shortening of those fibres to be made, as may be conceived from fig. 1, pl. 15, where by pulling the muscles abroad sidewise, they are contracted lengthwise.

Phocæna; or, The Anatomy of a Porpus, dissected at Gresham College; with a preliminary Discourse concerning Anatomy, and a Natural History of Animals; by Edward Tyson, M. D. Lond. Philos. Collect. N° 2, p. 37.

In writing a natural history of animals, the author proposes, that there may be a threefold account given; a physiological, an anatomical, and a medical; and these taken rather from autopsy than books, and of our own country, rather than the whole world; and to begin from the lowest degree of animation in zoophytes, or plant-animals, so ascending by nature's clue, to run through all the several tribes of animals, observing the harmony she keeps, or the liberty she takes in the various formation of them.

In the mean time he wishes that we had a good account of some of the most anomalous and heteroclite sorts of animals, or of those whose species are most different; for one of these may serve in a great measure for those of the whole genus, as the anatomy of a porpus for the cetaceous kind. In the anatomy of the porpus he observes, that it is of great affinity with dolphins; that it is viviparous, lactiferous, and stands in need of respiration, like land quadrupeds. That internally it so much resembles them, that there is very little difference in the make and formation of the viscera of both; but of several parts here the structure is most remarkable; as of the fat or blubber, which is only oil contained in abundance of small cells or bladders. That it has 3 stomachs, 300 kidneys, each having its particular emulgents, pelvis, and ureter; 2 glandulæ renales; 10 spleens; a urinary bladder; an omentum or call, made up of a curious contexture or network of vessels and fibres, though Bartholine and Jo. Dan. Major deny it to have any; a pancreas and ductus pancreaticus; a large liver, but no vesicula fellea, or gall bag. That the organs of generation are very curious, having vasa preparantia, ovaria, tubi fallopiani, uterus, pudendum, clitoris, et ubera. That the thorax is divided from the abdomen by a large muscular diaphragm, and in it are the lungs with two large lobes; the heart, which has two auricles, and two ventricles; and on the spine an admirable contexture of sanguinary vessels, variously contected and winding, emerging from the medulla spinalis. That above the sternum was the thymus, the glandulæ maxillares, thyroideæ. In the mouth he describes the tongue, the teeth, the larynx, which is very remarkable, the fistula or spiraculum, through which it spouts out water like whales; the os hyoidea. He takes notice also, that this fish had eyelids, and in them he observed Steno's ductus. That the eyes had all the tunicles, humours, and muscles, as a man's, as also the musculus septimus, or suspensorius of brutes. That the organ of the ear was different from any yet described, and the porus auditorius very small. That it had a tympanum, but no incus, stapes, or malleolus. That the brain was very large, and so different from those of other fishes, that almost in all things it resembled Dr. Willis's figures, only it wanted the par olfactorium, or the first pair of nerves. In the skeleton the most remarkable thing, he observes, is the fore fin, which much resembles an arm, having a scapula, an os humeri or brachii, a radius and ulna, a carpus, metacarpus, and five digiti. And though but from a single subject, yet the author has been so exact and curious in describing each part, that he has supplied several defects, and corrected many errors in the accounts of the anatomy of this fish given us formerly by Rondeletius, Bartholine, Jo. Dan. Major, and some others, and illustrated the whole with various figures.

The first Vol. of the English Atlas, containing a Description of the Places next the North Pole, as also of Muscovy, Poland, Sweden, Denmark, and their several Dependencies. Oxford, 1680. *Philos. Collect.* N° 2, p. 39.

An Account of an Observation (consonant with that of Dr. Tyson) made by Dr. Samson, on dissecting a morbid Body. Philos. Collect. N° 2, p. 49.

In a woman lately dissected, who was the day before her death with great difficulty delivered of a dead child, there were found two large globose tumors, depending on the left testicle, and may rather be called preternaturally-grown eggs, or parts of the extended ovarium: both of them lay in the pelvis, under the womb, and so hindered the egress of the fœtus, which was large and well grown. They were covered with a thick membrane, which had its veins and arteries as conspicuous as those are in the urinary bladder. That nearest to the testicle was the least, of the size of a cocoa-nut, which had in it a fatty substance, not fluid, of the colour of the yolk of an egg, and in the midst of it a lock of hair, which when it was freed from the grease, appeared of a flaxen colour: the fat itself crackled in the fire, melted and took flame like lard, and in a spoon over a candle would boil and smoke, excepting some small grumous parts. In the midst of the membrane was a hard and knotty substance, in which lay a small bone of a strange shape, with a periosteum on it, which was hard to separate from it. The bone is hard, white, and somewhat larger than the largest of the bones in the meatus auditorius.

The other tumor was thrice as large as the former, and about two inches distant from it, yet connected to it by a strong membrane of the extended testicle. Opening it, there sprung out a more white and liquid sort of grease; but in the middle was as thick as the former, and of the colour and constitution of live honey; for which cause it may be called a meliceris; though the inflammability, both of this and the other, makes them both steatomata. In the midst of this lay enveloped a large lock or two of hair, variously entangled like those the countrymen call elfs-locks, which are a species of the plica polonica: the colour was of a blackish brown, and the quantity four times as much as the former. Some part of this hair was long, and evidently grew out of the inward parts of the membrane, in which it was radicated, and from whence it was plucked. This fat was more inflammable than the other, neither did it crackle in burning as the former, and left fewer spots in the spoon. In the duplicatures of this membrane also was a lump which contained another misshapen bone, very hard and hollow, covered with a skin like a periosteum without, and the dura meninx within: so that it is hard to say, whether nature was forming a tooth with part of the jaw, or the whole cranium.

On Infinitely-infinite Fractions. By Dr. Rob. Wood, Master of the Mathematical School, lately founded by his sacred Majesty, in Christ-Church Hospital, for the Improvement of Navigation. Philos. Collect. N° 3, p. 45.

Infinitely-infinite fractions, or all the powers of all the fractions, whose numerator is 1, are all of them together equal to an unit, or 1. And so of others, as here below.

$$\begin{array}{ccccccccccc} & & \text{A} & & \text{R} & & \multicolumn{5}{c}{\overbrace{\qquad\qquad\qquad\qquad\qquad}^{\text{p}}} \\ & & & & & & \text{q} & & \text{c} & & \text{qq} \\ 1 & = & \frac{1}{1} & = & \frac{1}{2} & + & \frac{1}{4} & + & \frac{1}{8} & + & \frac{1}{16}\ \&c. \\ & & \frac{1}{2} & = & \frac{1}{3} & + & \frac{1}{9} & + & \frac{1}{27} & + & \frac{1}{81}\ \&c. \\ & & \frac{1}{3} & = & \frac{1}{4} & + & \frac{1}{16} & + & \frac{1}{64} & + & \frac{1}{256}\ \&c. \\ & & \frac{1}{4} & = & \frac{1}{5} & + & \frac{1}{25} & + & \frac{1}{125} & + & \frac{1}{625}\ \&c. \\ & & \frac{1}{5} & = & \frac{1}{6} & + & \frac{1}{36} & + & \frac{1}{216} & + & \frac{1}{1296}\ \&c. \\ & & \&c. & & \&c. & & & & & & \end{array}$$

A is a rank or series of absolute numbers, or rather of all the fractions whose numerator is 1; which is supposed to be continued in infinitum downwards. R is another series of all the roots, whose numerator is 1, of all the powers of such fractions; supposed likewise to be continued in infinitum. p, all the respective powers of such fractions, viz. squares, cubes, &c. or so many ranks of geometrical proportionals; supposed to be continued in infinitum, both to the right hand, and also downwards.

Lemma. Each of the said ranks of powers, together with their respective roots, is equal to each of the several numbers under A respectively.

a ——— *c* ——— *e* *g* — *f* ——— *d* ——————— *b*

Demonstration. If from the line ab you take, for instance, $\frac{1}{4}$ part towards a, suppose ac; and also from the other end of the same line ab, you take two such parts, or $\frac{2}{4}$ parts, towards b, suppose bd, viz. a number of parts less by two than the whole line ab was first supposed to be divided into, there will remain the line $cd = ac = \frac{1}{4}$ of ab. Then again, if from cd you take $\frac{1}{4}$ part thereof towards a, suppose ce, and from the other end $\frac{2}{4}$ parts of the same cd, suppose df, there will remain only $ef = ce = \frac{1}{4}$ of cd. And if you still go on without ceasing, to take on the side towards a, $\frac{1}{4}$ part of what was taken last before, and twice as much on the other side towards b, there shall be found between the two lines last taken, always remaining $\frac{1}{4}$ part of the line from which they were taken. From which $\frac{1}{4}$ part there may still after the same manner be supposed to be taken two other such lines. But if this be supposed to be done infinite times actually, then there will nothing more remain between, and so the continued division on either side will come exactly to the point g, supposing ag to be $\frac{1}{3}$ of ab and $bg = 2\ ag$. For, because that which was taken away towards b, was always twice as much as that which was taken away towards a, the total sum of all the lines taken away towards b, which altogether make up the line bg, must be twice as much as the line ag, which is the total sum of all the lines taken away towards *a*, viz. $bg = 2\,ag$; and consequently $bg + ag$, or the whole line *ab*, is equal to $3\,ag$: and therefore $ag = \frac{1}{3}$ of *ab*, which was to be demonstrated.

The like construction and demonstration may be used in taking away any other part of any quantity, and the like part again of the first mentioned part, and so in infinitum. The total sum of all the parts so taken, or supposed to be taken, shall be equal to a certain quantity, or part, or fraction, whose denominator shall be less by an unit than the denominator of the first mentioned part; as

$\frac{1}{6} = \frac{1}{7} + \frac{1}{49} + \frac{1}{343}$, &c. $\frac{1}{9} = \frac{1}{10} + \frac{1}{100} + \frac{1}{1000}$, &c.

And so, that which the incomparable Archimedes, in squaring the parabola, has only demonstrated in one particular case, viz. $\frac{1}{3} = \frac{1}{4} + \frac{1}{16} + \frac{1}{64} + \frac{1}{256} + \frac{1}{1024}$, &c. and that with a huge apparatus of preliminary propositions, amounting to a whole book, is here universally demonstrated in all cases, which are infinite, and by a very simple and easy method, in Des Cartes way.

Now if each of the said ranks of powers, together with their respective roots, be equal to the several numbers or fractions under A; as is demonstrated by the lemma; then is A the sum of them all, or equal to them all: that is, $R + p = A = R + 1$: for R is the same with $A - \frac{1}{1}$, or 1, or but $(\frac{1}{0})$ one infinite part larger. Wherefore $p = 1$. Q. E. D. viz. infinitely infinite fractions are equal to a unit, viz. to the least integer root.

Hence it appears; 1. That there is given a progression in infinitum.—2. That there is a progression not only to one infinite, but to several infinites, or rather to infinite infinites.—3. That this may be performed, viz. this calculation, by a very limited and slight capacity.—4. That this whole progression, or these infinite progressions, may be numbered or summed up.—5. And that into a sum, not only not infinite, but so very small, as to be less than any number.—It appears further, that some infinites are equal, others unequal; and that one infinite is equal to two, or three, or more, either finites or infinites.

For,

1. The infinite powers of the first rank are $= \frac{1}{2} = \frac{1}{1 \times 2}$, and also equal to all the infinitely-infine powers of all the other ranks.

The infinite powers of the second rank are $= \frac{1}{6} = \frac{1}{2 \times 3}$
Those of the third are $= \frac{1}{12} = \frac{1}{3 \times 4}$
Those of the fourth are $= \frac{1}{20} = \frac{1}{4 \times 5}$
Those of the fifth are $= \frac{1}{30} = \frac{1}{5 \times 6}$
&c. in infinitum.

viz. equal to the respective mean proportional numbers between the square numbers respectively, *ex. gr.*

4,	6,	9
9,	12,	16
16,	20,	25
25,	30,	36

&c. in infinitum.

2. The infinite powers of the first two ranks are $= \frac{2}{3}$.
Those of the first three are $= \frac{3}{4}$.
Those of the first four are $= \frac{4}{5}$.
Those of the first five are $= \frac{5}{6}$.
&c. in infinitum.

3. All the powers of all the infinite ranks, except the first, are $= \frac{1}{2}$.
all, except the first two, are $= \frac{1}{3}$.
all, except the first three, are $= \frac{1}{4}$.
all, except the first four, are $= \frac{1}{5}$.
&c. in infinitum.

These later corollaries may all appear by simple addition and subduction, and so may many more.

Experiments made with the Liquid and Solid Phosphorus. Communicated to the Collector, by Dr. Frederick Slare, F. R. S. and one of the College of Physicians. Philos. Collect. N° 3, p. 48.

The experiments that I showed the Royal Society the last summer were, some with a liquid, and some with the solid phosphorus. These two do not materially differ, being made both out of substances taken from the human body. The liquid is a substance mixed with a liquor, which though it burns a person when in a solid mass, will not offend the most tender hand, or even heat, when washed in it. This phosphorus retains not its light very long, if close stopped; yet in one sort I have observed a kind of flashing six or seven times successively, though the glass was close stopped. The other phosphorus, which is solid, differs not, as was said, materially from the fluid, being made for the most part from urine; but I am satisfied that it may be as well made of blood, if it could be as easily obtained as urine in great quantities, since the latter is only the serum of the blood strained through the kidneys. In this preparation, we have not only the common analysis into phlegm, spirits, volatile salts, sulphurs, or oils; but divers other extraordinary appearances before this grand product comes.

The substance of this phosphorus may be made as transparent as any resinous body, and will melt like wax in warm water: and when cold, it is exceedingly tough, and cuts like luna cornea, or rather somewhat harder. When it is all under the water, it ceases to shine; but whenever any part of it chances to emerge, or get up into the air, though the glass be hermetically sealed, or perfectly shut, yet it will shine. I have kept it in a large glass without water for several days, and yet continually shining with little or no diminution of its light or weight.

Of this solid I have had some parcels much more vigorous and inflammable than others. When I made some experiments last summer with this solid phosphorus, every one handled it without any danger: but I have since had some parcels that would scarcely endure the touch of a warm hand, without taking fire and burning. Making some experiments in the company of a very worthy and ingenious gentleman, I laid down a piece of this luminous substance, about two drachms in weight, and it took fire when no candle was in the room; it blazed like a faggot, and burnt the carpet and board it lay upon: this sort is only for the experienced and careful to meddle with. The less vigorous afforded us this experiment. We wrote with a pointed end what words we pleased in the light; then we removed into the dark, and had very radiant and legible characters, looking like words written with a beam of light. If we carry these glorious letters to the fire side, and suffer them there to grow warm, they will presently turn into dark lines, and remain as long as good ink may be thought to do. This light is very diffusive of itself, for I have marked down above a hundred characters with this illustrious pencil, and found not a twentieth part consumed. In like manner I weighed out half a grain, and spread it over my hand at night, which it gilded all over and continued light all the night; for so I found it next morning. As a further proof of its diffusive quality, having weighed out one grain and counterpoised it in good scales, it continued to flame in the open air for seven or eight days, insomuch that shutting my study windows by days, I could always see a beam of fire, and when I looked intent upon it, it sent up a white flame into the ambient air; which a large piece does very remarkably. After all was burnt out, we had no ashes or recrements, save only a little moisture which tasted subacid. Having suffered a larger piece to burn out, I had more moisture, which tasted like a weaker oleum sulphuris per campanam.* Its fumes are sulphureous; and in all its properties it seems mostly referable to sulphurs on account of its inflammability, and because it neither loses nor is dissolved in water.

What medical use may be made of this noble concrete, time may discover. And what service it may be of in helping us to explain other phænomena of nature, I should be glad to know, particularly as to that observation of the learned Dr. Croone, who, on rubbing his body with a fresh and well warmed shift, has made both to shine; and also that of a worthy Bristol gentleman, who with his son told me, that after much walking, both their stockings will frequently shine.†

* Phosphoric acid. (Phosphorous acid of the new nomenclature.)

† An electrical phenomenon.

Some Microscopical Observations made by Mr. Anthony Leuwenhoeck, concerning the Globulous Particles in Liquors, and the Animals in Semine Masculino Insectorum. Philos. Collect. N° 3, p. 51.

I set wine to ferment in a bottle in my study, the lees of which, after it had done fermenting, I found to be, for the most part, made up of globules, concreted of 6 lesser, which were less than those of blood or yest. The same also was observed in other instances with wine, as well as with blood, and beer yest. In fermenting syrups the globules seemed to consist of 3 or 4 only.

The same was observed in water also, viz. the globules chiefly consisting of assemblages of 6 other globules. Among these globules too were seen many animalcules swimming about. In milk, warm from the cow, were found globules of the size only of the 6th part of a blood-globule, and others of 2, 3, 4, or 5, sticking together. Such globules also are found in the air, in the time of mists or fogs.

Having examined the male seed of a red chafer, 2 of which I had coupled together, I found that there were many living animals lying together in a fluid matter, the fore part of their body roundish, but with long tails. I also examined the male seed of May flies, and I saw at first many little worms in it, but none alive; afterwards taking 2 coupled, I found an opening in the back of the female, and several eggs there; and I could plainly see living creatures in the male seed wriggling and bending themselves like worms into 6 or 8 flexures.

I examined grashoppers also, and found worms in the male seed, but not alive, in the month of July; but continuing my observations till the latter end of August, I found them perfectly alive, lying in some places 25 or more together, with their upper parts ranged in order by each other, and their tails spread out wider, and making much motion with them like serpents, though the other parts lay still. Prosecuting my inquiry further into the male seed of flies, I saw a great many small thin creatures or worms. In furthering this inquiry, I observed the stomach of a fly full of a clear substance, mixed with a great number of square figures or bodies, whose angles were all right, some were exact squares, others oblong, and of different size, and so clear and thin, as if one had seen a great quantity of looking-glass plates ground, of several sizes. This issued out of the stomach by a hole I made with a needle's point; I observed also many living worms in a female horse-fly, which were larger and shorter, but their wriggling much quicker, and they lay in a clear liquor within the bowels of the fly.

As I formerly spent much time and pains to observe how the worms of insects were produced out of the eggs, and how they grew and spun themselves a covering like the silkworm, and after several changes, turned into flies of several kinds; so I have now much laboured to trace the original of life in the male seed of insects. And to my great satisfaction I have certainly discovered a great quantity of animals, having the figure of serpents, but longer and thinner in proportion, and shut up in small bladders about the size of a small sand, which I take to be the testicles of those flies. And I am satisfied that flies have 2 of those bladders, though I could find but 1 in some, because I suppose the other might be broken by dissecting. But I am most surprised, that in the seed of so very small a creature as a fly, there should be found living creatures so large in proportion.

I have found such animalcules in creatures of all sizes and species, from a horse to a small horse-fly, and might have seen them possibly in smaller, if their exceeding smallness and transparency did not hinder. I have often found a great company of animals in the male seed of gnats, but smaller than those of flies, and have often seen great quantities of eggs in the female, which make her look larger and clearer.

I have lately examined water, in which beaten pepper was steeped, and found 2 sorts of animals for shape, and each of those sorts to contain greater and smaller kinds; the greater I supposed the elder, the less the younger; I conceive I saw in some of the greater sorts the young ones in their bodies; and when I observed 2 swimming joined together, I conceived they were coupled.

A Way of helping Short-sightedness. By Mr. Rob. Hooke. Philos. Collect. N° 3, p. 59.

Having found by many trials, that some short-sighted persons could find little or no relief by the use of concave glasses, for seeing objects distinct at any distance; I considered whether convex glasses might not be made useful for that purpose. Hereupon I considered the cause of short-sightedness, and finding that any one might be made short-sighted, and to be able to distinguish nothing but what is placed very near his eye, but within certain limits of distance, by looking through a very deep pair of spectacles, such as old men use; I concluded that what glasses should make this man, whilst looking through these spectacles, see things at a greater distance, would also help any other person that should be short-sighted by nature.

Next I considered, that by the help of a convex glass, placed between the object and the eye, the image of the object may be made to appear at any distance from the eye; and consequently all objects may thereby be made to appear in a place not too far off nor too near to, but at a convenient distance from the eye; so that the short-sighted eye shall view the picture of the object in the same manner as if the object itself were in that place. But then because the pictures themselves are inverted, and therefore will be uncouth to one not used to see them in that posture, I considered of these expedients to help that defect also.

First, if it be only for reading of a book or writing, there needs nothing but the inversion of it, and then holding the convex at a due distance; for the picture of the letters will appear erected in the due place, for the eye to see and distinguish them very plainly.

Secondly, for seeing to write, I thought this would be the best expedient; that the short-sighted person should first learn to read with his naked eye upside down, which may be quickly attained.

Thirdly, for distinguishing objects at a distance, I can assert by my own experience, that with a little use of contemplating objects inverted, one shall have as good an idea, and as true a knowledge of all manner of objects, as if they were seen erected in their natural posture. For, in truth, all men have the picture of the object inverted at the bottom of the eye, and not erected, which is the place where they are really seen; and yet we see by use, that we have an idea of it as if it were erected, and by much use of seeing things inverted, the same idea will be formed as by seeing them erected.

Of the best Form of horizontal Sails for a Mill, and of the inclined Sails of Ships. By Mr. Hooke. Philos. Collect. N° 3, p. 61.

The invention of horizontal vanes consists in disposing each of them so, as that they may receive the greatest impression from the wind that is possible in every point of the circumference through which they pass; and the least hindrance to the motion by the stagnant air behind. This is founded on the same principle with that of the sailing of ships, and other vessels on the sea, viz. on disposing and ordering of the vane or sail so, as to stand in the best position possible to move the arms of the mill or the body of the ship. The first principle then common to both is, that the vane or sail be as near as possible a perfect plain and smooth surface, without any bellying or curvity. Secondly, that the air may have as many passages between the parts of the vane or sail as may be, that the moved air may come to it as free as possible without being intercepted by a stagnant air before it, to impede or divert its force. Thirdly, that the plain of the vane or sail be put in the middle inclination, between the way of the wind and the way of the arm, or that of the body of the ship.

This contrivance will not only be useful for all manner of mills, either for grinding, sawing, polishing, bruising, drawing and raising water, blowing bellows, &c. in the same manner as common windmills are now used; but may be also of great use for watermills in rivers, where there can no dam be made, as may also the perpendicular vanes of other mills.

An Occultation of Aldebaran, or the Bull's Eye, by the Moon, Jan. 1, *N. S.* 1681, *observed at Dantzic by M. Hevelius. Philos. Collect.* N° 3, p. 65.

The star's first immersion was at $7^h\ 37^m$, viz. at Mare Syrticum, under the island Cercinna, the altitude of Andromeda's Head being then 50° 32′. The star emerged at $8^h\ 46^m$, viz. at the greater island of the Caspian Sea.

An Eclipse of the Moon, of August the 29*th,* 1681, *N. S. in the Morning, observed at the Royal Observatory of Paris. Philos. Collect.* N° 3, p. 66.

Digits eclipsed,				h.	m.	s.
0°	0′	0″	.. The beginning at	1	58	30
6	0	0	.. At the centre of the moon	2	37	30
10	1	0	.. The greatest obscuration	3	35	45
6	0	0	.. To the centre of the moon	4	29	0
0	0	0	.. The end of the eclipse	5	13	0
			The whole duration	3	14	30

Note, That at $5^h\ 13^m\ 56^s$, the apparent altitude of the upper edge of the sun was 17′ 10″, and that of the moon 1° 11′ 30″.

The same Eclipse of August 19, *in the Morning, O. S.* 1681, *observed at Greenwich by Mr. Flamsteed. Philos. Collect.* N° 3, p. 67.

Beginning of the eclipse at $1^h\ 50^m\ 40^s$ in the morning, corrected time. At $3^h\ 14^m\ 12^s$ the remaining lucid parts were 1092 = 5′ 27″, after which, the moon was no more seen for clouds.

A full Account of divers strange Symptoms which happened to a Patient at Berne. By Sigismund Konig, M.D. Abridged and translated from the Latin. Philos. Collect. N° 3, p. 68.

This communication gives a history of the case of a young woman, Margaretta Lauwer, at that time about 25 years of age, who in the spring of 1678, when she was in her 21st year, *menstruis deficientibus,* was affected with the most excruciating pains, seizing at different times various parts of the body, and accompanied with a sudden eruption of vesicles or bladders, of a hand's breadth, filled with a serous fluid, and producing a most violent burning sensation; so as to be quite intolerable, inducing delirium, unless the contained fluid was speedily let out. When the blisters were thus destroyed in one part, they soon afterwards appeared in another. The girl was removed to the hospital, where she had the assistance of all the physicians thereto belonging. After resisting a course of demulcents, dissolvents, evacuants, &c.; the disorder yielded at last to a mercurial salivation; and the patient was dismissed in March 1679, after 8 months of medical treatment, being directed to drink chalybeated goat's milk.

From this time she continued well until the 3d of January, 1680, when the vesicular eruption re-appeared. On the 5th she was admitted into the hospital a second time; and it was again intended to have recourse to salivation, which had before succeeded so well. In the mean while it was thought proper to employ some preparatory remedies. During the use of which a sudden retropulsion of the acrid humour from the surface to the interior, took place on the 15th, when all the blisters disappeared. Although this was a matter of rejoicing to the patient, she being thereby freed from her excruciating pains, yet was no good augured from it by her physicians, who apprehended the humour might fall upon some important viscus; to prevent which (as much as possible) some discutient and diaphoretic medicines were prescribed. Five days afterwards, viz. on the 20th, these apprehensions were seen to be too well founded; the patient being seized with pains in the loins, bladder, perinœum, and inguina, accompanied with a prostration of strength, loss of appetite, symptoms of inflammation, suppression of urine, a quick and irregular pulse; from all which it was inferred that nephritis had taken place. Accordingly recourse was had to venesection, emulsions, and an emollient and paregoric clyster; which to the astonishment of every person came away by vomiting, in a quarter of an hour afterwards. The clyster being repeated it was again discharged by the mouth, and along with it a number of small stones, to the quantity of half an ounce in weight, but without having any fæces mixed with them. The warm bath was prescribed, blisters were applied to the limbs (with a view to revulsion) and anodynes and discutients to the loins and region of the pubes; venesection was repeated (whereby the fever was abated) and laxative decoctions were given; which last, however, as well as broths and whatever else she swallowed, were vomited up; and along with them a great number of small stones, as hard as flint, together with broken pieces, resembling white marble both in colour and hardness. Recourse was again had to clysters, which, as before, were discharged upwards, and along with them an increased quantity of stones, which in the preceding vomitings were not larger than peas, but now were as large as filberts. Although the difficulty of making water was extremely urgent, yet on introducing the catheter into the bladder not a drop of water came away. The catheter stuck there as though it had been glued in, and when it was stirred it showed that the bladder was full of mucus. We suspected that there were calculi in the kidneys, the bladder, and the mesenteric glands, as we had had proof that they existed in the stomach, and intestinal canal. The belly was distended, but not to a great degree, accompanied with oppression of the præcordia, difficulty of breathing, and a most acute lancinating pain in the region of the right kidney and in the left hypochondrium, and a noise like the clashing of stones one against another was heard when the belly was pressed with the hand, or when the patient vomited, and this noise still continues. But what is the most surprising of all, the girl during the whole of her illness had a plump and rosy appearance.

Our utmost efforts were directed towards putting a stop to the generation of these calculi, and towards dissolving those already formed, by the employment of various volatile, martiated, remedies; but nothing seemed to make any impression except spirit of nitre. At the same time various decoctions were injected into the bladder, to attenuate the before-mentioned mucus, but without effect; though anodynes were added to lessen irritation. At length on the 2d and 12th of February 4 oz. of thick and green urine were drawn off by the catheter. She still continued to vomit 2 or 3 times a-day, whenever a few spoonfuls of broth, barley-water, or any other liquid food, were given, and along with them a quantity of calculi equal to $\frac{1}{2}$ an oz. or 6 drachms weight, until the 14th; from which time until the 16th of June, being an interval of 4 months, she neither eat nor drank; for as soon as a single spoonful of broth was given, it was immediately rejected, and along with it some blood, together with a greater quantity of stones than before; so that in order to prevent these sufferings, we were now obliged to desist altogether from offering her either meat or drink. In this state the patient continued for 4 months, abstaining from liquid or solid food, and taking no other medicine besides a small spoonful of oil of sweet almonds, mixed with spirit of nitre, every 5th or 6th day. This was found to be the best solvent, and was more acceptable to the patient than any other remedy; she took in the whole between 9 and 10 oz. of it during the above-mentioned time. The bowels still remained costive; all the clysters that were administered were still brought away by vomiting, accompanied with stones, varying in colour (being white, red, and grey) as well as in solidity, roughness, and smoothness of surface, and internal structure. Some were covered with blood, others with mucus. The dysury was still urgent; and although the catheter was introduced every 3d day, not more than 2 or 3 oz. of slimy green urine could be drawn off at a time, and that not oftener than every 10th or 12th day;* for the only supply of liquid which the patient had was from the clysters. On the 6th of April, 3 oz. of a clear, thin, blue urine were voided, but it was never afterwards of this colour. On the 17th the urine again appeared greenish, and was loaded with a large quantity of gravel. The pains afterwards returned with excruciating violence (especially in the loins) so as to induce delirium, followed by stupor, laughing, singing, fever, &c. With the intention of procuring evacuations downwards, April 29th, 2 grains of mercurius vitæ were given, and on the 2d of May 3 grains more; but the consequence was 2 vomitings, in which there came away 7 drachms of calculi. To overcome the obstinate constipation, and also with a view of exciting a ptyalism, 4 oz. of quicksilver were given on the 8th of May, and on the 10th 6 ounces more; but it slipped through the intestinal canal without bringing any thing along with it. In this state the patient continued, without taking any sustenance, until the 16th of June, when it was determined to clear the intestines by a solution of sal polychrestum in a large quantity of warm water. Accordingly 6 oz. of this solution were administered every quarter of an hour, until 3 pints were swallowed; the patient's mouth being held close after each exhibition, so as to prevent the liquid from being vomited back. By this method an evacuation downwards was effected, consisting of a large mass of excessively indurated fæces, which distended the extremity of the rectum to such a degree as to excite an apprehension of bursting it. Thus what had been vainly attempted for the space of 4 months by a great variety of medicines, was at last accomplished by a simple solution of polychrest salt in common water.† After this the delirium abated, and the appetite returned.

Up to Nov. there was an evacuation by stool every 5th or 6th day with occasional vomitings, so that calculi were now voided both upwards and downwards, some of them so large as to weigh 2 drachms or more; and it will easily be conceived that these rough and sharp-pointed stones could not come away without being accompanied with blood and the most excruciating pain. On the 5th of Nov. the bowels again became constipated, and all things got worse. The fæces came away by vomiting, and all hope of recovery vanished. In consequence of the suppression of urine, on the 4th of Feb. of the present year

* From the author's words it would appear, that although the catheter was introduced every 3d day, yet an evacuation of urine did not take place each time; but when it did, (i. e. every 10th or 12th day) not more than 2 or 3 oz. came away at such times.

† The author's expression is, by water alone (una aqua omnia fontana solvit); but just before he mentions the addition of polychrest salt; the proportion however of the salt to the water is not stated.

(1681) the catheter was introduced, but no water could then be drawn off; yet shortly after, to the astonishment of all, the patient voided (with the sufferings of a woman in labour) 8 pints of feculent greenish urine, but without any calculus. After this she vomited up (instead of discharging by its natural passage) 3 or 4 oz. of strong smelling urine every 2nd or 3d day, until the 16th of May; since which time, by the help of baths and the copious use of water mixed with spirits of nitre, she this summer is so far recovered as to have a healthy appearance, and be able to walk about, having a tolerable appetite, and making every day from 3 to 5 oz. of clear yellowish water, which deposits some sediment, and is at times slimy and bloody. She has stools but scanty and hard every 4th day, and she now and then vomits up some calculi, but in much smaller quantity than before; on the other hand, sharp-pointed stones are at this time more frequently discharged from the bladder. The belly is still somewhat tumid, with a painful hardness in the left hypochondrium and in the right lumbar region, and when handled produces a sound like that of stones rubbing one against another.

The sudden production of these calculi is, the author remarks, truly surprising. He thinks that if all the calculi which the patient voided at different times had been saved and put together, they would have weighed 5 ℔. He found some of them to crumble into powder on exposure to the air, and on the contrary to become harder when put into spirit of wine, or any other liquor, except an acid. He asks what could be the cause of the vesicular eruption? What the nature of that mucus which was contained in the bladder? How the clysters could pass from one extremity of the intestinal canal to the other (ab ano ad œsophagum) and be brought away by vomiting, while the colon and rectum were obstructed with indurated fæces? What could be the cause of the green and blue colours of the urine? Why, when the catheter was introduced, no urine could be drawn off; and yet soon after by an effort of nature, an amazing quantity was spontaneously evacuated; after which there was again a suppression of it? In what way the urine could get to the stomach and be discharged by vomiting? Lastly, how the patient could exist so long without meat or drink?*

* This is altogether one of the most extraordinary cases of calculous affection upon record. It cannot be doubted that the calculi which were discharged by the mouth and anus were for the most part generated in the alimentary canal; for they appear to have differed in colour and quality from biliary calculi; neither did the seat of the pain correspond with the situation and direction of the gall-duct; nor were there any symptoms of jaundice. While the lithic concretions were forming in the alimentary canal, they were likewise produced in the bladder, and, as the symptoms strongly manifested, in the right kidney also, if not in both kidneys. Their sudden generation in so many

Telluris Theoria Sacra, Authore T. Burnetio. Lond.* 1681, *4to. Philos. Collect.* N° 3, p. 75.

The learned and ingenious author, considering the earth as one of the greater bodies of the world, has endeavoured in this volume to give an account of its production, duration, progress, and changes, from physical causes, or the method of nature in the progress of other beings, and thereby to reconcile what we find in sacred history concerning it with a rational and philosophic theory, which was not done as he conceives before now.

Il Fosforo overo la Pietra Bolognese preparata per rilucere fra l'Ombre, fatica di Marc Antonio Cellio dell' Academia Fisico-mattematica di Roma, 1680. *Philos. Collect.* N° 3, p. 77.

The principal places near Bononia where the stone is to be found are, 1. At Pradalbino. 2. In a small brook near the village Roncania; in which is found great quantity also of a white stone, like sal ammoniac; also another kind of ferruginous stone, which yields a vitriolate and stiptic efflorescence. 3. At Monte Paterno, which is most noted of all for these stones, especially of such as are easiest to be prepared.

It is commonly reported to have been found out by a shoemaker of Bologna, called Vincenzo Casciareto, ingenious, and a lover of chemistry, who trying to get gold out of it, by chance observed this shining quality in its calx, without any other addition of lime, sulphur, talc, antimony, or any other substance.

It has no certain figure, but some are round, others cylindrical, others lenti-

parts of the body, and that not till after the disappearance of the vesicular eruption, is, as indeed Dr. Konig himself has hinted, a most remarkable circumstance; and serves to show how cautious practitioners should be in repelling or suppressing some kinds of cutaneous affections.—It will be seen in N° 181 and 182 of the Transactions, that two of the calculi, which Dr. Konig's patient discharged by the intestinal canal, and which were sent to the Royal Society from Berne, were examined by Dr. Slare, who found them to dissolve, with effervescence, in acids.

* Dr. Tho. Burnet was a learned divine, and author of several ingenious works, besides this on the theory of the earth; which, though containing many uncommon beauties, yet the theory is fanciful, and repugnant to the true principles of philosophy, as was demonstrated by Mr. Keil, and other authors. The second part of this work appeared in 1689; and the whole was afterwards published in English. Dr. Burnet was born at Croft, in Yorkshire, 1635, and educated at Clare-hall, Cambridge. In 1685 he was appointed master of the Charter-house; in which situation he had the merit of opposing King James, who had ordered the governor to admit a papist as a pensioner of the house. In 1692 he published his Archæologiæ Philosophicæ; in which he took some liberties with the book of Genesis; which not only stopped his further promotion in the church, but occasioned his dismission from the office of clerk of the closet. He died at the Charter-house in 1715, at 80 years of age.

cular; these last are often the best, as being more shining and transparent. They are usually as large as an orange; and though Licetus says there never was any greater than that in Aldrovandus's museum, weighing 2½ ℔, yet the author has had them of 5 ℔.*

The colour is various, as ash, sky, rusty, yellow, earthy and white; but the best for this use are sky-coloured and white. When it is prepared, it has in it an efflorescence, in which lies the greatest part of its virtue of receiving light, which it does most from the sun, but less degrees from the day, the moon, or a flame. Though this light in the dark appears like a coal [gleed], it is not sufficient to read by, unless applied close to the word: it retains not its light long, nor its virtue above 5 or 6 years. The best way of seeing it is in a dark room, after the eye has been there kept for some time, and the stone be brought in immediately from being exposed to the sun.

For preparing it, make a cylindrical furnace of iron or copper plates, about 7 inches in diameter, and about 7 inches high. Line the inside of it with strong lute, that the hollow of it may be about 6 inches over, kept from slipping down by the lower edge of the plate turned in. Make at the top 4 notches, 2¼ inches high, and 1½ inch broad. To this make such another cylindrical part of the same diameter, but a little higher, making 2 ash holes, or air holes, opposite to each other at the bottom, large enough to put in one's hand; line this also as the other with good lute, and make a bottom also of lute to reflect the heat. Make a cover in the same manner, and line it with lute; then fit a grate of iron wire, which may sustain the coals, and freely admit the air; place this between the lower and middle parts, and lay upon it pieces of lighted charcoal, and upon those some others, well charred but not lighted. When you have made this bed of charcoal, as high as the notches, having made powder of some other of these stones beat in a mortar, and searced, dip the stones to be calcined in good strong aquavitæ, and roll and cover them with that powder, then lay them close together on the bed of charcoal, and make another bed of the like charcoal over them to the top of the furnace, which then cover with the round close head. Let the coals burn till all are consumed, and the remaining stones are cold; then take them out, and taking off the crust from them, wrap them up in silk, and keep them in a close box till you use them.

If you would cover any figure with it, beat the crust you took off into a fine

* The so called Bolognian stone is a sulphate of barytes. There are generally, however, aggregated with it other earthy substances, besides a portion of iron.

powder, and having first wiped over the figure with the white of an egg, sprinkle upon it this powder, which will shine as the stone.

This sort of furnace is not absolutely necessary, but it is convenient as well in determining the time, as the degree of heat; for if too great, it will destroy the quality; if too little, it will not raise it.

Prattica minerale del Marchese Marco Antonio della Fratta, &c. 4to. Bologna, 1678. N° 4, p. 80.

This ingenious author, having practised in the most famous mines of Europe, in this short treatise teaches how to make pure metals out of the respective ores.

A further Prosecution of Experiments with Phosphorus. By Dr. Frederic Slare, F.R.S. Philos. Collect. N° 4, p. 84.

To try the elasticity of this shining substance, when brought to a flame, I made the following experiment. I conveyed a quantity of it into a small bubble of glass, as large as a nutmeg, but blown very thin in the flame of a lamp, that it might be the more sensible to the elasticity of the substance expanding into flame. Then I hermetically sealed up the end of the stem of this glass-bubble, so that no air, nor any thing else, could escape, without breaking it. Then I approached the sealed glass to the warmth of the fire, and soon found it kindle into a flame, and to continue to do so by emitting flames, and as it were filling up the whole capacity of the glass for some short time; after which it seemed to be extinguished without breaking the glass, or so much as cracking it.

That I might satisfy myself what this matter so flaming did resolve into, I made the following experiment. I made another small bubble, with a large tube for the neck, and left it open at the top to be as a chimney to the fire below it in the ball; then I made the matter flame as before, by approaching it to the heat, and found my chimney as well as the upper part of the bubble lined with a yellowish sulphur, which though thus sublimed was not yet wholly divested of its shining property; but when a little warmed the whole bubble shone.

To try at what rate it would burn in the open air, I made a piece of it flame merely by approaching it to the warmth of the fire, and found it burn like a piece of nitre, but without any explosion, for it only flamed away pretty quick. I have further observed that it leaves a red tenacious matter on its going out, not unlike red wax, and is so sour that it sets the teeth on edge, and dissolves iron.

Exposing a large piece of it that was carefully weighed, it continued a great while shining before the light was quite extinguished. And examining the quantity of the liquor it resolved into, I was not a little surprized to find it thrice its first weight at least. Some that tasted of it called it spirit of sulphur, others spirit of salt.*

It being now generally agreed, that the fire and flame have their pabulum out of the air, I was willing to try this matter in vacuo. To effect this, I placed a considerable lump of this matter under a glass, which I fixed to an engine for exhausting the air; then presently working the engine, I found it grow lighter, though a charcoal that was well kindled would be quite extinguished at the first exhaustion; and upon the third or fourth draught, which very well exhausted the glass, it much increased its light, and continued so to shine with its increased light for a long time; on re-admitting the air, it returns again to its former dulness. Endeavouring to blow it up to a flame with a pair of bellows, it seemed to be quite extinguished; as it was a good while before any light appeared.—All liquors are apt to extinguish this light, when the matter is plunged into them; nor will it shine or burn though you boil it in the most inflammable liquors, even oil of olives, spirit of turpentine, or spirit of wine.

On Roman Urns, and other Antiquities, near York. By Dr. Lister of York. Philos. Collect. N° 4, p. 87.

Roman urns are found in many places throughout the kingdom; but the different workmanship of these vessels, their composition, and places where they were made, have been, I think, but little noticed. Here then are found at York, in the road or Roman-street without Midselgate, and likewise by the river side, where the brick kilns now are, urns of three different tempers, viz. 1. Of a bluish grey colour, having a great quantity of coarse sand wrought in with the clay. 2. Others of the same colour, having either a very fine sand mixed with it full of mica, or catsilver, or made of clay naturally sandy. 3. Red urns of fine clay, with little or no sand in it. These pots are quite throughout of a red colour like fine bole. Also many of those red pots are elegantly adorned with figures in basso relevo, and usually the workman's name, which I think others have mistaken for that of the persons buried there, on the bottom or cover; as Januarius and such like; and that very name I have seen on several red pots, found both here and at Aldborough. These are glazed inside and outside with a kind of varnish of a bright coral colour.

The composition of the first sort of pots, first gave me occasion to discover the places where they were made: the one about the midway between Wilber-

* Phosphorous acid of the new nomenclature.

fosse and Barnby on the Moor, 6 miles from York, in the sand-hills, or rising grounds, where the warren now is. The other Roman pottery on the sand-hills at Santon, not far from Brig in Lincolnshire. In the first place I have found broken pieces of urns, slag, and cinders. At the latter place there are yet remaining some of the very furnaces, whose ruins I take to be some of those metæ or sandy hillocks. It is remarkable, that both the above-mentioned potteries are within less than a mile of the Roman road, or military high-way.

The Roman urns above described differ in these particulars from what pots are now usually made among us. 1. That they are not at all glazed with lead, which perhaps is a modern invention. 2. That a far greater quantity of sand is used than clay; which thing alone made it worth their while, to bring their clay to the sand-hills. 3. That they were baked either with more leisure after long and thorough drying, or enclosed within certain coffins to defend them from the immoderate contact of the flames: which I am induced to believe, because there seem to be fragments of such things to be found. It is certain the natural colour of the clay is not altered by burning: so that both the degrees of heat and manner of burning might be different. And one of these pot-sherds as I have heard, baked over again in our ovens, will become red. As to the two last kind of urns, it is likely the first of them with their particles of mica in it, were made of a sandy blue clay, which abounds among the western mountains of Yorkshire, and particularly at Carleton, not far from Ickley, a Roman station. The red urns seem to have been their master-piece, wherein they showed the greatest art, and seemed to glory most, and to eternize their names on them. I have seen great varieties of embossed work on them. And lastly, the elegant manner of glazing is far neater indeed, and more durable than our modern way of leading, which is apt to crack, both with wet and heat: and at the fire it is certainly unwholesome, by reason of the fumes lead usually emits, being a quick vaporable metal. This ancient glazing seems to have been done by the brush or dipping; for both inside and outside of the urn are glazed, and that before the baking. And something of the materials of it seems to be remembered by Pliny, lib. 36, c. 19. Fictilia ex bitumine inscripta non delentur. The painting of pots with bitumen is indelible. And again, Tingi solidas ex bitumine statuas, lib. 35, c. 15, the bitumen he says sinks into the very stones and pots, which is something more than glazing.

The great plenty of these urns found in many parts of England, seems to argue them also of English manufacture, but where I cannot guess, unless wrought at the bole mines, of which clay alone they seem to be made in Cleveland, for that the barren tract of land called Blackmoor was well known to the Romans, the jet rings taken up with these urns sufficiently testify. Now jet and bole are no where that I know of to be found with us in England but in that tract; being fossils peculiar to those mountains. Of these jet rings some are plain, and others wrought, but all of them of an extraordinary size, being at least 3 inches diameter, and yet the inward bore is not above an inch and a half, which makes them too little for the wrists of any man, as they are much too large for the fingers; so that probably they were never worn as armilla or annuli, bracelets or rings. And since we are upon the subject of plastics, or the Roman clay-work, we cannot but take notice of the opinion of Cambden; who will have the obelisks at Burrow-Brigs in this county, to be artificial, when in truth they are nothing less, being made of one of the most common sort of stone, viz. of a coarse rag or milstone-grit; but doubtless the size of the stone surprized him, either not thinking them portable, or perhaps not any English rock fit to yield natural stones of that magnitude; but that they are Roman monuments I suppose none doubts, because pitched here by a very remarkable and known Roman station, Isurium.

And then consider what trifles these are, compared with the least obelisks at Rome. And as to the rocks whence they might be hewn, there are many of that stone near the river Nid, and on the forest of Scarsbourg; and a little above Ickley, another Roman station, within 16 miles of Burrow-Bridge, there is one solid bed of this very stone, the perpendicular depth of which only will yield obelisks, at least 30 feet long. And yet at Rudstone, near Burlington in the Yorkshire woulds, full 40 miles wide of these quarries, is an obelisk of the very same stone, shape, and magnitude, of these before-mentioned. And it must be observed that almost all the monuments of the Romans with us were of this sort of stone.

A Letter of Mr. Anthony Leuwenhoeck, dated from Delft, Nov. 4, 1681; *containing an Account of several new Discoveries made by him this last Summer. Philos. Collect.* N° 4, p. 93.

I have formerly written that the skin of the hair of an elk, hart, &c. was made up of globules, and that I had also examined our own hair, which I then judged to be composed of globules. I have since written that our hair has a bark like trees, composed of globules, whose irregularity is caused by being whilst soft squeezed out through the skin. The innermost parts of the hair are made up of threads. But many supposing hair to be hollow, others to have marrow like a bone; made me draw the figure of a hog's bristle, to show that what hollowness does sometimes appear in the hair, which some fancy to be marrow, is nothing but the inward clefts of the hair. For the hair grows not like plants, but by protrusion or squeezing outwards from within the skin; for what was root at first within the skin, becomes the body of the hair when thrust out. When it is first protruded out of the skin it is very soft, but suddenly drying in the air it becomes harder, first without in its bark, and afterwards within, whence the bark being first dried will not shrink as the inward part does, but the inward stringy part shrinking, they necessarily cleave one from another, sometimes with one and sometimes with many clefts, which makes the appearance of dark lines in the hair; these are mistaken for the marrow.

I have several times this summer walked into our meadows, in order to observe the fresh excrements of cows, horses, and other animals, but I could never yet discover any animals in them: but I found multitudes of these small globules swimming in a clear liquor, some of which were not above one sixth part, others not above a 36th part of a blood globule. I have examined also the thick part of the urine of a mare after she had been hard ridden; and found that this thick ash-coloured part of it was caused by a great variety of differing globules, some as large as those of blood, and composed of 6 others: these first were like a very close grown bunch of grapes, whence though they were not perfectly round, yet I call them globules; amongst these I found some $\frac{1}{6}$ of a blood globule, and some $\frac{1}{36}$. In cock's dung squeezed out as soon as killed, I found an exceeding great number of worms like eels, which I found to be the male seed of the cocks. Then I squeezed out the dung of young hens, but I found only one animal, $\frac{1}{6}$ part of a blood globule. It was constituted of a transparent liquor full of globules, $\frac{1}{6}$ of a blood globule, these were compounded of 3, 4, 5, or 6 other globules, also many others of $\frac{1}{36}$ of a blood globule. Young pigeons dung about a month old pressed out, had in one no animals; in the second, whose dung was more transparent, were many, so that in the compass of a sand were 100 of an egg shape, $\frac{1}{6}$ of a blood globule, moving swift, the rest of the matter was like that of hen's dung.

Two Astronomical Observations made by Mr. J. Flamsteed, viz. Occultations of the Bull's Eye, at Greenwich, Anno 1680, *with a* 16-*foot Tube. Sept.* 4, *in the Morning. Translated from the Latin. Philos. Collect.* N° 4, p. 99 and 100.

	h.	m.	s.	
At	3	5	42	the star was immersed at the moon's cusp.
	4	8	20	the moon's diameter was 6595 = 32′ 54″.
	4	14	2	the star emerged.
	3	5	40	the star seemed to adhere to the bright limb of the moon; but

after 2 seconds more nothing of it was to be seen. The place of the immersion was near the most southern of three small spots lying in the middle between Palus Maræotis and Mount Climax.

2. *The other Occultation observed Oct.* 28, *the same Year.*

	h.	m.	s.	
At	7	11	52	the diameter of the moon was .... 6745 = 33′ 39″
	7	19	46	the star dist. from the bright limb 5895 = 29 24.
	8	6	9	the star reached the limb.
	8	6	30	the star disappeared, at the longitude of Palus Miris towards the north, at its northern extremity.
	9	2	5	it emerged from the obscure limb, at the longitude of Insula Major, from its northern limit.

An Account of some of Dr Elsholt's curious and useful Experiments. Communicated in a Letter from Berlin to Mr. T. H. Philos. Collect. N° 4, p. 104.

1. An universal balsamation, or conservation of all things, animal, or vegetable, moist or dry; especially all sorts of waters and syrups, that they may not turn and corrupt; with small or scarcely any cost.—2. The great vine, and wine-cure, consisting in the vines and branches melioration, to make them attract or draw in more of the solar virtue and efficacy; in the fermentation of the wine, supplying what may be defective, &c. &c. &c.—3. To prepare drink of water,* not much unlike wine, and much more wholesome; the spirit whereof shall be as good as the best spirit of wine.—4. To make good vinegar of such a water, more agreeable and healthful for use than the ordinary wine-vinegar.—5. A new invented art of stilling brandy without copper vessels, out of all sorts of grain, wheat, rye, barley, malt, &c. yielding more spirit, and more wholesome than the common way, especially in those places where more beer is drank than wine.

Observations of the Comet of 1680 *and* 1681, *made at the College of Clermont. By P. J. de Fontaney, è S. J. Professor of Mathematics.* Paris, 1681. *Philos. Collect.* N° 4, p. 106.

This ingenious author has in a short discourse comprised all his observations and sentiments concerning the comet which appeared in the latter end of the year 1680, and the beginning of 1681. Concerning the first, which appeared in November, and was seen in England, France, Germany, Italy, and most

* By adding sugar to it, and subjecting the same to fermentation.

parts of Europe, he has nothing of his own observation, having attempted after he heard of it to discover it several mornings, but without success: he therefore proceeds to the observations of the second, which he first mentions to have been observed by himself the 17th of December 1680. On Dec. 16, in the evening, he says, the length of its tail was about 70 degrees, its breadth in the middle about 2, and at the upper end about 3 degrees, full like a rain-bow, reddish, and a little bended towards the west; it passed through the Eagle, the Dart, and the left wing of the Swan, covering the southern star in the neck of the Eagle. The 17th about 5 in the evening he discovered the head, of the size of a star of the first magnitude, among those of Antinous. He now viewed the head with a 12-foot telescope, and saw it differing both from stars and planets, being a dusky light like a cloud, about the size of the moon, and brighter in the middle than the extremes. From which he draws these consequences; first, that its blaze was transparent: Secondly, that the head is not made up of small fixed stars, as Stellæ Nebulosæ are. Thirdly, that he sees no reason to conclude it a planet, because sometimes no nucleus, sometimes many, are seen, which sometimes divide, sometimes unite.

Upon the whole he concludes, that the blaze or tail is made by the rays of the sun passing through the head, which he conceives a clear solution of the constant opposition of the blaze to the sun. 2. That these rays are received and reflected to us by a matter more dense than the æther. 3. That the head of the comet is not a star, they being too dense, but this thin enough to transmit rays of the sun. 4. That yet the matter of the head reflects the rays, and may be of the same nature with the blaze. 5. That the head is more dense than the blaze for the most part, though sometimes otherwise, and that it is not necessary it should be round. 6. That it is not enough that the matter of the head be transparent, but it must give a greater power to the rays trajected to enlighten the parts behind it, since it is not observable that the parts before or round about it are so well enlightened. 7. That there are divers convex parts in the head, which make this conic union of the rays behind them, which make the brighter parts of the blaze. 8. That to form a comet, there is only necessary a union of some denser parts of the other formed into convexities, and these disposed into a convenient order; which he believes may as well be supposed to be naturally effected by these celestial clouds, as the figure and posture of the clouds; which make a parhelion or mock sun, is naturally produced in the atmosphere: so that in fine he concludes, that as a mock sun is nothing but a sublunary meteor, fitly formed and disposed to represent the figure of the sun; so a comet is nothing else but a celestial meteor, whose convex parts of condensed matter reflect the beams of the sun sufficient to make the appearance of the star or head, and transmit others which make the appearance of the tail or blaze.

An Account of a Treatise concerning the late Comet, Published at Turin 1681, *By Donato Rossetti, S. T. D. Canon of Leghorn, and now Reader of Philosophy in the University of Pisa, and Tutor in Mathematics to the Duke of Savoy. Philos. Collect.* N° 4, p. 114.

In this treatise is found little of observation on this comet, but only on account of the ingenious author Dr. Rossetti's own thoughts and theory, concerning the various phænomena of comets in general. He conceives then that all the comets that have ever yet appeared, have been de novo generated, and again dispersed in the elementary regions about the earth. And notwithstanding all that has been alleged by other authors to prove them to be in much higher regions, from their little or no parallax, he pretends to show that their arguments are not sufficient either to prove that comets are above or beyond the moon, or to determine the distances of the planets, by reason that the refractions that the rays from each of them suffer, in passing through the atmosphere, cause so great an irregularity, that tables sufficiently exact cannot be made of the planets themselves, and therefore much less of the comets which have not been yet brought to a theory.

An Explication of the Comet which appeared at the end of 1680, *and in the beginning of* 1681; *with a Table which shows when it began to appear, and when it should cease or disappear, together with the daily Motion of it, as to Longitude and Latitude. Dijon, Jan.* 8, 1681, *on the Observations of Dr. Anthelme, Carthusian of Dijon. Philos. Collect.* N° 4, p. 116.

The contents of this tract were to show a specimen of predicting the future course of the comet, from some of the first observations of it, which has been attempted by several, and upon very different hypotheses. The hypothesis on which the author founded his calculation, is, that comets are bodies as old as the creation, and have motions as regular as the planets; and that the reason why they are but seldom seen is, that they move in vast orbs very eccentrical to the earth, and are only visible in that part of the orb that approaches the earth, which has so little curvature that it may be accounted almost a straight line, as Kepler and some other eminent astronomers have supposed. That the bodies of them are transparent, and transmit the sun's beams, which makes the blaze or tail, which he supposes always opposite to the sun. He conceives them to have no significancy as to mutations on the earth, as of war, sickness, famine, death of great men, or the like.

There is newly published a large and very elaborate Treatise of Dr. Olaus Rudbeck, Professor of Anatomy and History, at Upsal in Sweden; comprised in two Volumes; the one containing the Discourse itself, written both in the Latin and Swedish Tongues; the other, the Maps and Descriptions referred to in the Discourse. Philos. Collect. N° 4, p. 118.

Olai Rudbeckii Atlantica, sive Manheim, &c.

This deservedly famous author has here undertaken a great work, and much to the honour of his country, to set forth the rise and progress of the kingdom of Sweden, from Japhet the first king and possessor thereof in the times nearest the flood, to Charles now at present reigning, of which the chronological tables by him calculated give a short view. His helps herein have been, histories of all sorts, ancient and modern, foreign and domestic; the ancient poets, and principally of Sweden, traditions which have so much greater authority, by the language having been always the same, and the land never conquered by invasion; runic inscriptions, and monuments of greater age, and in greater quantities than are to be found any where else.

An Account of some considerable Observations made at Ballasore in India, serving to find the Longitude of that Place, and rectifying very great Errors in some famous Modern Geographers. Communicated by Mr. Edmund Halley, F.R.S. Philos. Collect. N° 5, p. 124.

Anno 1680, Oct. 28, old stile, the moon applied to the Bull's eye; which star was observed to be eclipsed at Greenwich by Mr. Flamsteed, and at London in Basinghall-street, by Mr. Haines, and myself; the Greenwich observation is in the last of these tracts: at London we noted the immersion at 8h. 6m. 00s. and that the star was newly emerged at 9h. 2m. 52s.; the just consent of our two observations leave no room to suspect their nearness to truth. This appulse I procured to be observed in India by Mr. Benjamin Harry, master of the ship Berkly Castle, and a very able artist, who upon his return gave me this and some other very accurate observations. Riding at anchor in Ballasore road, in the latitude of 21° 20′ north, and about 20 miles E.S.E. from the town, he observed that the star was not eclipsed, but the moon passed to the northward about 24 or 25 minutes, and by his pendulum watch, rectified by altitudes and the rising and setting of the sun, he noted that precisely at 16h. 00m. the Bull's Eye was in equal altitude with the moon's centre, that at 16h. 30m. the star was in equal altitude with the lower limb of the moon, and at 17h. 12m. the occidental limb of the moon was in a right line with the Bull's Eye and

Capella. Now taking these observations under the examination of a calculus, I find that at 8h. 6m. or the immersion at London, the true place of the moon correct by parallax was Gemini 4° 32′ 24″; but at 16h. 00m. at Ballasore road, the true place of the moon was Gemini 5° 54′, that is 1° 21′ 36″ more than at 8h. 6m. at London: now according to the moon's velocity at that time, she passed an arch of 1° 21′ 36″ in 2h. 8m. 40s. of time, so then at 10h. 14m. 40s. at London, the moon was in the same place as at 16h. 00m. at Ballasore road; whence the difference of longitude will be 5h. 45m. 20s. or 86° 20′, Ballasore being so much to the eastward of London.

The same person, in the same ship and place, observed another application of the moon to the same star, on the 23d of December following, in the morning. This was an occultation to most parts of Europe, and was observed at Dantzic by Mr. Hevelius, and at Avignon by Mr. Gallet: the former makes the correct time of the immersion, December 22d, 7h. 46m. 12s. the emersion at 8h. 54m. The latter, the immersion at 6h. 18m. 22s. the emersion at 7h. 19m. 46s. This, as the other, was no eclipse in India, the moon passing to the northward, and by the observation of Mr. Harry, the Bull's Eye and the moon's under-limb were in equal altitude when they were both 13° 45′ high to the west, which gives the time 14h. 49m.; and when the south horn of Taurus was 23° 30′ high, which makes the time 15h. 13m. the western limb of the moon was in a line with Capella and the Bull's Eye. Now by calculation I find, that at 14h. 49m. at Ballasore the moon's true place was Gemini 6° 30′ 30″, and at 7h. 46m. 12s. at Dantzic, the true place was Gemini 4° 55′ 11″, that is 1° 35′ 20″ short of the place deduced from the observation at Ballasore road, which make in time 2h. 32m. 40s: whence it follows, that 10h. 18m. 52s. at Dantzic, makes 14h. 49m. at Ballasore road, and the difference of longitude 4h. 30m. 8s.; and Dantzic being by many and undoubted observations proved to be 1h. 15m. 30s. more easterly than London, Ballasore road will be from London 5h. 45m. 38s. or 86° 24′: and the same difference of meridians will be found 86° 14′ if we make use of the emersion at Dantzic. As for the Avignon observation I leave it for those that please to try it, but I dare engage they shall find nearly the same result. N. B. Avignon is 0h. 19m. 40s. or 4° 50′, to the eastward of London.

For further confirmation hereof, being on shore at Ballasore town, he observed with very great care and exactness, Nov. 18, the same year, that at 9h. 13m. the star which Tycho calls, in Cotyla dextra Aquarii duarum præcedens, (and which was then in Aquarius 28° 52′, and lat. 2° 46′ north), was in a right line with the cusps of the moon, then near the first quarter. The star's place is confirmed by the agreement of Hevelius's observations with those of Tycho, and the theory of the moon cannot be considerably faulty in that part of her orb, it falling precisely on her greatest equation, wherefore by the theory and numbers of Horrox, the true place of the moon at 2h. 53m. at London, is found Aquarius 29° 22′ 10″: but at 9h. 13m. at Ballasore, her place was in Aquarius 29° 41′ 17″; that is 19′ 7″ more than at London, which in time gives 36m.: so that 3h. 29m. at London, was 9h. 13m. at Ballasore, and the difference of longitude 5h. 44m. or 86° 00′ precisely; which the Dutch maps make full out 99°. And the French maps of Sanson, pretending to correct them, have made them 5 degrees worse, and the error 18 degrees completely.

An Idea of Mathematics, written to S. H [Samuel Hartlib.] By Dr. John Pell. Translated from the Latin. Philos. Collect.* N° 5, p. 127.

Concerning what I formerly wrote and explained to you, on the improvement of the mathematical sciences, the amount is chiefly as follows :—When men are without inclination, genius, assistance, and leisure requisite for these studies, it is no wonder if they make but little progress in them. But it seems to me, that by help of the following means, a tolerably good remedy may be provided for this evil. Namely, if,

I. 1. There be composed a Mathematical Monitor, as it might be called, which may contain proper answers to these three questions: 1st, What advantages may be expected from the study of the mathematics? 2d, What helps are

* John Pell, D. D. and F. R. S. an eminent mathematician of the 17th century, was born at Southwake in Sussex, 1610, and educated at Cambridge. In 1628 he drew up the "Description and Use of the Quadrant, written for a friend, in two books," the original MS. of which is extant among his papers in the Royal Society, &c.; the same year he held a correspondence with Mr. Briggs on the subject of logarithms. In 1643 he went to Amsterdam, where he was appointed professor of mathematics; and in 1646 the prince of Orange sent for him, to be professor of philosophy and mathematics at Breda. In 1652 he returned to England, and in 1654 was sent by the protector Cromwell, as agent to the protestant cantons of Switzerland. In his negociations abroad it seems he was not unmindful of the interests of Charles II. by whom he was afterwards promoted to the living of Fobbing, in Essex. Assisted by Sancroft, afterwards archbishop of Canterbury, he brought the reformed calendar into the upper house of convocation. He was declared a fellow of the Royal Society in 1663. Dr. Pell was author of a great many writings in mathematics, on algebra, geometry, astronomy, &c.; he made great alterations and additions to Rhonius's algebra, translated by Mr. Branker; and it seems he introduced into that science the method of registering the steps in the margin, in the reduction of equations; he demonstrated the 2d and 10th books of Euclid, also some parts of Archimedes, and the greatest part of Diophantus's 6 books on arithmetic; he had thoughts also of publishing an edition of Apollonius. But few, however, of Dr. Pell's writings have been published; many of them were left in the library of Lord Brereton, at Brereton in Cheshire, and many others were placed in the house of the Royal Society by Dr. Birch in 1755, who has given a pretty full account of his life, in the History of the Royal Society, vol. 4, p. 444. Dr. Pell died in great distress in 1685, in the 76th year of his age.

now extant for attaining that knowledge? 3d, What order is to be employed in using those helps? Therefore this monitor should contain as follows:

1. A clear and easy discourse on the limits or extent of the mathematical arts, and of the benefits that may accrue, not only to those who study them, but also to the nation to which they belong. 2. A catalogue of mathematicians, and of their works; which may exhibit, 1st, A synopsis of all kinds of mathematical books, both in print and in libraries in MS. proper numbers of reference being affixed to every kind. 2d, A chronological catalogue of all celebrated mathematicians, according to the order of the years in which they flourished, with the dates of their works. 3d, A catalogue of the same works, according to the order of the years in which they were printed in any language; adding also the names of all the mathematical books published that year, in any country or any language: exhibiting the form and the quantity of each volume; placing the year before the title, and after the title the year in which it was last printed, with the number referring to the synopsis given in the first page of the catalogue.

3. An advice to the studious, concerning the best books of every kind; in what order and method to be read, what parts to be selected, what to be omitted, and how to retain every thing in memory.

4. An exhortation to all persons of wealth, leisure, and genius, for the pursuit of these studies: that having regard to the great benefits derived from hence, both to themselves and others; as well as to the pure and sincere pleasure arising from the search of hidden truths, and the solution of difficult problems; that they may seriously apply themselves to the advancement of science; and the rather as more expeditious methods are now discovered than were formerly known, by the saving of much labour, time, and expence. Then an exhortation to all such as are distinguished for setting a right value on these studies, as well for power as wealth, that they may become patrons to such ingenious men, by proposing handsome rewards, to encourage them to complete their useful discoveries. Lastly, to all princes and people, who may procure great benefit to their estates, by giving the utmost encouragement to the cultivators of such arts.

For this end it will be very proper,

II. That a public library be founded, furnished with all the books abovementioned, and with one instrument of every kind yet invented; having also an endowment sufficient for the purchase of all mathematical books as they are annually published, and for the maintenance of a librarian; whose duty it may be, to read over all the books published in his own country, suppressing such as are useless or erroneous, and admonishing authors not to publish only such things as are already known and treated of by others: that he also show his approbation of useful inventions, and recommend the inventors to proper patrons:

that he receive, to enter in his catalogue, and dispose in the proper repositories, one copy of the books so read over, when presented to the library, well bound, at the charge of the author or bookseller: that he give a civil and ready answer to any studious persons, who may consult him about any problem, whether it be already solved or not, that they may neither mispend their time on what has been already done, nor be deterred from prosecuting inventions or new discoveries: that he receive, &c. as above, all manuscripts that may be presented or bequeathed to the library: that he keep up a constant literary correspondence with learned foreigners of this kind, that he may not be ignorant of what books are published in other countries: that he observe, among his countrymen, who are the best cultivators of these arts: that he cultivate an acquaintance with all such artists as excel in constructing mathematical contrivances and instruments of all kinds: that, after a fair trial he give his testimony, both on speculative knowledge and practical dexterity, to practical men of all kinds, that such as have occasion for this kind of men may not be imposed on by ignorant pretenders.

The catalogue will readily inform, among the vast multitude of books in the world, which they are that belong only to this kind of study. The library will exhibit a copy of every such book, and inform us where more copies may be had. It will be also a kind of storehouse, both to natives and foreigners, whence they may readily learn what helps that country can supply to these studies.

Such then, in my opinion, is the readiest way of using the advantages we are already possessed of. If more be wanting, it may be proper, by the aid of skilful artists,

III. That the three following new works be composed and published.

1. Mathematical Pandects, containing, as clearly, methodically, concisely, and ingeniously, as it can be done, whatever may be collected, or deduced by way of corollary, from mathematical books and discoveries made before our time; quoting the most ancient authors in which they are found, and noting in all following authors where they have pilfered from others without acknowledgment; or, which is worse, have arrogated to themselves the inventions of others. By this means, that large library would be contracted into a much narrower compass, to the great saving of labour, time, and expence, for those that come after.

2. A Mathematical Compendium, containing, in a concise manual, all the most useful tables, with precepts to show their application to the solution of problems, either of pure mathematics, or applied to other subjects. Finally, that we may not be always confined to books in this kind of learning, there should be contrived,

3. The Self-sufficient Mathematician, or an instruction to show how any ma-

thematician, not averse to labour, may acquire so much skill, that without the aid of books or instruments he may accomplish the solution of any mathematical problem, and that as easily as another by only turning over books.

This then is that idea of the mathematics which, in my manner, I have long since figured to myself; being always firmly persuaded, that then only we can hope for success in great undertakings, when we have conceived an exact idea of them in our minds, and of the fittest means of putting them in execution. And if we cannot express this idea completely, yet it is something to come as near as may be. I conceive this is so far from being above the human powers, that I think it may be accomplished by the industry of one man alone, who is not prevented by the multitude of other concerns. For it is manifest that the Library and Catalogue may easily be procured, if money be not wanting. And as to the Pandects, if the task of composing them were committed to me, I should impose on myself much severer conditions than those above mentioned. For first I would delineate the infallible process of human reason in the investigation of whatever it proposes to itself; showing how it proceeds from the first rudiments or principles, by an uninterrupted chain, to the highest as well as the lowest application of them. An art which men would not perhaps be long without, if they were but carefully to examine, by what means such ideas have arisen in the minds of certain men whom they admire, and how such apt means have been discovered for attaining that end. How these pandects may be abridged into a manual, such as may be fit for common use, cannot be difficult to understand. But so to fix them in mind, that there shall be no further need of books, which is the thing aimed at in our self-sufficient mathematician, may be thought by most to exceed the power of the human mind. Yet I think men will abate of their doubts on this head, when they carefully consider what arts have been devised for strengthening the imagination, for assisting the memory, and for directing the reasoning faculty, as well as what wonderful effects may be produced by the united and constant exercise of these faculties.

The foregoing Idea considered and objected to, Oct. 1639. *By P. Mersenne.**
Philos. Collect. N° 5, p. 135.

Instead of all that apparatus which the author of the idea proposes, about

* Marin Mersenne, a celebrated philosopher and mathematician of the 17th century, was born 1588, at Oysé in Maine. He was fellow student with Descartes at la Fléche, where they contracted a friendship that lasted through life. Going afterwards to Paris, he studied theology in the Sorbonne, and entered in the order of the Minims. He was teacher of theology in the convent of Nevers, from 1615 to 1619, and became afterwards superior of that convent. Wishing however to be more at liberty for philosophical studies, he resigned all the offices of his order, and travelled into Italy,

collecting the various writings of mathematicians, it seems to me more adviseable to select only the best and most deserving of them. Beginning first with those ancient authors whose works are still in being; as Euclid, Apollonius, Archimedes, Theodosius, Pappus, Ptolemy, with their other fragments and manuscripts which have not yet seen the light; some of which are in the custody of Golius at Leyden, and some are preserved at Rome. Then to these might be added the more modern, as Victa for algebra, Clavius's 5 volumes, Herigon in 5 volumes. In like manner, among the opticians, should be chosen Vitellio, Kepler, Aquilon, De Villes. Among arithmeticians, after Diophantus, the best are Cardan, Tartaglea, and your countryman Nepair. For spherical triangles, and their computation by logarithms, you have Briggs, Gordan, Pitiscus, Snell, and our Morinus. For astronomical matters, after Ptolomy and some Arabians, those should be procured who have composed tables; as Alphonsus, John Regiomontanus, Kepler, and our Duret. And, in short, for fortification and music, eight or ten authors might be selected, who have most excelled in the practice. In like manner, for mechanical subjects, forces of motion, machines, and water-works, about 10 or 12 authors might be abridged, for such as are curious in those matters. And if 12 men of good judgment, having a friendly correspondence with each other, would undertake this business, so that each of them might compose a treatise on a science, in one clear and convenient volume; such things being supplied as are wanting in others, and unnecessary things being omitted; we might doubtless have in 12 volumes, in a short and nervous manner, all that could be desired in this matter. And in my opinion, whatever belongs to mathematics, either pure or mixed, might be comprehended in those 12 volumes. So likewise we might give all the neater parts of philosophy in three books; and all the liberal arts, or the mechanic arts, in three more; so that learning might be obtained at a small expence. And as to mathematical instruments, it would be of little consequence to have an apparatus consisting of all that have hitherto been invented. It might be sufficient to have four or five of the best of their kind, and those of the most convenient and useful sorts.

Germany, and the Netherlands, where he was greatly respected. He afterwards settled at Paris, where he died 1648, at 60 years of age.—Father Mersenne was of a humble disposition, mild, peaceable, virtuous, and of engaging manners. In mathematics and philosophy he presided long as the principal judge and controller. He was author of many excellent works; the principal of which are, 1. Quæstiones celeberrimæ in Genesim, 1623, fol. 2. Traite de l'Harmonie universelle, 1636 and 1637, 2 tom. fol. 3. Cogitata Physico-Mathematica, 1644, &c. 2 vols. 4to.; being an excellent work, consisting of a compendious body of mathematics, ancient and modern, historical and theoretical. 4. La Verité des Sciences, in 12mo. 5. Les Questions inouies, in 4to. 6. L'Optique et la Catoptrique, in Niceron's perspective, &c.

Answer to the foregoing Observations. By Dr. Pell. An. 1639. *Dec.* 2. *Ibid.* p. 137.

If I understand you right, learned Sir, you approve of all I have said, except that you think I require a larger apparatus than is necessary. It is your opinion, that not all the mathematical books and instruments should be collected, which I contend for, but the best of them only be selected. This opinion I should not oppose, if it were generally agreed on, among such a multitude of authors, which are the best, or which should have the preference before all others; and if I had this only in view, that something of labour and expence might be saved to the studious in these arts. But since it was my object to obtain the most perfect foundation for the whole scope of the mathematics, I ought not to delineate any other scheme than that might completely answer such a design. The principal part, I apprehend, of a project must consist in such a universal library as I have described. That being the case, I ought not to despise any attempt, much less to condemn any one unexamined, that may throw in his mite, and endeavour to promote this undertaking. And if I may give my judgment, the most trivial writing, or mathematical instrument, ought to be preserved, one copy at least, even for its errors, in some certain accessible place in every country. For we perceive many things that have been ingeniously invented, in the ruder instruments of former ages, that are now not only worthy of observing, but even of being imitated; as some writers of a lower class may give very good hints, and assist the invention of those of a happier genius; for we can often point at an excellence, which we cannot arrive at ourselves. We perceive many lemmata that have been well demonstrated by this kind of writers; yet, because of some one fundamental fallacy, their whole superstructure has come to the ground. If you think that many ought to be rejected, as well for their trifling and verbosity, as for error and false conclusions; you should consider how different are the notions and taste of mankind, and without estimating the judgment of others by the measure of your own sagacity. For there are some persons who can understand nothing, unless repeated to them a hundred times, and that almost in the same words: so that those tautologies are well adapted to such persons. And because we must always begin with the more known things; and the same things being not equally known to all; we must therefore make very different beginnings. So that you can scarcely find one learner, but he may be assisted by a rude instrument, or an unskilful author: hence then he who undertakes the office of a mathematical monitor, ought not to be unacquainted even with these. So that the complete collection of books, beforementioned, seems to me to be quite necessary.

Now the less I am pleased with these minute mathematicians, the more I should wish for a library of this kind, as being the only method of curing that licentious itch of scribbling. For these prating pretenders, ever trifling in a childish manner, while they would seem to accommodate themselves to the capacity of youth, may see that there are already too many who have compiled rudiments of this kind. And those who fondly aim at advancing the mathematical sciences by an infinity of new discoveries, when they see so many empty paradoxes, which have been condemned and ridiculed by the public, may take warning by the miscarriages of others. But especially the plagiaries, those pests of all true literature, will not have the impudence to vend, as their own, any old books, or any part of them, which perhaps have not been printed more than once. On the other hand, men of candour and ingenuity, capable of delivering their thoughts in a handsome manner, when they see so many have gone before them, on almost every subject, will be cautious not to produce any thing to the public, but what is new and their own invention. Now what has been treated of already may be easily known, either by consulting such an ample library, or, with less trouble, they may be informed by the librarian himself, to whose custody it has been committed. And these are the reasons in general, which I cannot retract, why I prefer such an universal library as is described above.

P. Mersenne's Answer, declaring his Satisfaction. Dec. 10, 1639. *Ibid.* p. 143.

I had no sooner read your letter, learned Sir, than I became wholly yours, and was ready to subscribe to your opinion, which I entirely approve. I was also impelled by an unusual ardor of mind; so that I would recommend this great project of yours to the potentates of the earth, if I had access to them. But where is the king that will make a beginning? For I cannot but call it truly royal work.

M. Descartes' Judgment and Approbation of the same. Feb. 1640. *Ibid.* p. 144.

I inspected the mathematical idea only by the bye; and now only recollect, that I found nothing in it that I should dissent from; and I much approved both the catalogue of the mathematical apparatus as there exhibited, and the self-sufficient mathematician there described, as containing every thing in himself. Nearly in the same sense I am accustomed to distinguish two things in the mathematics, the History and the Science. By the history I mean whatever is already discovered, and is committed to books. And by the science, the skill of resolving all questions, and thence of investigating by our own industry whatever may be discovered in that science by human ingenuity. He who possesses this faculty, has but little need of other assistance, and may therefore be properly called self-sufficient. Now it is much to be wished, that this mathematical history, which lies scattered through many volumes, and is not yet intire and complete, were to be all collected into one book. And for this purpose, there would be no occasion to be at the charge of seeking or purchasing many books. For, since authors transcribe many things from one another, whatever is extant may be somewhere found, in any library that is but moderately furnished. Nor is diligence in collecting all things so necessary, as judgment to reject what is superfluous, and knowledge to supply such things as are not yet discovered. Now if such a book were at hand, from thence any one might learn the whole mathematical history, and a good part of the science also. But if any one should desire to have the whole that belongs to the practice, as instruments, machines, engines, &c. were he a king, and had the wealth of the whole world at command, he could not supply the necessary expences. Neither indeed is there any occasion for it: it is sufficient if he can describe them all, and that he either knows how to make for himself such as are wanted, or can direct artificers to construct them.

An Account of the Dissection of an Ostrich. By Edward Brown, F. R. S. and of the College of Physicians. Philos. Collect.* N° 5, p. 147.

The ostrich is esteemed the largest and tallest of winged or feathered fowl, being sometimes 8 feet high, which bulk if we compare with the tominejo or humming-bird, weighing about 12 grains, we may readily discern within what compass and latitude the creation of birds was ordained.

On the top of his head there is a flat oval place, a nail in length, which is all callous, and without any hair or feathers, like the callous part of his breast, but not so thick, to preserve the brain from the serenes that fall in hot countries, and other injuries of the air, especially in the night, and the more considerably if he sleeps with his head upright, and not under his wing.

The gula is very large as well as long; the os hyoides stretches itself down on each side of the neck, the length of 5 or 6 inches.

Besides the many muscles in the neck for the motion of the numerous vertebræ and the head, there are two most elegant muscles which come from within the thorax, arising within the chest about the second rib, which insert themselves on each side of the aspera arteria; these I may name directores asperæ arteriæ. At the first dividing of the aspera arteria, or its divarication

* Other accounts of the anatomical peculiarities observed in the dissection of the ostrich are given in the 33d, 34th, and 36th vols. of the Philosophical Transactions.

on each side of the lungs there is a ring larger and stronger than any other ring of the windpipe. There are divers glands in the neck near the gula; these an ash-colour; and there are two most beautiful glands sticking to the carotid arteries as they come out of the breast, one on each side, these are bluish. The peritonæum doubles, and encompasses the stomach loosely. He has 7 ribs, and the intercostal muscles are broad, plain, and beautiful. He has no prominent breast-bone, like other fowls, nor a narrow chest like many quadrupeds, but a broad breast, and a large firm sternum, of the shape of a shield, broader than the sternum of a man; and indeed when he puts down his head, and bends his neck round to come in at a door, his breast is so broad, and his tread so different, that it is not at all like the entrance of a fowl, but wonderfully like that of a camel, but with this advantage, that the ostrich bearing his weight upon 2 legs only, his entrance is more bold and graceful. This was a young male ostrich, and had the penis about an inch long, with a small cartilaginous substance in it. The testes lie very high near the kidneys and back-bone, and were very small and slender, of a yellow colour. The ear is round, and the orifice will receive one's finger; the eye is large and bluish, almost as large as a man's.

The rimula of the larynx is long, and the cartilages about it strong, but no epiglottis, or likeness to a human larynx, although they that heard its voice, compare it to the crying or shrieking of a hoarse child, but more mournful and dismal; which confirms the account given by Mr. Sandys in his travels, that there are a great number of ostriches in the desarts, which keep in flocks, and often frighten strangers and passengers with their fearful screeches. The lungs are of fine florid colours, but small in proportion to the vast aspera arteria, they stick close to the back, and are perforated like other birds; but on blowing into the windpipe with a pair of bellows, we could not make them rise or fill. The heart has 2 ventricles about the size of a man's heart, but the right ventricle is much thinner, and the valves are more fleshy. There are 2 stomachs, as in granivorous fowls, a crop, and a gizzard; but the crop or first stomach differs much from that of other fowls, in that it is not placed without the breast as with them, but within the sternum, and is not round, but longer like a bag, and of a vast size, lying lengthwise in the body. But what was most satisfactory of all in this dissection was, the glands found in the coats of this stomach, a row of them on the back part of it, reaching almost from one end to the other, about 1000 of them, about 10 in breadth and 100 in length. These lie between the coats of the stomach, and every particular gland discharges itself by a peculiar orifice, through the inner coat of the stomach into its cavity; we found some of these glands round and globular, some oval, and some more flat, of an irregular figure, those which lie highest are roundest and thickest, those which lie more towards the bottom of the stomach, or where it unites with the gizzard, are more broad and flat. These bring in a juice which helps to digest that various nourishment which this fowl makes use of. The gizzard was very large, the inner coat not adhering so firmly as in other fowls, and was very thick like flannel, and on our first looking into the gizzard from the first stomach, it appeared as a piece of flannel or napkin, which the ostrich had swallowed, and so stuck there. The passage out of the gizzard into the small guts is very strait. The guts are about 20 yards in length; the smaller guts, beginning from the stomach, are 10 yards long; and the larger guts, down from thence to the anus, are near as much. At the beginning of the great guts there are 2 intestina cæca, each of them a yard long, and they have a screw or spiral valve within them, after the manner of the cæcum of a rabbit; this screw in both the intestina winds about 20 turns; the extremity of the cæcum is small, not much differing from the cæcum of a man. The excrement which is thrown out by the guts is of 2 kinds, a white thin sticking excrement, which it mutes like a hawk, and after that another sort of excrement comes, which is very like to that of a sheep, but larger. The mesentery, although it holds together such a number of guts great and small, yet it is not thick, but is only a transparent membrane, as generally in pennates, but it is very large, and in some places above 13 inches deep or broad, measuring from the centre to the guts. The liver has 4 lobes, and is of a colour not much different from that of a man's; we could find no gall-bladder.

There was a gland under the stomach which might seem to be a spleen; but pennata and insecta are said to have no spleens. The pancreas was slender, and above a foot long.

The kidneys are large, and of the length of my hand as they lie both together, they are of the shape of a guitar. The ureters are firm, strong, white, and long. Behind the kidneys lie 2 glands, somewhat oval, of about an inch and a half in length, close to the back-bone.

An Account of several curious Discoveries about the Internal Texture of the Flesh of Muscles, of strange Motions in the Fins, and the Manner of the Production of the Shells of Oysters, &c. By Mr. Leuwenhoeck, F.R.S. Philos. Collect. N° 5, p. 152.

Formerly I have stated, that musculous flesh, viewed with an ordinary microscope, I conceived them to be composed of globules, for so they seemed to appear to me. But with the use of better helps and more diligent inquiry, I now find that they are not globules but rimples. For on examining beef muscles, I found them to be made up of small strings lying close joined together one by another; which were so small that 50 of them, laid one by another, would not make the breadth of the 22d part of an inch, and if supposed a 20th, leaving 2 for the thickness of the membrane that incloses them, there will be found 1000 of such strings lying one by another to make the breadth of an inch, and consequently 1,000,000 of them in a square inch.

In some of my late observations I took notice, that about 100 of these muscular strings lying by each other were wrapped round, and inclosed with a membrane, which made a muscular chord. At another time I observed in the muscles of an ox's tongue 3 such muscular chords, each enwrapped with its distinct membrane, whose ends when cut across would be covered by a sand no larger than the 100th part of an inch; whence we may conceive there may be about 5000 of such muscular chords in a square inch.

I have compared also the magnitude of these muscular strings with the size of the hair of my peruke and of my beard, and I conceived that about 4 of these muscular strings of the diaphragm of an ox, near the ribs, would make but the thickness of a hair of my peruke, and 9 were only sufficient to equal the hair of my beard. Now we must not suppose that these muscular strings are round, but each has its particular form from the pressing of them close together.

In my last observations I examined the muscles of a hare, and therein I saw very clearly and neatly, that divers of these muscular strings ended very sharp in the membranes of the muscle, divers also that ended in the tendons. These observations I made with a very good microscope. These observations caused me again to renew my examinations of the muscular strings of fish, to find out their contexture; hereupon I viewed several parts of cod-fishes, and found that the thickest of these muscular strings were to be found in the parts under the belly, which we here call wangen. Here I found also that the strings of the membranes being separated were furnished with rings or rimples, after the same manner as I had observed the muscular strings of flesh to be. I further also took notice, that when I had cut the strings athwart, I could very clearly discover the ends of a very great number of small filaments, of which I conceived every muscular string of fish was composed.

Now from the knowledge of the number of filaments in the circumference, it will not be difficult to compute how great a number of such filaments there may be in the body of such a muscular string of fish; for, following the rule of Archimedes, we shall find that there may be almost 3200 of such filaments contained in every such string. Now who can in his thoughts comprehend the vast number there must be of such filaments in every muscle? and yet who knows but that still there may be another inferior order of filaments, and that every one of these 3200 filaments, contained in one single muscular string, may be yet farther composed of great numbers of smaller filaments.

After I had with admiration viewed the great number of flesh-muscles of the tail of an ox, I resolved to observe the tail of another creature, viz. a thorn-back, but in cutting through the same tail, I considered the blood which issued out of it, wherein I admired that the parts of the blood, which in mankind are globules, and make the blood red, are here altogether oval parts, which had a small thickness, driving through a crystalline matter; and where these oval parts lay single they had no colour, but when they lay 3 or 4 thick on each other, they had a red colour. This made me observe the blood of a cod and of a salmon, which I found to contain oval figures as the former; and though I endeavoured to observe the same very exactly, I could not find of what parts these ovals were constituted, for some seemed to have inclosed in them in a small space a kind of globules, and a small space from the said globule it was surrounded with a transparent ring, and then again about the same ring a longish shadowing circle, which made up the oval figure.

I took the season when oysters came to us in a short time from England, and observed with admiration, what an extraordinary motion the beard of the oyster made, and although I took some very minute parts of it, many of which would not together make out the size of a sand, yet these parts, so broken, had such a motion as was inconceivable; for I imagined that such a small part represented to me a shrimp with its continual moving pattens, and others like a lobster. And one might have sworn that it was no part of the beard of the oyster, but an animal of itself, notwithstanding the contrary appeared, for such a part of the beard made no progressive motion, and remained in its motion lying in one place so long time, that when my sight failed me with looking, I was forced to leave it; and besides the fibres, which in so small a part seemed pattens or paws, had the same motion with the parts of the whole beard.

I observed the shell of an oyster, and found it all made up of plates laid in great numbers one over another, always larger and larger; so that the increase of the oyster shell is caused by the addition of a new lamen or plate in the shell, which last new made plate exceeds the rest in magnitude. These laminæ seem to be made up of small pipes, which are much interwoven. But that which gave me most satisfaction was, that when each of these laminæ was arrived at its full size, then from the small pipes of the laminæ were put forth minute laminæ, which are not white like the rest, but of a brown colour, and constituted of globules.

An Eclipse of the Moon on the 19th of August, 1681. Observed at Dantzic by M. Hevelius. Philos. Collect. N° 5, p. 160.

Corrected time.		
At 2^h	$46\frac{1}{2}^m$	the first appearance of penumbra.
3	3	beginning of the eclipse.
5	2	the sun's centre rose.

An Account of a very strange and rare Case in Physic, with the Description of a monstrous Animal cast out of the Stomach by Vomit. By Dr. Lister, of York, F. R. S. Philos. Collect. N° 6, p. 164.

I send you (here inclosed) the true and exact shape of a worm, which a man vomited up here the last week. I found it myself in the blood which came up with it, having caused it to be washed for the more careful examination of it, much of the blood being clods of a kind of skinny and fleshy substance, haud aliter, quam in mulierum molis excernendis accidere solet. Of this kind of blood there was about 2 ℔ weight saved in the washing, and this strange animal among it; which was easily discovered, being of a dark green-colour like a horse-leech, and spotted. I could not perceive in it any life or motion; the girl that washed the blood having almost beaten off a fin, and part of one of the forks of the tail, and burst the belly of it; yet it was curiously and regularly shaped in all its members. The spirit of wine in which I put it has changed its colour, but yet it still remains perfect enough to satisfy any curious person.

The patient imagined he drank it the last summer in pond-water. This is certain, he had about his stomach and right side a most exquisite and tormenting pain, for at least 4 months last past, which many times threw him into horrors and chillness, ague-like; and indeed when he vomited this up, he was the sickest man I ever saw not to die; he also voided blood by stool several days, and now I believe he will recover, though his pains are not wholly ceased.

This animal was about 4 inches long, and in the thickest place 3 inches about; it had 3 fins of a side, all near the head, and the upper pair most exactly and elegantly figured, as is described; all these fins were thick and fleshy; but the forked tail was finny and transparent, and to be extended; it was placed horizontally, not as that of most, if not all, small fish, and even newts and tadpoles, in which particular it differs from them all, as well as in the fleshiness of the fins. See fig. 6, pl. 15.

Besides this odd animal, I found the head of another of a different shape, as is expressed, fig. 3, pl. 15, but of a dark green-colour also, as the other; the body of it had not been lost, or this other so ill-treated, if I had expected to have found, what we never looked for.

But what shall we say this monster was? I am apt to think that we often drink and eat what is alive; and it is certain some things will live in our stomachs in despite of concoction, not to instance in the many sorts of gut-worms natural to us, and which are bred with us, perhaps in some children even before they are born; these worms, I say, do freely wander up and down the guts and stomach at their pleasure, and receive no prejudice from the concoctive faculty of them; and for this reason we see insectivorous birds so solicitous to kill worms and all other insects, by drawing them again and again through their bills, as canes through a sugar mill, that they may be verily dead before they swallow them, and instinct is the great wisdom of undebauched nature; again, admirable instances there are of animals living within animals; of which in the insect kind the Royal Society shall ere long receive some notes of mine upon Godartius. And yet I am of opinion that what has been accidentally swallowed by us alive, and that shall have the power to live-on within us, may have its designed form and shape monstrously perverted, so as to appear to us quite another thing than naturally and really it is; and this I take to be the case of this odd creature, the present subject of discourse; so that it might have been the spawn or embryo of a toad or newt.

A New Theory of Vision. By Dr. Briggs. Philos. Collect. N° 6, p. 167.

The rise of the optic nerves is very remarkable, for as the other nerves rise in a flat manner from the basis of the brain, these rise higher, from 2 gibbous protuberances, called thalami nerv. optic. as *llll*, fig. 4, pl. 15; so that the clasping of the several fibres upon them may very well resemble the flexure of the strings of a viol upon its bridge, which is likewise protuberant or of a convex figure. Now let us suppose 2 viols of equal size, and alike strung, when their bridges are put on, those strings that are in the uppermost part will have the greatest tension, and those gradually less as they are remote from the same. In like manner, I suppose that the 2 fibres that are in the zenith or apex of the 2 thalami optici have the greatest tension, and the lowest or opposite part the least tension of all, by reason of a less flexure; and the intermediate fibres proportionable or intermediate degrees, with this exception, that the internal lateral fibres are to be supposed to have less tension than the external lateral fibres, because these last have the greater flexure, as is evident from *dd* compared with *gg*.

The superior fibre therefore in one thalamus opticus, and its correspondent or superior fibre in the other thalamus, which run in a like manner to the superior part of each eye, I call fibræ concordes, as *aa*, *bb*, *cc*, &c. and answer to unisons in 2 viols or lutes, and therefore may be also called fibræ homotonæ. These arise separately from the brain; nor was there any necessity of their union at their first origin, in order to their consent, since they answer so exactly in their site and tension. In like manner are those correspondent, that are in the lowermost part of the thalami optici, and run to the lowest part of the eye, because they have the least flexure or tension; and therefore the intermediate fibres have consequently the greater tension proportionably as they are nearer the top, or less, as they are nearer the basis, only with this difference, that the internal lateral fibres have less tension than the external, as before-mentioned. Hence it appears that the fibres that are parallel, or in the same position, are as it were symphonical, viz. the 2 superiors, 2 inferiors, external-lateral, and internal-lateral, in each thalamus opticus, exactly answer each other in site and tension; so that when any impression from an external object moves both fibres, it causes not a double sensation, any more than unisons in 2 viols struck together cause a double sound. Now this parallelism of the fibres is much secured by what we are next to consider, viz.

The position of the fibres of the optic nerve, which keep their distinct order. For that the nerves decussate or cross one another, or that they are so blended together in their union, as to cause a confusion in their fibres, is not to be imagined; but those that are in the thalami optici on the right side run distinctly to the right eye, and those on the left accordingly. In many fishes the case is clear, where the 2 nerves are joined only by simple contact, and in the camelion not at all, as is said.

Besides this union of the nerves, to keep the balance, it is also much assisted by the excellent contrivance of the muscles. For it is very observable, that the musculus obliquus inferior arises out of a peculiar foramen of the orbit, whereas all the rest arise from the bottom at the exit of the optic nerve; so that, both below by this muscle, and above by the trochlearis, a distortion of the eye is prevented. And as the two oblique muscles are equally poised, so are the attollens and deprimens; for though the former be somewhat the larger muscle, yet because there is less force required to pull down than lift up the eye, their powers are equal; as for the lateral muscles, they answer exactly both in site and size, and consequently keep the balance, which is required to the consent of the fibræ concordes.

The insertion of the fibres of the optic nerve into the bottom of the eye, and their keeping still their distinct position, according as they arise from the upper, lower, or lateral parts of the thalami optici, confirm what has been said. For the optic nerve consists of two coats, the exterior, being produced from the dura mater, makes the sclerotica, as the interior, arising from the pia mater, produces the uvea, and consists of medullary fibres, which being in a distinct order expanded at their insertion into the bottom of the eye, form the tunica retiformis. This coat looks at first view indeed like a mucous substance; but if it be put into a crystal glass filled with clear water, and exposed to the light or sun, after the other coats are removed, this coat will be very well expanded (especially if the water be a little warm,) and will show all the fibrillæ very fairly and distinctly, like the fine threads of lawn, so that all these fibres keep their due position, and answer to distinct parts of objects, and observe that order or parallelism requisite to distinct vision.

From what has been said, several problems concerning vision may be solved. As,

1. Why vision is not double, since the organ is so? The answer is, because the fibræ concordes are like unisons in a lute, and the rays strike both at the same time.

2. Hence also it appears, why upon pressing down one eye, an object appears double? viz. because thereby its rays fall upon discordant fibres in the other eye not pressed, so that there are two different sensations.

An explanation of the Figure 4, *pl.* 15.—*aa, bb, cc, dd,* &c. are the fibræ concordes of the optic nerves, as they arise alike from the thalami optici *llll,* which run also to like parts of the eye; where note that *aa* are the two uppermost fibres, and are supposed to have the greatest flexure; *bb, cc, dd,* the external-lateral fibres; *ee, ff, gg,* the internal-lateral; *llll,* the two protuberances, called thalami nervorum opticorum, from whence the optic nerves have their rise; *hhhhh,* the parts of the brain that lie under them; *ii,* the optic nerves, made here the larger to express as many of their fibres as could be distinctly represented; +, the place of their union; *mm,* the eyes divested of the sclerotica and uvea; *nn,* the processus ciliares lying just under the iris; *oo,* the pupils or sights.

A short Relation out of the Journal of Capt. Abel Jansen Tasman, on the Discovery of the South Terra Incognita; not long since published in the Low Dutch. By Dirk Rembrantse. Philos. Collect. N° 6, p. 179.

The geographical accounts given by this celebrated Dutch navigator, are superseded by the more exact observations and discoveries of our own countryman, Captain Cook.

Some further Observations about the Fabric and Texture of Muscular Fibres. By M. Leuwenhoeck. Philos. Collect. N° 7, p. 188.

I have several times observed and examined the muscular flesh of the large sea lobsters found upon the coast of Norway. But by all my observations of them I could not satisfy myself of the true texture and fabric of them. But about a fortnight since, 2 lobsters were sent me from Rotterdam; on which I renewed my observations about the texture of their flesh; since which time I have clearly seen that the muscular flesh of the body, as well as of the claws of the lobsters, is composed of exceedingly small strings, which strings also have their rimples, in the same manner as I mentioned in my last letter that the muscular strings both of flesh and of other fish also had. I separated these one from another; and notwithstanding their minuteness, I have again split some of them, and to my full satisfaction I have discovered and clearly seen that every string of the muscles of a lobster is again composed of a great number of much smaller threads.

These are the observations which I have made about the texture of the muscles of lobsters, which as they are new, so I hope they will be pleasing to you, and agree with what you have observed, which I should very gladly understand at your first conveniency.

In your last letter of the 10th of March, 1681-2, you say as follows—Your discoveries both in your former and this last letter are very considerable, but I am not a little pleased to find by this, that you have discovered the same thing in the muscles of flesh, which I long since did in those of fish; especially in those of lobsters, crabs, shrimps, of which I gave you some account about 4 years since, at which time also I showed them to the Royal Society at their meeting, namely, that the muscles of these consisted of an innumerable company of exceedingly small filaments or strings, almost 100 times smaller than a hair of my head, each of which filaments was of the shape of a string of pearl, or beads of glass, so that a small string of such a muscle as large as hair seemed like a necklace of small seed pearl, which is usually made up of a great number of smaller strings, of such seed pearls. On examining the muscles of a prawn, I took notice that when I saw several of these small muscular strings lying together, that they would appear as if they had been so many strings of bullets, corals or pearls lying together. But among about 25 small separations or bundles of these muscular strings taken from the prawn, there was one of them which was about the size of a head hair, which lay very neatly to my view, for every string of it had its particular rimples, which could not be seen so well when they lay all in a bundle; because they then appeared like bullets, or when 2 or 3 lay together, for then they appeared like a braided chord, but singly. And to be sure all these several appearances of bullets or braidings are no other than the varieties of the heightenings and deepenings, which are caused by the various falling of the light upon these rimplings, of the small strings of which every bundle of muscular fibres consists.

Some Observations on a Calculus found in a Horse. By H. P. M. D. and F. R. S. Abridged and translated from the Latin. Philos. Collect. N° 7, p. 191.

Having not long since presented to the Royal Society a stone of an extraordinary size, taken from the bowels of a horse at Lambeth, I was then requested to make further inquiries concerning the calculus, as well as the horse; and to transmit the result thereof to the Society. The stone, which weighed 4 ℔ 4 oz. was as large as a man's head, and not very unlike it in shape. I inquired about the situation of the stone, the age and condition of the horse, and whether the animal's urine was bloody, or whether he was troubled with strangury or suppression of urine. But the common people are inattentive to these things; and provided these animals are capable of going through the hard labour to which they are put, those who own them or look after them give themselves no trouble whatever about other matters. I imagine the person who took out the stone to be [anatomically] ignorant of the parts or their situation. He asserts that it was found between the bladder and intestine. Perhaps however, there is not much mistatement of the fact in this assertion. For the stone was on one side flat and smooth, viz. on the side down which the urine continually dribbled, but not without difficulty, by reason of the little space that was left for it; on the other side which adhered to the bladder, it was rough and porous, and daily increasing in bulk and weight, had worn away and destroyed the whole of the tender coat of the bladder, so that no vestige thereof remained. The horse, as I was informed by the owner who had kept him for 12 years, was 15 hands high, and was employed in carrying cloth from the dyers. The pieces of cloth were heaped upon his back while they were yet warm from the vats,*

* The author here enters into an inquiry, whether the liquor (impregnated with the dying materials) from the moist pieces of cloth so frequently applied over the horse's loins, or the mere warmth thereof (for the dyed cloth was put on the horse's back quite hot from the vats) might not injure the kidneys in such manner as to contribute to the generation of the stone. The last supposition is by no means improbable, especially when it is considered, that on taking off the load of dyed cloth, more or less inflammatory action might occasionally be excited in those parts, in consequence of the sudden chills which would succeed. What renders this supposition further probable, is an observation

in such manner that the animal used to groan under the burden. These journies having been discontinued for some years the horse became very restiff and unmanageable whenever a saddle was put upon him, or whenever any person mounted him; either because the pressure of the saddle incommoded him, or because when he was rode fast, he experienced from the shaking great pain and irritation. It is further to be remarked that he was constantly kept upon hay and other dry food, not having for some years been turned out to grass, which doubtless would have been of great service. Every year during spring and autumn he was observed to lose flesh, and to become so heavy and languid in his hind parts, as scarcely to be capable of moving his legs, which he dragged after him. About 8 or 10 days before he died, ischuria took place, the calculus having so completely filled up the cavity of the bladder as to leave no passage for the urine. The animal now threw himself upon the ground, beat and tossed himself about, cast up the earth with his feet, and expressed the acuteness of his pains by the most violent movements. It is very remarkable that during the whole time his urine was suppressed, he obstinately refused to drink, as if he had been instinctively aware that if he swallowed any water it would only aggravate his sufferings, as it could not pass off by the kidneys or bladder. This case of calculus I at first imagined to be without parallel; but I have since met with a similar instance in the Journal des Sçavans.*

An Account of a small Discourse about Comets, published in High-Dutch at Neurenburg 1681, *by a Lover of Astronomy. Philos. Collect.* N° 7, p. 196.

A dialogue between a churchman and a naturalist, containing the most trifling notions and fancies about comets.

of Salmasius's (quoted by this author) that he (Salmasius) had known several persons to be affected with stone in the kidneys, in consequence of sitting at table with their backs to the fire.

* *Description of a strange Stone found in the Body of a Horse. Philos. Collect.* N° 7, p. 195.

There was lately at this place (Paris) a stone of a very extraordinary size found in the body of a Spanish gelding, about 13 or 14 years old, which died in the riding-school of M. de Bernardy; the weight of it being 4 ℔, of a roundish figure, a little flatted, its longest diameter was 5 inches, and its shortest 4: it was of the colour of an olive, but a little inclining to a brown, marked with several red spots resembling coagulated blood: radiated circularly with black and white veins and waves: but for the rest of it so delicately polished, that it reflected the images of the objects about it. It was found enveloped in a membrane of fat, and fastened by two ends to the spine of the back near the kidneys: it was more than 12 hours after the horse was dead before it was taken out of his body, when it was found very hot, though the body of the horse was quite cold, and it retained a considerable heat about 6 hours after it was taken out.—Original.

An Account of another small Treatise written in High-Dutch, and printed at Bazil, Anno 1681. *Containing a New Theory about the Comets, invented by James Bernouilli,* the Title whereof is thus expressed: A new Introduction, showing how the Motions of Comets may be reduced to some certain and Geometrical Rules, so that their appearance may be predicted. Philos. Collect.* N° 7, p. 199.

Before explaining his theory, he thinks it proper to say something of the original cause of comets, of their course and motion, of their place and tail.

What concerns their principal or original cause, he says, that that old opinion which supposes them to be a collection of vapours and exhalations put into a flame by the uppermost part of the air, is scarcely worth any consideration: for if the comets by such an inflammation should have their light of themselves, there could be given no reason why the tail should always be seen in opposition to the sun; and moreover if the whole earth should be resolved into vapours, there would not be enough to form only the tail of such a comet. Descartes's opinion, he says, is strange enough: for according to this hypothesis, the sun or our earth might after sometime be transformed into a comet, as some have endeavoured to prove out of the solar spots. The author's own opinion is, that comets are like other stars or planets, perpetual bodies, which have their certain

* James Bernoulli, a celebrated mathematician of the 17th century, and professor of mathematics at Basil, was born at that town 1654. His father intending him for the church, placed him in that university to study divinity. But his inclination leading him powerfully to astronomy and mathematics, he soon made a considerable progress in those sciences, without a preceptor, and almost without books, and even as it were by stealth; a circumstance which led him to adopt for his device, Phaeton driving the chariot of the sun, with this motto, "Invito patre sidera verso," I traverse the stars against my father's will. From 1676 he spent some years in travels in Germany, France, Holland, England, &c. from whence he returned to his native country in 1682, where he exhibited a course of experiments in natural philosophy and mechanics, consisting of various new discoveries. The same year he published his Essay on a new System of Comets, as above, and the year following his Dissertation on the Weight of the Air. About 1684 M. Leibnitz published in the Leipsic Acts, some essays on the new Calculus Differentialis, but without discovering the method of it, Bernoulli, on explaining it, received great applause from that philosopher, who declared the invention belonged as much to Bernoulli as to himself. In this matter, and in that of Series, however, it is probable, he had derived some knowledge by his communications with men of science when in England. In 1687, Bernoulli became professor of mathematics in the university of Basil, where he died in 1705, in the 51st year of his age, by a disorder arising from his close and intense application to study. Many of his compositions were published in the Memoirs of the Academies of Paris, Berlin, Leipsic, &c. His works were collected and published at Geneva 1744, in 2 vols. 4to.; but with the unaccountable omission of his posthumous work, *De Arte Conjectandi*, one of his most ingenious labours, and published in 1713. James Bernoulli had an excellent genius for invention and elegant simplicity. He introduced his younger brother John into his own favourite sciences, who lived to a great age, and became equally celebrated with himself, and indeed often held very rude and vexatious contention with him for the mastery in those sciences. Their descendants have also to this day continued to pursue the same illustrious career in the mathematical sciences; resembling in that respect very much the celebrated race of the Gregories in this country.

place and room in the world, in and about which they ought to move as long as the world will last.—But their motion, he thinks, cannot be explained by any straight line, for if so, the comet would run from one vortex into another, and so could never transcend 6 signs or 180 degrees, which is against experience, and for that reason he supposes it must be some crooked or bended line, as a circular, elliptical, or some other of that kind.—As to the place of the comets, he thinks it cannot be beneath the moon: For 1. The parallax proves the contrary. 2. If the comet should be lower than the moon, it would be also sometimes eclipsed by the shadow of the earth, when by experience the comets have at that time a curled circular light in the form of a rose. And 3, the tail before and after the conjunction with the sun would be the shortest of all, against the experience of our sight. Next, as the comets for these reasons are above the moon, so he thinks they cannot have any place among the planets, for their orb requires a room far larger, to avoid the intersection which continually would happen with the planetary orbs. From hence he concludes that there is no place more fit for them than beyond Saturn, according to Descartes' opinion. And thus having adopted the solid orbs and vortices of Descartes, to avoid the intersections of such orbits, he is obliged to find a place in the immense space between Saturn and the fixed stars, where they perform all the motions about an imaginary centre in that space, without ever coming below the orbit of Saturn.

The Quadrature of the Circle; from the Leipzic Acts, No. 2. By M. Leibnitz. Philos. Collect. N° 7, p. 204.

Geometricians have always endeavoured to find out the proportions between straight and curve lines. And even to this day, though they have made use of algebraical helps, they do not find the matter so easy to be accomplished by the methods hitherto published. Archimedes was the first, for what we know, who found out the ratio between a cone, a sphere, and a cylinder, of the same altitude and base, viz. that of the numbers 1, 2, 3, so that the cylinder is triple the cone and $\frac{3}{2}$ the sphere; for which cause he commanded that a sphere and a cylinder should be carved on his tombstone. The same Archimedes found out the quadrature of the parabola. Of late there is invented a method of measuring innumerable curve-lined figures, especially when the ordinates are in any proportion multiplicate or submultiplicate, direct or reciprocal, of the intercepted abscissas.

For the figure ABCA (fig. 2, pl. 15,) will be to the circumscribed rectangle ABCD, as an unit is to a number expressing the multiplication of the ratio, increased by an unit. For instance, the intercepted lines AB or DC in a parabola, being in proportion as the simple natural numbers, viz. as 1, 2, 3, &c. And the ordinates BC being as the squares of those numbers, viz. 1, 4, 9, &c. or in duplicate the proportion of them, and the number expressing that ratio being 2, the figure ABCA will be to the rectangle ABCD, as 1 to 2 + 1, or as 1 to 3, or the figure shall be a third part of the rectangle. If AB or CD remaining as the natural numbers 1, 2, 3, and BC, BC be made as the cubes, or 1, 8, 27, &c. as in the cubical parobala, the ratio of the ordinates being triplicate of the ratio of the intercepted parts, the figure will be to the rectangle, as 1 to 3 + 1, that is to 4, or a fourth part. But if DC, DC be as squares, and BC, BC as cubes, that is the ratio of BC, BC, be $\frac{3}{2}$ the ratio of DC, DC; the figure, a semi-cubical parabola, ABCA, to the rectangle ABCD, will be as 1 to $\frac{3}{2}$ + 1, or shall equal $\frac{3}{5}$ of the rectangle. In reciprocals, to the number expressing the multiplication of the ratio, the sign — or minus must be prefixed.

But a circle could never be brought within the compass of these rules; and though it has been laboured for by geometricians in all times, there could never yet be found a number by which the ratio of the circle, to the circumscribed square, could be expressed.

Nor could the ratio of the circumference to the diameter be ever yet found, which is quadruple of the ratio of the circle to the square. Archimedes indeed, inscribing and circumscribing polygons to a circle, it being larger than the inscribed and less than the circumscribed, shows a way of giving the limits within which the circle falls, or of giving approaches, by which he shows that the ratio of the circumference to the diameter is more than as 3 to 1, or than as 21 to 7, and less than 22 to 7, which method others have since prosecuted, viz. Ptolemy, Vieta, Metius, and most of all Ludolphus van Ceulen, who has shown the circumference to be, as 3.14159265358979323846 &c.

to the diameter 1.00000000000000000000.

But these sorts of approaches, though they are useful in practical geometry, give no satisfaction to the mind, which covets the very truth, unless the progression of such numbers could be carried on in infinitum. Many indeed have professed to have found the true quadrature, as Cardinal Cusanus, Oron. Fineus, Jos. Scaliger, Tho. Gepherander, Tho. Hobbs, but all of them falsely, being disproved by the calculations of Archimedes, or by those of Ludolphus van Ceulen.

But because I perceive there are many who do not well understand what it is that is desired in this matter; they are to know that the quadrature, or the turning of the circle into an equal square, or any other right-lined figure, which depends on the ratio of the circle to the square of its diameter, or of the circumference to its diameter, may be understood to be fourfold, viz. either by calculation, or by linear construction: and each of them again may be either perfectly exact, or else almost, or pretty near. The quadrature by accurate calculation, I call the analytical. That which is done by accurate construction, I call the geometrical. That which is done by calculation pretty near, I call the approach. That which is by construction pretty near, I call the mechanical. The approaches have been furthest carried on by Ludolphus van Ceulen; Vieta, Huygens and others have given several mechanical.

The accurate geometrical construction may be had, by which not only an entire circle may be measured, but any section or arch of it also, which is by an exact and ordinate motion; but such notwithstanding as suits with transcendental curves, which erroneously are accounted mechanical, though in truth they are as geometrical as those which are commonly so esteemed, though they are not algebraical, nor can be reduced to equations algebraical or of certain degrees, they having degrees proper to themselves, which though they be not algebraical, are yet nevertheless analytical. But these things I cannot in this place explain so fully as they deserve.

The Analytical Quadrature, or that which is made by accurate calculation, may be again subdivided into three kinds, viz. into the analytical transcendent, the algebraical, and the arithmetical. The analytical transcendent is to be obtained, among others, by equations of indefinite degrees, hitherto considered by none. As if $X^x + X$ be equal to 30, and X be sought, it will be found to be 3, because $3^3 + 3$ is $27 + 3$, or 30; which kind of equations for the circle we will give in their proper place.

The Algebraical is done by vulgar numbers, though irrationally vulgar, or by the roots of common equations, which for the general quadrature of the circle, or its sectors, is indeed impossible. Now there remains the arithmetical quadrature, which is performed by certain series exhibiting the quantity of the circle exact by a progression of terms, first rational, such as I shall here propound.

I have found therefore, that if the square of the diameter be put 1, the area of the circle will be, $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \frac{1}{13} - \frac{1}{15} + \frac{1}{17}$, &c. viz. the entire square of the diameter being diminished, that it may not be too large, by a third part; and again, because hereby too much is taken away, being augmented by one fifth; and again, because by this last too much is added, diminished by one seventh, and so onward continually.

So that the first quantity will be

too great, viz. 1, but the error will be less than $\frac{1}{3}$;
The next too little, viz. $1 - \frac{1}{3}$, but the error will be less than $\frac{1}{5}$;
The 3d too much, viz. $1 - \frac{1}{3} + \frac{1}{5}$, but, &c. $\frac{1}{7}$;
The 4th too little, viz. $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7}$, but, &c. $\frac{1}{9}$;
&c. &c.

The whole series contains all the approaches together, or the values, both greater than they ought to be, and less than they ought to be.

So that by continuing the series, the errors may be made less than a fraction given, and consequently less than any assignable quantity. Whence it follows, that the whole series must give the true value. And though the sum of the whole series cannot be expressed by one number, and that the series be infinitely continued, yet because it consists of one regular method of progression, the whole may sufficiently enough be conceived by the mind. For since the circle is not commensurate to the square, it cannot be expressed by one number, but it must necessarily be exhibited in rational numbers by a series, in the same manner as the diagonal of a square; and the section made according to extreme and mean proportion, which some have called the divine section, and many other quantities which are irrational. And indeed if Van Ceulen could have given a rule by which his numbers 314159 &c. could have been continued in infinitum, he would have given us the arithmetical quadrature exact in whole numbers, which we have here done in fractions.

Now lest some less knowing in these matters might think that a series consisting of infinite terms cannot possibly be equal to a circle, which is a finite quantity, they must know that there are many series whose terms are infinite in number, whose whole sum is equal to finite quantities. As, to give an easy example, the series from 1 decreasing in subduple geometrical proportion, $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}+\frac{1}{64}$, &c. in infinitum, the whole of which notwithstanding makes no more than 1.

There are several things relating to this quadrature, which might be taken notice of. But I have not now the leisure to prosecute it. However I must not omit one, viz. that the terms of this our series $\frac{1}{1}, \frac{1}{3}, \frac{1}{5}, \frac{1}{7}, \frac{1}{9}$, &c. are in a continued harmonical progression. And a series made by skipping, as $\frac{1}{1}, \frac{1}{5}, \frac{1}{9}, \frac{1}{13}, \frac{1}{17}$, &c. is also of harmonical progression. And $\frac{1}{3}, \frac{1}{7}, \frac{1}{11}, \frac{1}{15}, \frac{1}{19}$, &c. is also a series of harmonical proportionals. Wherefore since the circle $= \frac{1}{1} + \frac{1}{5} + + \frac{1}{9}\ \frac{1}{13} + \frac{1}{17} +$ &c. $- \frac{1}{3} - \frac{1}{7} - \frac{1}{11} - \frac{1}{15} - \frac{1}{19}$, &c. by subtracting the latter partial series from the former partial series, the circle will be the difference of two series in harmonial progression. And because the sum of any number of terms in harmonial progression, how many soever, may by some compendium be obtained; hence compendious approaches may be deduced, if one would in this our series take out the terms affected with the sign $-$ by adding the two next into one, $+\frac{1}{1}-\frac{1}{3}$ and $+\frac{1}{5}-\frac{1}{7}$ and $+\frac{1}{9}-\frac{1}{11}$, and $+\frac{1}{13}-\frac{1}{15}$, and $+\frac{1}{17}-\frac{1}{19}$, and so onward, he will have a new series for the magnitude of the circle, namely $\frac{2}{3}$ (that is $\frac{1}{1}-\frac{1}{3}$) $+\frac{2}{35}$ (that is $\frac{1}{5}-\frac{1}{7}$) $+\frac{2}{99}$ (that is $\frac{1}{9}-\frac{1}{11}$), &c. wherefore the square inscribed being $\frac{1}{4}$, the area of the circle shall be $\frac{1}{3}+\frac{1}{35}+\frac{1}{99}+\frac{1}{19}+\frac{1}{323}$, &c.

But the numbers 3,35,99,195,323, &c. by skipping, are taken out of the series of square numbers, 4, 9, 16, 25, &c. diminished by a unit, and so made the series 3, 8, 15, 24, 35, 48, 63, 80, 99, 120, 143, 168, 195, 224, 255, 288, 323, 360, 399, &c. out of the members of which series every fourth after the first, is a number of this our series. But I have found the sum of this infinite series $\frac{1}{3}+\frac{1}{8}+\frac{1}{15}+\frac{1}{24}+\frac{1}{35}+\frac{1}{48}+\frac{1}{63}+\frac{1}{80}+\frac{1}{99}$, &c. to be $\frac{3}{4}$. And by culling out by single skipping, as $\frac{1}{3}+\frac{1}{15}+\frac{1}{35}+\frac{1}{63}+\frac{1}{99}$, &c. the sum of this infinite series makes $\frac{2}{4}$ or $\frac{1}{2}$. But if out of this again another progression be culled out by single skipping, as $\frac{1}{3} + \frac{1}{35} + \frac{1}{99}$, &c. the sum of that infinite series will be the semicircle, the square of the diameter being 1. Now, because by the same means the arithmetical quadrature of the hyperbola is obtained, I thought it not amiss to represent to view the whole harmony.

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20, &c.
1 4 9 16 25 36 49 64 81 100 121 144 169 196 225 256 289 324 361 400, &c.
0 3 8 15 24 35 48 63 80 99 120 143 168 195 224 255 288 323 360 399, &c.

$\frac{1}{3}\ \frac{1}{8}\ \frac{1}{15}\ \frac{1}{24}\ \frac{1}{35}\ \frac{1}{48}\ \frac{1}{63}\ \frac{1}{80}\ \frac{1}{99}\ \frac{1}{120}\ \frac{1}{143}\ \frac{1}{168}\ \frac{1}{195}\ \frac{1}{224}\ \frac{1}{255}\ \frac{1}{288}\ \frac{1}{323}\ \frac{1}{360}\ \frac{1}{399}$, &c. $= \frac{3}{4}$;

$\frac{1}{3} \cdot \frac{1}{15} \cdot \frac{1}{35} \cdot \frac{1}{63} \cdot \frac{1}{99} \cdot \frac{1}{143} \cdot \frac{1}{195} \cdot \frac{1}{255} \cdot \frac{1}{323} \cdot \frac{1}{399}$, &c. $= \frac{2}{4}$;

$\frac{1}{8} \cdot \frac{1}{24} \cdot \frac{1}{48} \cdot \frac{1}{80} \cdot \frac{1}{120} \cdot \frac{1}{168} \cdot \frac{1}{224} \cdot \frac{1}{228} \cdot \frac{1}{360} \cdot$ &c. $= \frac{1}{4}$;

$\frac{1}{3} \cdot \cdot \cdot \frac{1}{35} \cdot \cdot \cdot \frac{1}{99} \cdot \cdot \cdot \frac{1}{195} \cdot \cdot \cdot \frac{1}{323} \cdot \cdot$ &c. $=$ C;

$\frac{1}{8} \cdot \cdot \cdot \frac{1}{48} \cdot \cdot \cdot \frac{1}{120} \cdot \cdot \cdot \frac{1}{224} \cdot \cdot \cdot \frac{1}{360} \cdot$ &c. $=$ H;

where C is the circle ABCD, whose inscribed square is $\frac{1}{4}$, and H is the hyperbola CBEHC, whose square ABCD is $\frac{1}{4}$.

In the same figure, viz. fig. 5, pl. 15, to the asymptotes AF, AE, at right-angles to each other, let there be described the curve-line of an hyperbola GCH, whose vertex is C, and ABCD the power or square to which every rectangle made of the ordinate as EH and the intercepted part AE, is always equal. About this square let a circle be drawn, and let the hyperbola be continued from C to H, so that AE be double of AB. Then putting AE to be 1, AB shall be $\frac{1}{2}$, and its square ABCD will be $\frac{1}{4}$, and the circle whose power ABCD is inscribed, will be $\frac{1}{3} + \frac{1}{35} + \frac{1}{99}$, &c. but the portion of the hyperbola CBEHC, whose power inscribed is the same square $\frac{1}{4}$, which represents the logarithm of the ratio of AE to AB, or of 2 to 1, will be $\frac{1}{8} + \frac{1}{48} + \frac{1}{120}$, &c.*

* The above series, for the quadrature of the circle and hyperbola, were before invented by M. Mercator, and James Gregory.

END OF THE PHILOSOPHICAL COLLECTIONS.

A Description of Pen-park Hole in Gloucestershire. Communicated by Sir Robert Southwel. N° 143,* p. 2. *Vol. XIII.*

There is a place in Gloucestershire called Pen-park, about 3 miles from Bristol, and above 3 from the Severn, where some miners for lead discovering a large hole in the earth, one Captain Sturmy, a warm inquisitive seaman, who

† After being discontinued during four years, the Philosophical Transactions were begun again with this N° 143, by Dr. Plot, who had succeeded Mr. Hooke, one of the secretaries, Nov. 30, 1681; and who again undertook to renew the publication, on the encouragement afforded by the Society, in promising to purchase 60 copies of each number of the work.

has written a large folio on Navigation, would needs descend into it, and his narrative was as follows:

"On the 2d of July, 1669, I descended by ropes affixed at the top of an old lead-ore pit, 4 fathoms almost perpendicular, and from thence 3 fathoms more obliquely, between two great rocks, where I found the mouth of this spacious place, from which a miner and myself lowered ourselves by ropes, 25 fathoms perpendicular, into a very large place, which resembled to us the form of a horse-shoe; for we stuck lighted candles all the way we went, to discover what we could find remarkable. At length we came to a river or great water, which I found to be 20 fathoms broad, and 8 fathoms deep. The miner would have persuaded me that this river ebbed and flowed, for that some 10 fathoms above the place where we now were, we found the water had sometime been; but I proved the contrary, by staying there from 3 hours flood to 2 hours ebb, in which time we found no alteration of this river. Besides its waters were fresh, sweet, and cool, and the surface of this water as it is now at 8 fathoms deep, lies lower than the bottom of any part of the Severn sea near us, so that it can have no communication with it, and consequently neither flux nor reflux, but in winter and summer, as all stagnant lakes and loughs (which I take this to be) have. As we were walking by this river, 32 fathoms under ground, we discovered a great hollowness in a rock, some 30 feet above us, so that I got a ladder down to us, and the miner went up the ladder to that place, and walked into it about 70 paces, till he just lost sight of me, and from thence cheerfully called to me, and told me he had found what he looked for, a rich mine; but his joy was presently changed into amazement, and he returned affrighted by the sight of an evil spirit, which we cannot persuade him but he saw, and for that reason he will go thither no more.

"Here are abundance of strange places, the flooring being a kind of a white stone, enamelled with lead-ore, and the pendant rocks were glazed with saltpetre, which distilled upon them from above, and time had petrified.

"After some hours stay there we ascended without much hurt, except scratching ourselves by climbing the sharp rocks. But for 4 days after my return I was troubled with a violent head-ach, which I impute to my being in that vault."

Captain Sturmy falling from his head-ach into a fever, and dying; what from his death, and the opinion of an evil spirit, nobody was willing to have any more to do with the said hole from that time to this.

But Captain Collins, commander of the Merlin yacht, who is by his Majesty appointed to take a survey of the coast of England, coming to the Severn for that purpose, and visiting Sir Robert Southwel near Kingroad, Sir Robert told him how the story of this hole had amused the country; and that the narrative had formerly been sent to his Majesty and the Royal Society; and that there wanted only some courage to find out the bottom of it. The captain resolved to adventure, and on the 18th and 19th of Sep. 1682, he took several of his men, with ropes and tackling fitting to descend, with lines to measure any length or depth, also with candles, torches, and a speaking trumpet. What he found does much lessen the credit and terror of this hole, as will appear by the figure he took thereof, and the description following:

" It is down the tunnel from the superficies to the opening of the cavity below 39 yards. Then the hole spreading into an irregular oblong figure, is in the greatest length 75 yards, and in the greatest breadth 41 yards; from the highest part of the roof to the water was then 19 yards; the water was now in a pool, at the north end, being the deepest part, it was in length 27 yards, in breadth 12, and only 5 yards and ½ deep; two rocks appeared above the water all covered with mud, but the water sweet and good; there was a large circle of mud round the pool, and far up towards the south end, which showed that the water has at other times been 6 yards higher than at present.

" The tunnel or passage down was somewhat oblique, very ragged and rocky; in some places it was 2 yards wide, and in some 3 or 4, but nothing observable therein, save here and there some of that spar which usually attends the mines of lead ore. In the way, 30 yards down, there runs in, southward, a passage of 29 yards in length, parallel to the superficies above; it was 2 and 3 yards high, and commonly as broad, and alike rocky as the tunnel, with some appearances of spar, but nothing else in it except a few bats.

" The cavity below was in like manner rocky, and very irregular; the candles and torches burnt clear, so as to discover the whole extent thereof; nor was the air any thing offensive. The 3 men that went down the first day staid below 2 hours and ½. The next day the captain went down with 7 or 8 men, who staid below for an hour, and observed all things.

" The bottom of this hole, where the land-waters gather, is 59 yards down from the superficies of the earth, and by good calculation the same bottom is 20 yards above the highest rising of the Severn, and lies into the land about 3 miles distant from it."

The profile and ground-plot of the concave in Pen-park, before described, fig. 1, *pl.* 16.—A is the superficies of the earth; B the lead-ore pit; C the tunnel or passage down; D the long gallery; E the concave or cell; F the upper edge of the mud; G two small rocks that appear above the water; H the upper part of the water; I the bottom of the water; K the highest mark of the water; L two rocks.

Experiments made for altering the Colour of the Chyle in the Lacteals. By Martin Lister, Esq. N° 143, p. 6.

The passage of the chyle through the intestines into the lacteal veins, is a thing hitherto demonstrated to the eye by none. Dr. Lower ingeniously confesses the ill success he had in trying with air, or with tinged spirit of wine; by neither of which he was able to force a passage. And J. Walæus (Epist. de Motu Chyli) is very positive, that however the chyle in the intestines may be diversely coloured, yet it is still white in the lacteal veins. And Diemerbrook in his late Anatomy, published 1672, p. 37, affirms, Chylum semper album inveniri in vasis lacteis mesentericis et thoracicis—viridem vero rubrum alterius ve coloris, in iis a nemine hactenus visum fuisse.

Notwithstanding which, and my own former unsuccessful trials, Phil. Trans. N° 95, I did not doubt but that some happy experiment would show the contrary; and a purposely coloured chyle might find admittance into the lacteal veins, though not by force, yet by the consent and introduction of nature herself. The success of some late experiments I made to this purpose I shall here acquaint you with.

I caused a dog to be fed, and after about 4 hours I opened the abdomen, and making a small incision in the jejunum, I injected an ounce or two of a clear tincture of indigo dissolved in fair water and strained. This done, we stitched up the gut and all again, and turned the dog upon his legs. After 1 hour and ¼ we cut the stitches, and then beheld a copious distribution of chyle and turgid lacteal veins, but as white as ever. And yet carefully searching the guts, we perceived none of the injected liquor any where.

Another dog, which was kept fasting 40 hours, had a very little flesh, without water, given him, about 5 hours before the injection of the tincture of indigo; which was performed after the same manner as before, only it was now well warmed, and about 12 oz. thrown up the duodenum, and down the ileum. Here were empty guts, and not the least appearance of any lacteal veins in the mesentery. After full 3 hours the stitches were cut again, and carefully examining the mesentery, we found many lacteal veins of an azure-colour, and cutting some of the largest of them asunder, we plainly saw a thick blue chyle to issue forth, and to spread itself over the transparent membranes of the mesentery.

Whence, although it has been doubted of by some, yet it is most evident, that the lacteal veins receive their contents from the cavity of the intestines.

A correct Tide Table, showing the true Times of the High-waters at London Bridge, to every Day in the Year 1683. *By Mr. Flamsteed.* N° 143, p. 10.

An Account of the Tide-table.—Considering how much the river Thames is frequented by shipping, and how long it has been the chief place of commerce in these parts of the world, our seamen's accounts of its tides should be very exact, and their opinions concerning them rational; whereas if they be inquired into, nothing will be found more erroneous and idle. For, observing that the high-waters at and near the new and full moons, run an hour and a half, or 2 points of the compass, longer than at the quarters, they conclude generally that it is the variable winds that cause it, never considering how improbable it is that so inconstant and changeable a cause should effect so constant an inequality.

In which opinion the tide tables of our almanacs have contributed much to confirm them; for there the moon's age is got by the epacts; thence the time of her southing, by the allowance of 48 minutes of time for every day's age, as if her diurnal motions and returns to the meridian were altogether equable, than which nothing is more false; and then the time of the high-water at London bridge is made by adding 3 hours to the time of her southing so gotten, as if there were the same constant space of time between the moon's southing and the high-waters, which by this means are often made 2 hours different from truth and experience.

To amend this fault, some of the more skilful have calculated the times of the moon's southings exactly, and then made their tide tables by adding 3 hours constantly to them; by which means, though they agreed nearer with experience at the spring-tides, or near the new and full moon, yet they erred not much less than by the old way at the quarters, or in the neap tides; the inequality of the tides being above double to the error committed in finding the moon's southings by her age.

Mr. Booker, in his almanac, was the first that gave any directions for the amendment of this reckoning, and that was only to subtract an hour from the times in his tide-table, about the first and last quarters of the moon, because the neap-tides did not flow so long as the springs by 1 point of the compass. But Mr. Henry Philips, a person well known by his works of navigation, was certainly the first that brought the inequality to a rule, whose Theory of the Tides, and a table grounded on it, for the year 1668, was printed in Mr. Oldenburg's Philosophical Transactions, for the month of April that year, N° 34, which was found much more conformable to experience than was expected.

Having frequent occasion to pass between London and Greenwich by water, some 2 years ago I observed that the tides seldom hold out so long as Mr.

Philips's calculation gave them, and therefore in the months of October and November I began to observe them more diligently, and procured them to be carefully noted by an ingenious friend at Tower-wharf. From these observations I raised a correction of Mr. Philips's numbers, and caused a tide-table to be made agreeable to it, which was printed by Mr. Hook in his Philosophical Collections, N° 4.

But the weather then proving stormy and unseasonable, I dare not rely on those observations, nor that correction, therefore in the spring and summer months following, of the year 1682, I began to observe them again, and with the help of my friends and servants, I noted the times of above 80 high-waters at Tower-wharf and Greenwich, by which I found that the greatest and least differences between the moon's true southing and the high-waters were not, as Mr. Philips had placed them, at the full or new and quarter moons, but the greatest nearer to the neaps, the least to the highest spring-tides. I found also that the inequality was not the same that he had made it; and after a trial or two, that I could represent and answer above 60 of these observations with less than one quarter of an hour's difference; which, considering how difficult it is to determine the time of a high-water exactly, I cannot but esteem a very good agreement.

Hitherto our tide-tables have only showed the time of that one high-water which next follows the moon's southing; but in this new table I have given the times of both, concerning which, I desire it may be noted, that when, by reason of great droughts in summer, or extreme frosts in winter, the springs are low, and the fresh waters less than usual, the tides may hold up longer than the times noted in the table; as also when strong north-westerly or northerly winds blow, which bring in an extraordinary flood from the northern seas, and keep it up longer than other times. So on the contrary, when the winds blow hard on the opposite points of the compass, or when we have much rain and great freshes, the tides hold not out so long as the times showed in the table, the freshes overpowering and checking them sooner; yet have I never found that the differences between the calculated and observed high-waters have much exceeded half an hour; most commonly they are scarcely half so much.

This table may be reduced and made to serve for any other port of his majesty's dominions and neighbouring countries, by only subtracting or adding so much time to the high-waters noted in it, as the high-water observed in the said place shall be found to precede or follow the time of the high-water the same day. For by such accounts as I have met with and received of the tides in remote places, I find there is every where, about England, the same difference between the spring and neap-tides, that is here observed in the river Thames.

I could easily have made and given you a table for this reduction, if I dare have relied on the account our mariners give of the tides in other ports; but I find their opinions different, except where they have copied from each other in their calendars; by reason of the afore-mentioned difference between the times of the moon's southings and the true high-waters; for which reason I forbear it, till further experience shall have informed us better.

An Observation of the Beginning of the Lunar Eclipse which happened Aug. 19, 1681, *in the Morning, made on the Island of St. Lawrence or Madagascar. By Mr. Thomas Heathcot, and communicated by Mr. Flamsteed.* N° 143, p. 15.

Mr. Heathcot was surgeon to a ship, which lay then at the bottom of a deep bay, on the western shore of the island, and that part which the Portuguese and our maps call the Terra del Gada. He had a quadrant of 2 feet radius, and a telescope of 9 feet, but no clock; to supply which defect, he made a pendulum of a string and a bullet 39 inches long, that each single vibration might answer a second of time; waiting the beginning of the eclipse with his glass, as soon as he saw the true shadow enter on the moon's limb, he caused his friends, who assisted him, to make the pendulum vibrate, and count its vibrations; of which they had numbered 140, or $2^m\ 20^s$ of time when he took the height of Procyon, then east of the meridian, 25° 39′; the next day he observed the sun's meridional height with the same quadrant, whence he found the latitude of that place 19° 29′ south; hence the time when he took the height of Procyon is found $4^h\ 51^m$ morn, and subtracting the $2^m\ 20^s$ past since the observed beginning of the eclipse, it gives, for

The true beginning of the eclipse.................	4^h	48^m	40^s
Which at the observatory here I noted at..........	1	50	40
Therefore this part of Madagascar is more easterly	2	58	0

or 44° 30′, which our maps make 52°, that is $7\frac{1}{2}$° more remote from it than it really is.

Observations made at Dantzic, of the Comet which began to appear there, Aug. 16, 1682. *By M. Hevelius.* N° 143, p. 16.

Of the late comet, I have obtained many of the meridian altitudes, and distances from the fixed stars. But it would be tedious to mention all these, neither have I now time to submit them to a strict calculation. At present it may suffice to observe, that the comet was first seen here at Dantzic, Aug. 25, new style, 1682, and that I duly observed it from that time till Sept. 7. As to the path of this comet, with its velocity, and the angle of its orbit made with the ecliptic, these may be gathered from the following table: which, however, it may be observed is not composed from any accurate calculation, but is deduced by a looser way of reasoning, from a consideration of the globe.

Month. Day.	Time of the Day.	Longit. of the Comet.	Latitude of the Comet.	Motion in its Orbit.	Daily Motion more exact.
Aug. 26	3^h 0^m morn.	♋^{s} 23° 30′	21° 0′ north		
27	11 0 even.	♌ 5 0	23 30	10° 0′	5° 28′
28	even.				5 35
29	even.				5 41
30	3 30 morn.	18 0	25 20	13 20	
30	9 0 even.	22 0	25 40	3 30	5 46
31	3 30 morn.	24 30	26 0	2 20	5 50
Sept. 1	3 30 morn.	♍ 1 0	26 0	5 45	
1	9 0 even.	6 0	25 40	4 45	5 46
2					5 43
3	8 30 even.	20 0	24 30	11 30	5 40
4	even.				5 34
5	even.				5 24
6	9 0 even.	♎ 5 0	20 30	15 0	5 0
7	even.				4 30
8	8 0 even.	12 0	18 15	8 0	4 0
9	8 30 even.	15 30	17 15	3 30	3 30
10	8 0 even.	18 30	15 45	3 0	3 0
11	even.				2 40
12	8 0 even.	23 0	14 0	5 0	2 20
13	7 30 even.	25 0	12 30	2 0	2 0

So that, by its proper motion in its orbit, from Aug. 26 to Sept. 13, it has moved over 83° 27′, and in the ecliptic 91° 30′; also its latitude first increased to 26° north, and then decreased to 12° 30′.—It may be remarked, that its northern node is in 24° of ♉, and its southern node in 24° of ♍; but its limits were in 24° ♌ and ♒. The angle of the orbit with the ecliptic was nearly 26°. But whether for its whole duration it was entirely consistent with its nodes, or whether it varied, and how much, will appear from the calculation.—As to its appearance, the head was rather brighter and larger than that of the year 1681 but, on the other hand, it had a much shorter tail. With a long telescope we could observe in the head only one nucleus, and that always of an oval and gibbous figure; except that on Sept. 8 especially, a very bright ray proceeded from the nucleus, which was rather crooked, and passed into the tail. This is the more remarkable, as I do not remember to have seen the like appearance in any other comet. It may also be observed, that sometimes its tail was directed pretty exactly in opposition to the sun, as Aug. 30 in the morning; but often with a considerable deviation, as is usual in most comets. Its coma was also of various lengths: for, at first the tail was of about 12° long; afterwards rather shorter, and sometimes longer, as far as to 15° or 16°; but towards the end it diminished continually. See fig. 2, pl. 16.

An Account of some Books. N° 143, p. 20.

I. Κλαυδιου Πτολεμαιου Αρμονικων Βιβλια γ. Claudii Ptolemæi* Harmonicorum Libri Tres. Ex Codd. Mss. Undecim, nunc primum Græce editi. Johannes Wallis, ss. Th. D. Geometriæ Professor Savilianus Oxoniæ, Regiæ Societatis Londini Sodalis, Regiæque Majestati a Sacris; recensuit, edidit, Versione et Notis illustravit, et Auctarium adjecit. Oxonii, e Theatro Sheldoniano, A. D. 1682. In Quarto.

This work having been never before printed in Greek, and but very imperfectly in Latin, by Anton. Gogavinus of Graves, above 100 years since: the learned professor thought fit to give it in a more perfect edition. For which purpose he has diligently compared the manuscript copies, for restoring and perfecting the Greek text; and adjoined a new Latin translation of the whole, with notes on the text: rectifying many mistakes of the transcribers, especially in the numbers.

This work of Ptolemy gives an account of the nature of sounds in general, but especially of those that are musical; as, of the several sorts of tones, and their ratios one to another, &c. Showing also how harmony may be fitly compared to the motions of the human soul; and those of celestial bodies. And it is the more considerable, as being not only the best of the kind in Greek, but as it also gives an account of the rest; wherein they agreed or disagreed, one with another, and on what principles.

* Claudius Ptolemy, or Ptolomy, was a celebrated mathematician of Pelusium, who flourished under Adrian, and Marcus Aurelius, about the year 138, and who left several other valuable works, besides that above noticed. As, 1, his geography, first printed at Bologna, 1462, in folio, and several times since. 2. Some astronomical works, the principal among them the Almagest, first printed at Basil, 1551, folio. 3. Some works on judicial astrology, &c. in 1535. He is also celebrated on account of the system of the universe, called by his name, in which the earth is placed in the centre of the planetary system; a system which was adopted by astronomers for many ages, but is now totally abandoned for that of Copernicus.

There is also added an appendix, by the Doctor, containing a brief account of the ancient harmonics, according to the different sects of the authors, compared one with another; and with the music of this age: showing how and wherein the Greek music agreed or differed from ours; and how the several particulars of it are retained in ours, but differently expressed; and what in them answer to each other. Here is a short collection of most of what occurs material in the several Greek authors on this subject; as well those published by Meibomeus, as others yet remaining among us in manuscript.

II. Observations on the Dublin Bills of Mortality, 1681. And the State of that City. By the Observator on the London Bills of Mortality, Lond. 1683.

III. Johannes Godartius of Insects. Done into English and methodized; with the Addition of Notes. By Martin Lister, Esq. The Figures etched upon Copper, by Mr. F. Pl. York. Printed by J. White, for the Publisher, 1682, in quarto.

This author seems rather to have diverted himself by conversing with these animals, which he did for 40 years, than to have given himself the trouble of well understanding them. And yet after all this, says Mr. Lister, we shall find him every where very just and true in his observations. That all the insects he writes of he most industriously fed, and brought up to their change, which is more than any man ever did before him. And though the histories he has given may seem but few, for so long a time; yet in these few we have something of all the genera of insects that are in nature.

Mr. Lister has also taken great care, and at his own expence, to have all the figures finely etched upon copper plates. The whole impression intended for the more curious, consists of but 150 copies.

IV. Epistola Invitatoria, ad Observationes Magneticæ Variationis, communi Studio junctisque laboribus instituendas. Dat. Altorfi Noricorum Pridie Festi Paschalis, 1682.

Dr. Sturmius, mathematical professor at Altorf, the author of this letter, herein first takes notice of the several steps by which the doctrine of the magnet has been advanced to its present state. That its power of attracting iron has been noticed in all times; but that its verticity to the poles was first observed about 400 years since by our countryman Roger Bacon. That it gives the same virtue to steel, whence the invention of the needle between 300 and 400 years since, by the Italians: the various declination of the needle from the meridian in various places, by Sebastian Cabot: the inclination of it to the nearer pole, by our countryman Robert Norman. And that within these few years, the variation of the declination, and that in one and the same place, has also been noticed by many authors.

Vipera Caudisona Americana, or the Anatomy of a Rattle-Snake, dissected at the Repository of the Royal Society in January 1682-3. *By Edward Tyson, M. D. Coll. Med. Lond. Cand. et R. S. Soc.* N° 144, p. 25.

The observations I shall here give are such as a single subject would afford, not what might complete the history of so curious an animal. And though it were mightily to be wished that we had an accurate account, and exact anatomy of one of every distinct species of animals, yet this cannot be expected but of those that are most common; where frequent repeated dissections might fully inform us of nature's admirable contrivance and mechanism of animal bodies.

This which we dissected was sent to Mr. Henry Loades, a merchant in London, from Virginia; who was pleased not only to gratify the curiosity of the Royal Society in showing it alive, but likewise gave it them when dead, and so afforded an opportunity of farther satisfaction in observing the inward parts of it; which I find so conformable in almost all respects to those of a viper, that I have taken the liberty of placing it in that class, and (since it has not yet, that I know of, any Latin name) of giving it that of vipera caudisona;* for as I am informed by merchants it is viviparous, and the epithet sufficiently distinguishes it from those that have no rattle, although of these too there ought to be made a subdivision. But I shall concern myself at present only with the anatomy, which I think is yet given by none; though to me it seems the principal part in a natural history of animals; and for other accounts I shall refer to Marcgravius, Piso, Johnston, Nierembergius, De Laet, Hernandez, and others who have written of it; and describe it under the names of boiçininga, or boiçinininga, and boiquira, which are the Brasil names. By the Portuguese it is called cascavela and tangador; by the Dutch raëtel-schlange; by those of Mexico, teutlacocauhqui, or teuhtlacot-zauhqui, i. e. domina serpentum, and from its swift motion on the rocks like the wind, hoacoatl.

Before we look within, we shall take a short survey of its outward parts. See pl. 17. This that we dissected was 4 feet 5 inches long; the girt of the body in the largest place, which was the middle, was 6½ inches; the girt about the neck 3 inches; near the rattle 2 inches; the head flat on the top, as is the viper, and by the protuberance of the maxillæ somewhat representing the head of a bearded arrow; at the extremity of it were the nostrils, (fig. 5, a), between them and the eyes, but somewhat lower, were two other orifices (fig. 5, b), which I took for the ears, but after found they only led into a bone that had a pretty large cavity, but no perforation. Vipers have not

* The Linnæan generic name of the rattle-snake is crotalus. There are several species.

these orifices in the head; and Charas says that they hear by the nostrils; and that to them run not only the olfactory but auditory nerves also. The eye was round, about $\frac{1}{4}$ of an inch diameter; in colour, the make of the pupil, and other respects, like a viper's, as indeed, except in the rattle, was the whole external shape of this animal. There was a large scale jetting over the eye, which seemed to serve as a palpebra for defending it from any thing falling on it; but I could not perceive it was capable of closing, although inwards it seemed to have a membrana nictitans, which removes any dust that might adhere to the eye.

The scales on the head were the smallest of any; those on the back larger, and so proportionably greater to the largest part of the body; and so diminishing thence again to the setting on of the rattle; all in figure somewhat resembling parsnip seeds. Their colour various: the scales on the back had an edged rising in the middle, which was still less protuberant as they grew nearer the sides, where they were flat.

The belly seemed flat, covered with long scales of a yellowish colour, speckled black. From the neck to the anus we numbered 168; beyond the anus were two half scales; thence 19 whole scales of a black lead colour with yellowish edges; from thence to the rattle 6 orders or rows of smaller scales of the same colour.

The scales of the belly were joined to each other by distinct muscles; the lower tendon of each muscle being inserted into the upper edge of the following scale; and the other tendon of the same muscle inserted about the middle of the foregoing scale. These muscles (fig. 1, s, s, s) were more fleshy towards the middle of the scale; and then its fibres ran obliquely ascending. To each scale was appropriated a rib, whose point joined with the extreme of it, which must much advantage the use nature seems to design them for, by strengthening them to perform their reptile motions; for the scales are as so many feet, which being free and open downwards, they thereby take hold of the ground, and so contract their body forwards, and then shoot out again, and so perform their motion. Whence it is observed by Nierembergius,* that on rocks their motion is much quicker than on the earth or plains.

Since they must be always grovelling on the ground, it is a provision of nature in furnishing them with this coat of armour for their defence; which is so curiously contrived, that though it covers the whole, yet by its frequent jointings it admits of all motions. And for this too, the vertebræ of the spine seem admirably contrived; there being a round ball in the lower part of the upper

* Hist. exotic. l. 12, c. 1.—Orig.

vertebra, which enters a socket of the upper part of the lower vertebra; as the round head of the thigh-bone does the acetabulum of the os ischii, by which means it can turn itself any way.

Having placed it on its back, we opened it; and observed that the tendons of the abdominal muscles made a linea alba in the midst of the scales of the belly; where likewise ran a large blood vessel (fig. 2, r r) arising from the vena cava towards the lower part of the liver. But not to be too nice here, we proceeded to examine the viscera, concerning which I shall here give some remarks.

And first, the wind-pipe was different from that of most other animals; which usually having their cartilages annular, or at least conjoined by a membrane, form a fistula for conveying the air into the bronchia; which thence is transmitted into the small bladders of the lungs. But here (fig. 1, a a a) which is common with it to the viper kind, as soon as it enters the breast, presently meeting with the lungs, it consists only of semi-annular cartilages; which being joined at both ends to the membrane of the lungs inwardly is quite open, and immediately transmits the air to the vesiculæ of the lungs; as will better appear by (fig. 4, a a a). For, dividing the wind-pipe, we perceived it easily extended above $1\frac{1}{2}$ inch wide; whereas before it meets with the lungs the cartilages are annular. The trachea or wind-pipe was 20 inches long, terminating near the heart and beginning of the liver, and reaching to that part of the lungs which made the great bladder. The cartilages of the trachea near the beginning were $\frac{3}{8}$ of an inch, but toward the end $\frac{1}{4}$ of an inch, and lying flattish from end to end.

The use of the trachea is plain, for conveying the air into the lungs; which how considerable an organ they are nature seems to show us by the admirable contrivance and largeness of their structure. They begin from the throat, and run down 3 feet in length. The upper part (fig. 1, b) of them that lay in the forepart of the body for the length of a foot, and reached to the heart, was made of small vesiculæ or cells, like the lungs of a frog: but from the frequent branchings and checquers of the blood vessels there, appeared of a florid red. This part tapers proportionably to the body; the lowest part of it near the heart moderately blown, was in compass $5\frac{1}{2}$ inches; a little lower, for the space of 4 inches, the cells gradually disappeared; so that they seemed at last to form only a reticular compages of valvulæ conniventes on the inside of the membrane of the lungs; and the compass of the greatest place here was about $6\frac{1}{2}$ inches; but from thence to the end of the lungs was only a large bladder (c, c, c, c, c) without any cells; composed of a thin but strong transparent membrane, the compass of which blown as the former was $8\frac{1}{2}$ inches.

The lungs of the salamandra aquatica, and some other animals, are only two large bladders. In the frog, crocodile, &c. are two large lobes, filled with membranous vesiculæ or cells. Our rattle-snake, and all that family, though they have but one lobe of lungs, yet in that they comprise the two former sorts; the forepart being filled with numerous vesiculæ; the latter an entire large bladder.

In the land tortoise there are two lobes, one, on each side; but these are subdivided into several others, according to the partitions of the ribs that are fixed to the shell; and they lie chiefly in the belly, that is, the lower part of the body. But what I would remark is, that where the bronchia first enter these subdivisions it is reticular; then they form a large cavity: so that in these animals, where the nixus of respiration is not so frequent, nature provides a sufficient store-house for this (so necessary a pabulum vitæ) in these larger bladders, whence it is dispensed according to the exigency of the œconomia animalis. For the tortoise viper, rattle-snake, frogs, toads, &c. which sleep a great part of the year; as before they betake themselves to this repose, they take in their store of food; so perhaps that of air too, a more constantly requisite supply of life. For when thus stupidly asleep, and sometimes to all appearance dead; it may be questioned whether they have any motion of those parts, which is required for drawing in fresh air in inspiration. But since their life here is so imperceptible and small; this stock may be sufficient, the decay being so little. So the salamandra aquatica, that lives under water, for lungs has two large bladders, not unlikely for this reason; that it might not be forced so often to raise itself out of the water to breathe in fresh air when the former is spent and decayed.

In a viper I lately dissected, which remained alive some days after the skin and most part of the viscera were separated, I observed the lungs all this while not rising and falling, as in inspiration and expiration, but constant, equally distended with air; that as soon as it died, it expired, and they fell. But the stomach was empty, and I doubt not was so some considerable time before; as was the rattle-snakes,* which for 4 months at least had eaten nothing: so that although they can live so long without food, yet nature is mighty provident in supplying them with air, in bestowing on them such large receptacles for receiving it.

We shall now take notice of those parts that are for receiving the food; and first of the œsophagus, or gula, which serves for the transmitting it into the stomach; and indeed this seems the only use of this part in most other animals; but here nature may be thought to intend it for something more, and to make use of it upon occasion as a stomach, or stomachs too; for upon blowing up this part, I observed two large swellings as represented in fig. 1. d, f;

* Narrant multi, qui eum serpentem domi alere solent atque educare, annum integrum durare absque cibo ullo potuque. Nieremberg. Hist. Nat. l. 12, cap. 1.—Orig.

nor was the true stomach capable of that extension as these were. The whole length of the œsophagus was 2 feet 3½ inches; the length of the proper stomach (g) 5 inches, lying in a straight line with the œsophagus, but thicker than it, having a remarkable coat more on the inside, easily distinguishable by its colour, substance and plicæ, and jetting over the inside of the gullet; and in all respects as in the viper. From the pylorus the duct (fig. 2. g,) straight again for 1½ inch; and then formed a large intestine, (e, e, e,) which afforded a pleasant sight, by the weaved rugæ of its inward coat; which gut after some small windings, ended at last in the rectum, (f, f, f,) whose capacity was much less than the former. In the stomach and guts I observed abundance of lumbrici teretes, which is a disease vipers likewise are subject to. The whole length from the throat to the anus is but one continued ductus; though often variously distinguished, according to nature's different intention in the several species of animals.

But since in that promiscuous food they take in, which they swallow always whole, there are often some parts unfit to be digested, and therefore to be returned again; (the gullet here being very long, and upon that account incommodious for this action;) nature has provided these swellings in it, where they may be respited, till recovering its force, it gives them another lift, and upon a third effort at last wholly ejects them. And if what is confidently reported by many, be true, that on occasion of danger they receive their young into their mouths, these are fit places for receiving them.*

The food before it can prove aliment, must be comminuted, and broken into the smallest particles; which in these membranous stomachs, I cannot see how it can be performed, but by corrosion. A principal menstruum in doing this, I take to be that liquor, which is discharged by the glands that are seated in some at the beginning of the throat, and are called salival, or just above the stomach or gizzard of birds, and called the echinus, or in others in the stomach itself, and called the glandulous coat; and such I take the inward coat of the stomach of our rattle-snake to be.†

When comminuted it is discharged into the guts; which, that the chyle might not pass off with the fæces, are often convoluted, or winding, as here, (fig. 2.

* Of this we have the attestation of Mons. Beauvois, who, (in the 4th vol. of the American Philosophical Transactions) declares himself an eye-witness of the process, having seen a large female rattle-snake, which he happened to disturb in his walks, open her mouth and receive 5 young ones down her throat, which in about a quarter of an hour she again discharged: on his approaching a second time, the same circumstance again took place, and the snake made its escape.

† Modern physiologists have shown that this conjecture respecting the agency of a solvent liquor or menstruum (the gastric juice) in the process of digestion, is well founded.

e, e, e,); that so by impeding a too quick descent of it this way, or by valves, a separation may the better be made; and then the fæces, as useless, cannot quicker be discharged than by the rectum; which where the fæces are hard, is furnished with a stronger muscle the better to help its action; and such seemed the rectum here; and the fæces harder than usual in vipers.

So that the whole ductus alimentalis, from its uses, may ordinarily be divided into 4 parts. 1. That which conveys the food, the œsophagus. 2. That which digests or corrodes it, the stomach. 3. That which distributes the chyle, the intestines. 4. That which empties the fæces, the rectum. But a leech is all stomach, from one end to the other, and devours at a meal several times the weight of its whole body: the stomach when swelled and stretched with blood is far bigger than the leech itself; nay, several times exceeds it. But I mistook the number, it was not one, but many stomachs; for the cavity is divided by several transverse membranes, into divers distinct cameras; but these membranes in the middle have a hole that leads from one into the other: but by the pouching out of each side, each of these may be reckoned also two; in all we may number, (there being 10 or 11 of these cameras, besides those 2 long ones which at last run to the tail) at least 22, if not 24 stomachs, but the rectum which lies between the forking of the 2 last long sacculi, or stomachs, is but small, and short in respect of the whole; but of this perhaps more in my anatomy of this animal.

The heart, (fig. 1. k,) was placed near the bottom of the trachea, on the right side of it. The length of it was 1½ inch, its figure rather flat than round; encompassed with a pericardium, and the auricle (l) larger than the heart itself. It had but one ventricle, the valves small, and fleshy: and the inside of the ventricle distinguished by 4 or 5 cross furrows. Why Charas should make the heart of the viper to have two ventricles, I see no reason; I should much more easily allow a double auricle, one at the entrance of the vena cava, of which there are two branches (n, n, n,) descending and one ascending; the other for the arteria aorta, which has two ascending and one descending branch (m, m, m,) as in the figure.

A little below the heart lies the liver (o, o,); which was about an inch wide in the largest place, and seemed divided on one side by the vena cava into two lobes of an unequal length; for that on the left side was about 10 inches, and that on the right side about a foot long; its colour a brown red, and its use no doubt for the separating the gall that was contained in a bladder (p) seated at some distance below. The gall-bladder here was 2 inches long, the colour of the gall contained in it a grass-green, which sweating through its coats had deeply tinged all the adjacent parts. The ductus cysticus, by which it empties itself into the intestine, arises from the top of the bladder; so gently descending passes through that part which Charas takes for the pancreas (d); but which the ancients called the spleen, and so enters the beginning of the large intestine.

Now I shall describe the kidneys. But I must first observe the fat which was very plentiful, and is said by Nardus Anton. Recchus,* to be used by the physicians of Mexico with good success, in the sciatica and all pains of the limbs, and for discussing preternatural tumors. The membrane it adhered to, I take for the omentum; which encompassed all parts contained in this lower belly; and was joined to both sides of the ribs, so running to the rectum, and forming a bag that enveloped the parts here, but was free, and not conjoined towards the belly. The lower belly I call it to distinguish it from the rest of the trunk, for the whole was but one continued cavity; there being no partition of it by any diaphragm; and I have represented the parts contained here, in my 2d figure, as the others are in the 1st; but proportionably much larger, as appears by the descriptions.

The two kidneys were about 7 inches long, and about ½ inch broad each; though one continued body, yet plainly distinguishable into several smaller kidneys, as I remember in one I numbered 15, but also very curiously contrived, and with so much beauty, that I want words to express what the pencil could not imitate, much less can be represented in a print. I shall therefore in my description, the better to help out and illustrate my meaning, have a constant reference to the figures; which being anxious to make as well as might be, I spent so much time, that I had not an opportunity of satisfying my curiosity in all respects (the parts drying) as I desired; but I observed, as likewise several others who viewed them, when first taken out of the body, that the whole seemed a delicate compages of vessels, and the intermixture of those of the blood with those other white ones, that are the secretory, composed most regularly formed bodies. In my figures, that on the left side (fig. 2,) represents the upper surface of the kidney, which appears first in the dissection; the other, the lower side which lies to the back; in both there are two large blood vessels running down each side; one marked (n, n, n, fig. 2.) the other, where the vas deferens runs; but is not here represented; and from these arise several lesser branches (o, o, o, fig. 2.) at set distances, which curiously spreading themselves form as it were ramifications of trees. As many as there were of these emulgent vessels (for so I take them to be) so many kidneys were in each; the interstices (p, p, p, fig. 2.) of these blood vessels were filled up with other white ones; which I doubt not are for the secretion of the urine, and on this side appeared

* Rerum Med. Novæ Hispan. Hist. lib. 9. c. 17. p. 328.—Orig.

more numerous than on the other; but it is impossible to represent the curious interweavings of both; but here in the under side of the right kidney in some places they appeared more distinct; for (QQ, fig. 2.) shows the large blood vessel, whence arise the emulgents (r, r, r, fig. 2.) which spreading themselves very thick into the bodies (s, s, s, fig. 2.), make them appear all bloody, between which for a little space there appears a small body of the white secretory vessels (t, t, t, fig. 2.)

The ureters ran almost the length of the kidneys: being a common trunk that received the lesser branches that went to each single gland (it is in part represented by the letters (v. v. fig. 2.), and both terminated near each other in the cloaca, making a rising there; for our rattle-snake, like birds, had a cloaca, which in the female viper receives the orifices of the ureters, and the two uteri; and in part it may be said that of the rectum too, which yet had a convenient valve that covered it.

Near the verge of the cloaca, we observed two other orifices which seemed covered by the folding of the skin, and these led into those two bags (fig. 2, m, m,) which I have taken the liberty to call the scent-bags. Charas is much mistaken, who supposes them to be the parastatæ or conservatories of the seed, as likewise those he would refute that would have them to be other testicles: and I the more wonder at this his mistake, since he could not but have observed them, as I have, in the female vipers too; which sufficiently shows his error. One of them was about an inch long, and as big as a goose quill, but taper towards the end, and from the colour of the liquor it contained, appeared darkish; the other bag was something less, and its colour as in the viper; this difference I suppose may be accidental: the liquor included in them was something crass, and of a strong and very unpleasant smell; such, but in a more intense degree, as the animal emitted before dissection; which Martial likewise takes notice of, having placed it in the last but one in his catalogue of stinks. L. 4. Epigr. 4. And De Laet * makes mention of some snakes in the West Indies, that stink worse than any fox or pole-cat.

We now come to the organs of generation: and I find that Charas is as unhappy in the description of some of them, as he was in his conjecture about that part we call the scent-bag. We shall begin with that, wherein the seed is first made, the testes, (fig. 2. h, h,) which are very unproportionate in length; the right being 2¼ inches long, the left but 1¼ inch long, scarcely so big in compass as a goose-quill. The unequal length of this part Charas takes notice of in vipers. I shall add, that the ovarium of the female viper is the same; for that

* Hist. Indiæ Occident. l. 15. c. 6. p. 555.—Orig.

of one side was as big again as the other. The colour of the testes was white, as is usual, and so was their substance. I took notice of the vasa præparantia, which had nothing uncommon: but the deferentia (i, i,) were remarkable; for though they ran in a straight line almost from the testes to the penis, and formed no large body, yet this duct was so often involved, that were it unravelled and extended its whole length, it would be twice as long: which made me think, that it was only the extension of the epidydymis, for the whole testis is but a congeries of curiously convoluted vessels which terminate in the epidydymis, whose continuation makes the deferens: and where its convolutions are many upon the body of the testis itself, there the deferens is an even duct; but as in our subject it making no such body there, or but a very small one, in its passage downwards it was every where crimpled, and about the middle of the kidneys often convoluted, which is represented in our figures.

Where they emptied themselves I could not so well observe in the rattle-snake, since the parts which I had laid out for making the scheme soon dried, before I had an opportunity of nicely examining them. But since upon the dissection of a viper I found that they (fig. 3. a) were continued along the penis single, where the penis was so; and afterwards divided, and ran to the end of each. Nor were there any vesiculæ seminales or prostatæ here to receive them; and a reason for it, I shall allege when I have described the penes (fig. 2. K), which here were very remarkable, not only for their structure, but number likewise; there being 4 in all, two on each side, which lay sheathed in the body, that upon first opening it they were not to be perceived, but only the large orifices where they were drawn in as a finger of a glove may be by a thread fastened to the end. But having protruded them by a probe, they appeared as is represented in (fig. 2.) And I observed that towards the basis, or root, they were single of each side, and that here they were thick beset with prickles, whose points looked backwards, and were very sharp; and seemed, especially when dry, like the substance of the bristles of a hedge-hog: but hence they were divided, and formed two round bodies, of the size of a small goose quill, about $\frac{3}{4}$ of an inch long of a red colour; but the whole, as protruded, was above an inch long. When protruded I found they could easily be retracted, and drawn in by the help of large muscles, (l, l,) that were fastened to them and ran along under, and were at last inserted at the end of the tail at the setting on of the first rattle; which upon the trial was so plain that we need not doubt of the use of them, and I shall therefore call them retractores penum. But Charas seems to mistake them in vipers, for the penes themselves; which he describes to have their origin from the extremity of the tail; as does Baldus Angelus Abbatius,*

* De Viperæ natur. et facultat. cap. 19, pag. mihi 60.—Orig.

Aldrovandus*, and others, who perhaps misled him in the account of these parts. Nor as to the other extremity are they more in the right, which by their picture and description, they make to be altogether single, and covered and quite beset with prickles like the skin of a porcupine. Whereas this part in vipers too, as well as in the rattle-snake, divides and forms two large round bodies, or two distinct penes. And this Baldus, or rather Camentius, who made the dissection for him, seems to have observed where he says, Quando turgidi fiunt, aut extra violenter emittantur, uti sæpe apud Paulum vidimus, pene hanc formam referre Y aspectu aspero ut Erinaceus. For in vipers they are hispid to the end; but not in the rattle-snake, as is plainly represented in the figures of both.

There are several animals which have no penis at all, but vasa deferentia, as most fishes. The rattle-snake and that family have these organs of generation the most numerous of any I have hitherto met with. But why the male rattle-snake, or the male viper, should have 4 penes, when the female has but 2 uteri for receiving them, seems a difficulty to me. Amongst many conjectures I have had about it, what seems the most to satisfy me, is this: that they have the penis here on each side double, or forked, that so being entered the uteri, by spreading themselves like the pythagorean Y, they may the better and more firmly be retained there till they have performed their duty. And this too seems one use of the aculei or bristles towards the root of them; for having their points looking backwards when once they have entered the pudendum, they must needs lock them in, and retain them there, till such time as the parts being tired, and subsiding, have leave to retreat. For in animals which have no vesiculæ seminales, it is requisite that the coitus be long, that so the seed which cannot quickly, may leisurely be transmitted from the testes: but where it is beforehand stored up in the vesiculæ, there the coitus is soon over; but when they must expect the generation, or at least a sluggish descent of it, nature makes provision for the more convenient performing it. So in dogs, which have no vesiculæ seminales, near the root of the bony penis, there is a large body made up of an abundance of cells and vessels, which upon the rushing in of the blood and spirits, is so mightily extended and swelled, that it forcibly keeps him in, until such time as the impetus be over, and the part subsides. Therefore in the rattle-snake, (where, as we have observed, there are no vesiculæ, and where the vas deferens is all along crimpled and winding, and so upon both accounts must be thought to be long in coition) the contrivance and structure of these parts seem very requisite. For although in this action they twist their

* Aldrovandus de Serpent. et Dracon.—Orig.

bodies, which may be some advantage too, yet not sufficient alone; for otherwise upon a little occasion the parts would be apt to slip out, which now they cannot, being forked, and hooked in too by the aculei or bristles. But the deferentia being continued to the end of the penes do likewise show this must be the use of them. But that the female may receive no injury by these spines, nature has made that part of the uteri which they enter strong and gristly; as we observed in a viper: and that the male too might not be harmed by an over extension of these parts, those strong muscles which serve for retracting and drawing them in, do likewise secure them in this respect too. It may be likewise considered, since they are naturally so cold and frigid, whether these aculei may not serve to incite them, and stir them up. From these parts we pass to the poisonous teeth.

But first I shall remark something of the other parts in the mouth: as the tongue, the larynx, and the smaller teeth.

The tongue (fig. 5, g,) was in all respects like that of the viper, being composed of two long round bodies, contiguous and joined together from the root $\frac{2}{3}$ of its length; which with great agility the animal could dart out, and retract again. The use it is designed for, I suspect with Charas, to be in part for catching flies and other insects.

Over the tongue (fig. 5, f,) lay the larynx; not formed with that variety of cartilages as is usual in other animals; but so as to make a rime or slit for receiving or conveying out the air: nor was there any epiglottis for preventing other bodies from slipping in; this being sufficiently provided for, by the strict closure of them: and the air passing through only such a slit, without the contrivance of other parts for modulating it, can only make such a sound as we observe in their hissing.

The teeth are of 2 sorts, (fig. 5, c, c, h.) 1. The smaller, which are seated in each jaw, and serve for the catching and retaining the food. 2. The poisonous fangs (fig. 5, d, d, fig. 6, h, fig. 7.) which kill it, and are placed without the upper jaw, are all canini or apprehensores; for since they do not chew or bruise their food, but swallow all whole as they meet with it, there is no need of molares.

Of the first sort of teeth: in the lower jaw there are 2 rows on each side, 5 in a row, the inward smaller than the outward, so that there are here 20 in all; in the upper jaw there are but 16, 5 on each side, placed backwards, and 6 before. These do no harm, which was known to mountebanks (as Cæsalpinus and others observe) formerly; who to give a proof of the force of their antidotes, would suffer themselves to be bitten by vipers, but first took care to spoil them of their fangs.

These fangs are placed without the upper jaws, towards the forepart of the mouth, not fastened to the maxilla, as the other teeth; but the 2 outmost and largest fangs (fig. 6, g.) were fixed to that bone (f.), which if any, may be thought to be the ear-bone. The other fangs I could not perceive were fastened to any bone, but to muscles or tendons there. These fangs or larger teeth were not to be perceived upon first opening the mouth, they lying couched under a strong membrane or sheath; but so as to make a large rising there on the outside of the smaller teeth of the maxilla; but at pleasure when alive they could raise them to do execution with, not unlike as a lion or a cat does its claws. These teeth were hooked and bent like the teeth of a barbarossa; but some of the smaller of them (fig. 7) were bent at right angles, but their shape and size will be best understood by the figures we have made of them. On each side we met with about 6 or 7 not altogether placed so exact as is represented in the head in fig. 5; which was done to show them more distinctly. For the second tooth, upon raising it, lies more on the side of the first; and the other being fastened only to muscles or tendons which are flexible, it is difficult to assign them their posture. In all these teeth, especially the larger, we took notice of a pretty large foramen or hole towards the root of it, and towards the point there was a plain visible and large slit, like the cut of a pen sloping, and that part from the slit to the root was perfectly hollow; which first of all was discovered to us by pressing gently with our finger the side of the gum; for then we perceived that the poison readily arose through the hollow of the tooth, and issued out of the slit. This we tried several times, which trials spoiled our inquiry into the bags and glands that furnish them with that liquor. But our defect therein may well be supplied with what Charas and Redi* have written of the same parts in vipers.† This poisonous liquor I observed to be of a water-colour, lightly tinged yellow: perhaps in some it may occasionally be deeper, and this, it may be, has given rise to that fond opinion of those who have imagined that it was transmitted by a vessel from the gall-bladder. Indeed scarcely any subject in philosophy has admitted more controversies than this of the poison of vipers, in what it consists, what it is, and how it produces its dire effects. Severinus, in his Vipera Pythia, has made a large collection of them, and who so please may there satisfy their curiosity about it.

I now proceed to describe the skeleton: and first of the bones of the head. I observed that the cranium (fig. 6, a) here was entire, without sutures, as re-

* Add also Mead and Fontana.

† See vol. i. p. 58, of this Abridgement. And respecting the controversy between Redi and Charas, see ibid. pp. 411 and 654.

presented in our figure; only where some other bones were joined to them, as forwards over the nostrils, were 2 small bones, (cc) to which were fastened the cartilages (d) or rather bones which divide the nose. The other bones seemed admirably contrived for the great extension and widening of the maxillæ, which seems a great provision of nature; for since it must swallow all things whole, and its head is but small, without this most mechanical contrivance it were impossible to do it. The upper jaw forward was joined to the bone that receives the poisonous fang, and which had a large cavity in it, which opened outward, and was thought to be the foramen of the ear, (fig. 5, b) but inwards we observed no perforation for a nerve, unless there might be one that comes to it under that bone (fig. 6, ee) which conjoins it to the cranium. This articulation seems advantageous, both for the motion of the fang, which lies sometimes couched, sometimes erected, as the jaw too; but its principal and most remarkable advantage for swallowing large bodies, is the curious articulation of the maxillæ backwards to the cranium by 2 bones, which from their use (since we know no name to distinguish them by) we shall call maxillarum dilatores. Their shape, size, and aptness for this motion, will readily enough be conceived by the eye in observing the figure (fig. 6, no). For the lower jaw being not conjoined at the mentum, as is usual in other animals, but parted at a good distance; upon the receiving a large body, as the membrane here to which they are fastened easily extends, so by lifting up, as also by bringing these two bones more to a straight line, it must needs considerably widen the rictus of the mouth, and for this cause too they are made 2, not 1, for performing this motion more easily. This articulation (m) of the dilatores (which is very curious) with the upper and lower jaw, makes those protuberances of the head, which we likened to that of a bearded arrow, as does Hor. lib. iii. Od. xxvii.

The lower jaw of each side was composed of 2 bones, as appears in the figure, but firmly conjoined. The fore bone was for receiving the small teeth, the hinder towards the articulation grew broad, as likewise did the bone of the upper jaw answerable to this place in the lower. But this upper jaw towards the poisonous fang divided into 2 bones, one was fastened to the bone of the poisonous fang outwards, the other, which received the small teeth, was inserted into the same bone more inwards.

The vertebræ, according to the whole figure of the body, were smallest towards both extremes, and largest in the middle. From the neck to the anus there were as many observed scales on the belly, viz. 168, but from the anus to the setting on of the rattle 29 more in number than the scales. The former vertebræ had a flat upright spine (fig. 8, a) towards the back, and a slender round oblique descending one (b) inwards to the belly. To each vertebra, besides those spines just mentioned, there were other processuses (c d) for the advantage of setting on of the ribs, and the articulation with each other; but what was most remarkable is (what I have already hinted) that round ball (e) in the lower part of the upper vertebræ, which enters a socket of the upper part of the lower vertebræ, like as the head of the os femoris does the acetabulum of the os ischii; by which contrivance, as also the articulation with each other, they have that free motion of winding their bodies any ways. The ribs in the neck were small, but larger towards the middle of the body, where they were about 2 inches long; but towards the tail they grew less and shorter again, and did all terminate at the beginning of the scales of the belly. In the vertebræ of the tail inwards there were 2 spines, (fig. 9, bb) whereas in the other vertebræ there was but 1; as likewise there were here transverse slender processuses (cc) something analogous to ribs.

To the last vertebra of the tail was fastened the rattle (fig. 11, 12); in our subject there were but 5, but some others seemed to be broken off. That next the tail was of a lead-colour, the others of a cineritious. It is well described by (Mus. Reg. Soc. p. 51) Dr. Grew, who says, "They are very hollow, thin, hard, and dry bones, and therefore very brittle, almost like glass, and very sonorous. They are all very nearly of the same bulk, and of the self-same figure, most like the os sacrum of a man; for although the last of them only seems to have a rigid tail, or epiphysis adjoined to it, yet have every one of them the like: so that the tail of every uppermost bone runs within two of the bones below it: by which artifice they have not only a moveable coherence, but also make a more multiplied sound, each bone hitting against two others at the same time."

The use of this rattle (since I know no other) I shall give in the words of Piso, who tells us, Huic tam pernicioso colubro benigna natura cautionis quasi gratiâ crepitaculum addidisse videtur, ut illius sonitu admonitus quilibet homo non solum, sed et qualecunque pecus, vel jumentum, tempestivè sibi caveat à vicino hoste. Both he and Nierembergius and others assert, that every year there is an addition of a new rattle, which Dr. Grew suspects, for then they must live 16 years, for so many joints there are observed in some in our repository. I have been told in some there have been above 20. These rattles are placed with their broadest part perpendicular to the body, not horizontal. And the one is fastened to the last vertebra of the tail by means of a thick muscle under it, and by the membranes that conjoin it to the skin.* I have not given the figure of the whole skeleton, since what is wanting may be sufficiently understood by the description, and whoso pleases may view the skeleton

* Piso de Indiæ utriusque re Nat. et Med. l. 5, c. 2, p. 274.—Orig.

(fig. x, b) itself in the repository of the Royal Society, very curiously prepared by that ingenious young gentleman Richard Waller, Esq. a worthy member of the society; whose great assistance to me I must here gratefully acknowledge, as to the designs, and otherwise; his curious pencil illustrating what my pen was often less able to describe.

The Explanation of the Figures.

Fig. 1. represents that part of the body opened, which contains the lungs, the heart, the gullet, stomach, &c.—aaa The arteria aspera, or windpipe; B the upper part of the lungs, which is vesiculous; ccccc the lower part of the lungs, which makes a large bladder; d the first swelling œsophagus, or false stomach; eee the œsophagus or gullet, and that part of it where it is straighter; f the second swelling of the œsophagus, or second false stomach; g the true stomach; h a short straightening of the gut, a little below the pylorus; i the intestines; k the heart; l the auricle; mmm three arteries, whereof there are two ascending and one descending; nnn three large veins, whereof two are descending and the third ascending, which last seems to divide the liver into two lobes; oo the liver; p the gall-bladder; q the spleen, as it is called by the ancients, but by Charas the pancreas; rrr a large blood-vessel that runs in the midst of the scales of the belly; ss the muscles belonging to the scales of the belly.

Fig. 2. shows those parts that are contained in the lower part of the body.—a the intestines cut off just below the pylorus; b the gall-bladder; c the ductus bilarius, that passes through the middle of the spleen, or as called by Charas, the pancreas, and enters the large gut; d the spleen, or pancreas; ee the intestines which was very large and winding but short; ff the rectum; g the anus; hh the testes; iiii the vasa deferentia; kk the penes on each side, which first at the root are conjoined and are thick beset with bristles; ll the muscles that serve for drawing in the penes; mm the scent-bags; nn a large blood-vessel that runs on one side of the left kidney; ooo the emulgents that arise from the same; ppp the secretory vessels; qq the large blood-vessels of the right kidney; rrr the emulgents arising from it; sss a round body of blood-vessels; ttt secretory vessels; uu the ureters.

Fig. 3. represents the penes of one side of a viper.—a the vas deferens, which afterwards divides and runs to the end of the penes; b the penes; c the muscles which retract the penes.

Fig. 4. represents part of the lungs opened by the trachea.—aaaa the arteria aspera, divided in the middle; bbb some larger branches of blood-vessels; ccc the vesiculæ, or cells of the lungs.

Fig. 5. exhibits the head of the rattle-snake, with its mouth opened to show his teeth, and other parts there.—a the hole of the nostril; b the foramen which leads to a large cavity, which has no perforation for any nerve inwards, but yet it is thought to be for hearing; cc the small teeth in the upper jaw; dd the large fangs, or poisonous teeth; eee the place where the bladders of poison lay; f the larynx; g the forked tongue; h the teeth in the lower jaw; i the place where the lower jaw is divided at the mentum.

Fig. 6. represents the scull.—a the cranium without any sutures; bb the orbits of the eyes; cc two small bones over the nose; d the gristly or rather bony sepimentum of the nose; ee a small bone that lies between the cranium and that bone in which is fixed the poisonous fang; ff a cavity in that bone to which is fastened the poisonous fang, whose outward orifice is represented in the fifth figure by the letter (b) and is thought to be the ear; g the large poisonous fang which is fastened to the ear-bone; h the other poisonous teeth, which are not fixed in the bone but to muscles; ii the upper maxilla, which contains the small teeth; kk one side of the lower maxilla, with its double row of teeth, which in the middle seems to be joined by a suture; l the distance at the mentum, between the two sides of the lower maxilla or jaw; mm where the two maxillæ are joined together backwards, and by a tendon are fastened to another bone, which from its use, and for distinction sake, we call dilatores maxillarum; nn the dilatores of the jaws; oo a short bone which joins the dilators to the scull or cranium; p the vertebræ of the neck.

Fig. 7. represents the poisonous teeth.

Fig. 8. shows one of the vertebræ of the back.—a the outward spine of the vertebra, which is flat longways; b the inward spine of the vertebra, which is round; c a large flat processus for the articulation of the vertebra; d small transverse processuses for the setting on the ribs; e a round ball, like the head of the os femoris, which enters a socket of the lower vertebra, as that does the acetabulum of the os ischii.

Fig. 9. shows one of the vertebræ of the tail.—a the spine towards the back; bb the two inward spines; cc the transverse spines, analogous to ribs.

Fig. 10. represents the vertebræ of the tail and the musculous flesh which fastens the first rattle.—a the vertebræ; b the muscle on which is fastened the rattle.

Fig. 11. exhibits a single rattle, which has three joints: the first and largest appears when conjoined with others, the two other serve for the fastening on the succeeding rattle, and are covered by them.

Fig. 12. shows the five rattles as joined together.

Martini Lister è S. R. Lond. de Fontibus Medicatis Angliæ Exercitatio nova et prior. Eboraci. 1682, *in 8vo.* N° 144, p. 59.

The most rational way, in the opinion of this author, of ascertaining what the saline contents of the several waters in England are, is to crystallize them.

This crystallization is to be done with great care and accuracy, not at once in a lump, as it seems most have been satisfied with, but after many experiments, ablutions, dissolutions, and shootings, till you have the whole mass of salt fairly and singly crystallized.

Then to compare these crystals with the crystals of all the known fossile salts, to which end these known salts also are to be exactly described from their fairest crystals.* All which he has carefully done, described, and figured.

Joh. Alphonsi Borelli Neapol. Math. Profes. Opus Posthumum: Pars prima, Romæ 1680; *pars altera, ibidem,* 1681. N° 144, p. 62.

In this work the ingenious author first gives an exact description of a muscle, which within its tendinous or nervous membrane contains several small bundles of fibres, which constitute an hexagonal square, or triangular prism; the fibres themselves in each prism being parallel, and variously connected to each other; the microscopical appearance of a single fibre representing a cylinder, not hollow like a reed, but full of a spongy pith like elder. He gives an account of the several species of muscles, from the position of their fibres, and asserts their proper action to be contraction; adding a modest but solid censure of Steno's structure of a muscle, and manner of its operation. He confutes the common opinion, that nature with a very small force lifts up the greatest weights, the contrary being demonstrated, that the power does 100 or 1000 times exceed the weight of the limbs that are lifted up by it.

He gives us likewise an account of the wonderful structure of the back-bone, to the cartilages of which he attributes a greater force than to all the muscles that contract it, as is evident from this proposition: that if a porter carry on his back a weight of 120 ℔, the power nature exercises by the cartilages of the vertebræ, and the musculi extensores of the back, is equal to the force of 25,585 ℔, that of the muscles alone he computes to be 6404 ℔, and observes

* The method here proposed of ascertaining the saline contents of mineral waters is by no means satisfactory. The nature and proportions of the saline and other ingredients in such waters can only be accurately determined by chemical analysis, conducted on the principles laid down, and the examples given by Bergman, Kirwan, Westrumb, and others.

that the retention of a joint stretched out is not from the tonical action of antagonist muscles.

Hence he goes on to deliver the various postures of an animal, which he does by assigning his centre of gravity in all his possible positions. As in a man stretched out at length, the centre is between the nates and pubes. That a man cannot well stand on one heel, or the tip of a toe, because in these cases the line of direction falls without his basis, &c.

That though birds have 2 feet, yet they neither walk nor stand the same way as a man; which depends on the different structure of their joints. For, 1. they differ in the number of the bones. 2. In the form. 3. In the distribution and make of their muscles. 4. In the joints themselves.

He demonstrates the manner how a bird when sleeping sits firm on a twig, though the muscles are then inactive; namely, by a strong constriction of its claws, and consequently a firm comprehension of that twig, necessarily and mechanically resulting from the gravity of the bird, and the shortness of the tendons of those muscles that contract the claws.

That quadrupeds cannot stand in their natural prone position on 1 or 2 feet, because the centre of gravity and its line of propension cannot fall in either, or between both.

He shows the art of scating upon ice, as also how progression in quadrupeds is performed, and likewise leaping, in which the vis motiva is to the weight of the body as 2,900 is to 1. That in leaping according to a line inclined to the horizon, at oblique angles, the line described by the centre of gravity shall be a curve parabola, as being compounded of the straight uniform motion forward, and the accelerated descent of the heavy body. Next he gives an exact account of flying, the main stress of which is in the largeness of the muscles that move the wings, the potentia of which exceeds the weight of the bird 10,000 times; with many more curious particulars about their several ways of flying.

He describes the action of swimming, and how fishes change their specified gravity on occasion, by the compression and dilatation of the air contained in their air-bladders performed by the many and strong muscles about their bellies. He assigns the reason why man does not swim by instinct as well as other animals, to be chiefly on account of the gravity of the head so much exceeding the proportion of that of the rest of the body.

The several ways to live and move under water were described before, as the diving bell, the leathern cylinder, &c. but that which he seems most to insist on, is of a brass or copper vesica, about 2 feet diameter, to contain the diver's head, this to be fastened to a goat's-skin habit fitted exactly to the shape of the body. He contrives a circulation for the air by pipes within the vesica, and bestows on him an air-pump by his side, by which he may make himself heavier or lighter, in imitation of the engines nature has given to fish for that use. By this means he avoids the objections the others are liable to, particularly that of the air, the moisture with which it is clogged in expiration, and by it made unfit for the same use again, being here taken from it by its circulation through the pipes, to the sides of which it adheres, and leaves the air as untainted as before.

He concludes this first part with a description of the diving ship, the motion of which he conceives would be much facilitated by one single oar in the poop, which should be flexible, and made with a spring, from the vibration of which the ship should be impelled as fishes are by their tails.

In the second part the author describes the mechanical mode, and assigns the immediate cause, by which the contraction of the muscles is performed.

Concluding that the muscles are contracted from the inflation of their fibres by adventitious bodies, as it were by wedges; and having refused an incorporeal natural faculty for the immediate mover, as also any aërial substance, and rejected the blood filling the pores of the muscles, together with the manner by which moistened ropes are contracted, he infers, that the ebullition, caused in the muscles by the concurrence of the blood and succus nerveus, is the immediate cause of the intumescence and contraction, which he confirms and illustrates by arguments and experiments.

He next gives an account of the internal motions of the fluids of the body, as of the circulation of the blood. Describing the muscular structure of the heart, and showing how it differs from other muscles by the wonderful texture of its fibres.

He at last infers that the moving faculty of the heart exceeds the resistance of the whole blood in the arteries, and of the ligaments that hinder their dilatation, which is greater than the force of a weight of 180,000.

He ascribes respiration wholly to the muscles that enlarge the thorax, viz. the intercostals and the diaphragm, together with the weight and elasticity of the air. The manner, by drawing up the circumference of the ribs towards the throat, by directions that make acute angles with the planes of the ribs. The structure of the thorax in the tortoise, he observes, is remarkable, there being no divided ribs, but one continued bony arch, and no diaphragm; and instead of lungs, 2 long bladders, containing also the blood-vessels. These bags are not alternately filled and emptied, but constantly remain full of air, which is not renewed in them but partially, by the external muscles that stick to the skin, which when inactive make a hollow sinus, but contracted a plane.

Then follow some observations on the nutrition of plants, on the use of the liver, on the torpedo, on the vulgar error of the porcupine darting its quills, and lastly on the causes and cure of fevers.

An Account of a Roman Monument found near South Shields, at the Mouth of the River Tyne. By Martin Lister, Esq. N° 145, p. 70.

Description of a mutilated supposed Roman altar; the letters mostly defaced, and illegible.

An Abstract of a Letter from Mr. Anthony Lewenhoeck written to Sir C. W. Jan. 22, 1682-3, from Delft. N° 145, p. 74.

Having lately met with a book published by a physician of our country, which treats of human generation, and the egg-branch (or ovarium) as it is found in women-kind; and not doubting but what is there said is also applicable to quadrupeds, I examined egg-branches of several lambs of a year old, that had been several months in the winter kept in a stall for fatting, separated from the rams. From what I have hitherto found I cannot but wonder why it should be generally believed, that the tuba fallopiana does draw, or suck down an egg from the egg-branch, through so narrow a passage as it has; considering also that some of the eggs were as large as pease, and others as large as the whole egg-branch : that they were made up of glandular parts interwoven with blood-vessels, and were shut up so fast in their skins or membranes that I could not with my nails tear one of them from the egg-branch : that some of them consisted of very irregular and unlike parts, which were in some places inclosed in particular skins, and had not at all the shape of an egg : that some of them which stood out beyond the rest were burst open ; and yet when I went to pull them off, they stuck so fast that the whole egg-branch came along with them. The smallest eggs, and of a lesser size, were also firmly rooted and fixed in their skins, and had often a watery substance in them, that besides the supposed eggs of the egg-branch, there were others lying at a distance from it of an inch and more on each side of the womb, and were included in particular skins.

My opinion therefore of these eggs is, that they are emunctories, or the emptyings of some vessels lying near, such as are often found among the membranes, or adhering to the bowels of animals. But as to generation, though I have formerly been very reserved in declaring my thoughts of it, yet being now further instructed by manifold experience, I dare venture to affirm it rather to come from an animalcule, such as I find not only in human seed, but that of all birds, beasts, fishes, and insects, than an egg. And the rather for that I find in the seed of a man, as also of a dog, two different sorts of animalcules, answering the different sexes of male and female.*

I know some men will even swear that they have found the aforesaid eggs in the tuba fallopiana of beasts. But I need not believe that these round bodies they have seen in it should be drawn down from the imagined egg-branch, through the long and very narrow passage of the tuba fallopiana, because some of the bodies are as large as a pea, nay as the whole egg-branch; and of a very firm and compacted substance: but the way through which they should pass is no wider than the compass of a small pin. Again if it were so as is said, these bodies would be found, not by chance, but always when searched for immediately after copulation; but that is so far from being true, that it is hardly to be imagined, if we consider how little time is taken up in the copulation of several animals, as a cow, rabbit, &c. In which so short time nevertheless ought to be drawn down through a long and narrow passage, a great number of bodies; in some cases 2 or 3, in others 6 or 8, and more, according to the number of fœtuses to be produced.

But supposing such bodies there to be found, why may they not be formed ex residuo seminis masculi, gathered together into a ball or globule; as we see several other substances in animals that are neither of too thick nor too thin a consistence, as fat, sanies, &c. Or secondly, there being no part of the body which is not nourished, and which does not cast off some things that are superfluous, why may there not in the womb or tuba be several excretions made, which by compression on all sides may be brought into a round figure? This supposal being true, it will follow that egg-like bodies are also in the womb or tuba of females that have not been accompanied with the male.

It may be queried, if one animalcule of seed be sufficient to produce a fœtus, why are there so many thousands in one drop of it? I answer that in an apple-tree, enduring 100 years and bearing every year a great many thousand blossoms, which may a great part of them be apples, having each of them 6 or 8 seeds, each seed being placed in a proper soil, and carefully cultivated, is capable of becoming a tree; yet it may happen that nothing grows from all the apples

* It is scarcely necessary to remark that this hypothesis of generation is now universally exploded. A living filament furnished by the semen masculinum has been substituted in place of Mr. Lewenhoeck's animalcula, by an ingenious philosopher of this country, now no more; but the theory which at this day has most advocates, is that which supposes that the primordium or germ exists in the female before coition, and that during that act it becomes subjected to the influence and actual contact of the semen masculinum; whereby its previously quiescent parts are stimulated into action; and being conveyed (in the manner described in a note at p. 697, volume 1, of this Abridgement) from its former repository (the ovarium) to another receptacle (the uterus) it there undergoes a gradual developement, and after a time acquires the contour and resemblance of the parent forms.

that fall down; whether through want of sun, rankness of grass, weeds, or other accidents. So in the womb, each animalcule might suffice for a generation, if the place where it comes to be nursed be fit for it; but the womb being so large in comparison of so small a creature, and there being so few vessels and places fit to feed it, and bring it up to a fœtus; there cannot be too great a number of adventurers, when there is so great a likelihood to miscarry.

It may be asked again, why a woman brings forth only 1 or 2 children, since if there were but two proper places in a uterus, several of the animalcules might there be fed. I answer, it may happen to these animalcules, as it does to 7 or 8 seeds put into a small hole of the ground; that seed which puts out the largest and strongest root, starves all the rest, and becomes a tree. It may be asked me again, why I make the animalcules found in the seed of several animals to be of such different sizes, comparatively to the animals they belong to, viz. in the space of a small sand in the seed of a cock, 50,000; in the row of a cod-fish 10,000; in the row of a ruff, which fish is 1000 times less than a cod-fish, the animalcules as large as the others. Whereas it seems reasonable that the animalcules ought to be in size to one another, as the creatures in which they are found; from whence it would follow, that those animalcules which are in insects, would never be capable of being discovered, because of their exceeding smallness. I answer that we must satisfy ourselves in these things as well as we can; for, not to speak of a cocoa-nut, a great wall-nut with its green shell weighs down 1000 apple tree seeds, and yet the proportion between the trees is not so great.

In my letter of the third of March 1681-2 I described the texture of a flesh and fish-muscle; but have since examined that of a flea, judging that if I could find the same filament, I might be positive that the muscles of animals are all of the same make; having therefore several times separated and exposed to view that muscle of the breast, to which the leg is partly fastened; I observed the same ring-like indentings in the filaments, that I had seen in other places. Some appeared to me thicker in the middle then at the ends, as fig. 13, pl. 13, ABCDEFGH is the description of the filament of a flea broken out of the breast, from which I perceived the filaments of this insect to grow tapering towards the ends, and lose themselves in a membrane or tendon, like the filaments of the muscle of an ox. Some of the indentings were as CF, but most throughout were as ABGH. Several times I had an appearance as if a filament were constituted of several lesser threads joined together, and lying by the sides of each other. In pursuing my observations, I took some of the flesh of the legs of the flea, and found it like that of the breast; here I counted 12 of these before named filaments, and some threads without indentings which I conceived might be vessels.

I also with much trouble took out the testicles of a flea, and placed them before my microscope, and drew out the figure as well as I could, as fig. 14, ABCD is the testicle, AF and DE are the vasa deferentia. When the testicles were first taken out they were of a dark colour, but in less than $\frac{1}{10}$ of a minute their moisture was evaporated, and then they became crumpled, which I have here represented as well as I could.

I also searched for the vessels having as it were rings about them, and am satisfied they cannot be air vessels, but rather arteries; for I saw them not only encompass the guts, but spread over and among the eggs. The sting or snout of the flea, or rather the sheath in which it is kept, had teeth on each side like a saw; and may not unfitly be compared to a quill that is split ragged. When the sting is enclosed in the sheath, the teeth on each side go between each other. The length of this sheath is about 3 diameters of the thickness of a hair.

I have made many attempts the last autumn, to find out in what time the worm coming from the egg of a flea, would become a full grown flea. The eggs of a flea kept in a warm place, were hatched in 4 days, and became worms, which I endeavoured to bring up, but notwithstanding all my endeavours I could keep them no longer alive than 12 days. When I placed about the half of a small fly in the glass by the worms for their food; the part of the fly caused such a steam on the glass that the worms being hairy were entangled in the moisture, and remained immovable till they died. When the worms were 12 days old, they were about the length of 4 eggs, and the thickness of one. Since I could bring up no worms, I took some which I thought had attained their full growth, and observed the same to spend 8 or 10 days in spinning their web, and then they stripped off their skin and became nymphæ. These nymphæ I saw move on the 4th day, though they were clear and white: on the 6th they were red about the head: on the 10th they broke their case, which was a very thin skin, and leaped into the glass, living there without food for the space of 7 days.

I also took some flesh from the breast of a louse, and found the flesh threads of the same make as those of the flea. I took also the flesh from the feet of a gnat, and found that of the same make with the former. But perceiving the legs and body of the gnat to be furnished with very fine feathers, I have caused them to be drawn as fig. 15, pl. 13. The wing also of the gnat being adorned with feathers, I have drawn that too, as fig. 16, which is a wing as it appears to the naked eye. Fig. 17, ABC is the same represented in a larger proportion, to show that not only the whole circumference of the wing, as here 17, is beset with great and small feathers, as fig. 18: but also the nerves that stiffen the wing. The film of the wing which is between the nerves, seems by the microscope to be full of a great many small risings; but upon a stricter examination, they are really small hairs, as fig. 19, where a beginning was made to represent the whole wing: ABC are the feathers, and ADEC are the hairs on the film.

A certain physician having told me that several people afflicted with agues, had been cured by the use of sal volatile oleosum, which had attenuated and rarefied their blood. I resolved to make what observations I could of the mixture of that salt with blood. And therefore pricking my finger with a needle, I put the first time two parts of blood to one of salt; a second time equal parts of each: the blood turned immediately of a more lively red colour, as blood usually does when mixed with fair water.

The parts of that blood that lay nearest the salt, changed colour first, and by degrees those further distant. But taking my microscope to observe it, I found the blood globules each to be dissolved into 6 distinct globules.

I then took four parts of salt, &c. and one of blood, and viewing it as quick as I could with my glass, I found some of the blood globules much diminished in $\frac{1}{8}$ of a minute, but in $\frac{1}{4}$ of a minute they were wholly dissolved. I saw once 20 blood globules at a distance from the rest; but in continuing to count them, they came first to 18, then 16, after to 3 or 2, which also were dissolving.—There was also here and there a globule that would not dissolve; nor with a very little salt would any.

A Correction of the Theory of the Motion of the 4th Satellite of Saturn. By Mr. Edmund Halley. N° 145, p. 82.

I here send you an astronomical account of the most remote of all the planets of our vortex, and of the satellite of Saturn, discovered in the year 1655 by M. Huygens, who in that accurate treatise of his, Systema Saturnium, from page 25 to 34, gives the theory of its motion, as well as the shortness of the interval of time between his observations would admit.

The late conjunctions of Jupiter and Saturn giving me frequent occasions of viewing them both, with a telescope that I have of about 24 feet, and pretty good of that length, I easily remarked this satellite of Saturn, and having found it in a convenient position to determine its place, I perceived that Huygens's numbers were considerably run out, and about 15 degrees in 20 years too swift; this made me resolve more nicely to inquire into its period; and accordingly I waited till I had gotten a competent number of observations, the most considerable whereof are these.

An. 1682, November 13d. 13h. 00m. P.M. the satellite appeared on the north-side of Saturn, and a perpendicular let fall from it on the transverse diameter of the ring, fell upon the middle of the dark space of the following ansa; and the same night, 19h. 00m., it had passed the conjunction, and the perpendicular fell exactly on the western edge of the globe of Saturn, as in fig. 1, pl. 18. The northern latitude, and retrograde motion, made it evident that the satellite was then in perigæo.

Again, November 21d. 16h. 15m. this satellite of Saturn was on his south-side, the perpendicular on the line of the ansæ fell on the middle of the dark space of the western ansa, and the same night, 19h. 00m., the perpendicular fell precisely on the centre of Saturn, and the distance therefrom was somewhat less than one diameter of the ring, as in fig. 2, by this it was evident that the satellite was in apogæo.

I observed it in apogæo again on the 24th of January 1683, at 8h. 00m. P.M. the perpendicular on the line of the ansæ fell exactly on the western limb of the globe of Saturn, and at 9h. 30m. P.M. the said perpendicular fell within the globe more than half way to the centre, and the distance from the line of the ansæ towards the south seemed much about one diameter of the ring. Fig. 3.

Lastly, February 9d. 1683, 8h. 10m. P.M. it was again in apogæo, and I could by no means discern towards which side it inclined most, nor whether the transverse diameter of the ring, or the distance of the satellite therefrom were the greater; so that at that time it was precisely in apogæon. Fig. 4.

To compare with these, I chose two out of those of M. Huygens, which seemed the most to be confided in; the first made 1659, March 14d. st. n. 12h. 00m. at the Hague; when the satellite appeared about one diameter of the ring under Saturn, but it was gone so far to the westward, that he concluded, that about 4 hours before, or 7h. 40m. at London, it had been in perigæo. Fig. 5.

Again, March 22d. 1659, 10h. 45m. the satellite was a whole diameter above the line of the ansæ, and the perpendicular thereon fell nearly on the extremity of the eastern ansa. See fig. 6.

By the first of my observations it appears that the satellite was in perigæo 1682, November 13d. 17h. 00m. nearly, at which time Saturn was $3^s\ 21^\circ\ 39'$ from the first star of Aries, in the ecliptic, but the earth reduced to Saturn's equinoctial, and the satellite was $9^s\ 23^\circ\ 46'$ from the same first star of Aries. And March 4d. 1659, 7h. 40m. Saturn's place in the ecliptic was $6^s\ 0^\circ\ 41'$, but the earth reduced, and consequently the satellite, in 11^s 28° 18′ from the first of Aries. The interval of time is 8655 days 9h. 20m.; in which the satellite had made a certain number of revolutions to the fixed stars, and besides 9^s 25° 28′, or 295° 28′, whose complement to a circle 64° 32′, is 2 days, 20h. 36m. motion of the satellite, according to Huygens. So that 8658 days 5h. 56m. or 12467876 minutes of time, is the time of some number of entire revolutions; and dividing that interval by 15 days 22h. 39m. or 22959m. (the period of Huygens) the quotient 543 shows the number of revolutions; and again dividing 12467876m. by 543, the quotient 22961$\frac{1}{10}$m. or 15 days 22h. 41m. 6s. appears to be the true time of this satellite's period. Hence the diurnal motion will be 22° 34′ 38″ 18‴, and the annual besides 22 revolutions 10^s 20° 43′. Having made tables to this period, I found that in the apogæon observation of Huygens the satellite was above 3° faster than by my calculus, and that in the three other observations of my own, being likewise in the superior part, it was 2½ degrees slower than by the same calculation. Now it is evident that these differences must arise from some eccentricity in the orbit of this satellite, and that in March 1659, the apocronion, as I may call it, was somewhere in the oriental semicircle, and that in November 1682 it was in the western semicircle, and supposing the apocronion fixed, it must necessarily be between 9^s 23° 46′ and 11^s 28° 18′ from the first star of Aries, that being the common part between those two semicircles: and because the difference was greater in Huygens's observation than in mine, it will follow that the linea apsidum, or apocronion, should be nearer to 9^s 23° 46′ than to 11^s 28° 18.′ I will suppose 10^s 22° 00′ from the first star of Aries, which also happens to be the place of Saturn's equinox, and the greatest equation about 2½ degrees. On account of this inequality, the mean motion of the satellite will be found about 2° 45′ slower in 23½ years, or 7m. in a year; whence I state the annual motion at 10^s 20° 36′ above 22 revolutions, and the correct epocha for the last day of December 1682, at noon, in the meridian of London, 9^s 10° 15′ from the first star of Aries; from which elements I compose the following table.*

The other two satellites of Saturn discovered by Signor Cassini at Paris, Anno 1672 and 1673, I must confess I could never yet see. I have been told that they disappear for about ⅔ of Saturn's revolution, and were only to be seen when the ansæ were very small, it being supposed that the light which proceeds from the ansæ, when considerably opened, might hide these satellites. In the year 1685, when the ansæ will be quite vanished, will be a proper time to look for them, that so we may bring their motion to rule, and know where to find

* These are omitted, as of no use now.

them, for want of which knowledge it is likely they are at present not to be found.

A Lunar Eclipse observed at the Royal Observatory at Greenwich, Feb. 1/11, 1682, P. M. By Mr. John Flamsteed, Mathematician to the King. N° 145, p. 89. *Translated from the Latin.*

At 8h. 1m. P. M. with a tube of 16 feet, I took the moon's diameter 6702 = 33′ 25″; then the distance of her nearest limb from the nearest limb of Meræotis 145 = 0′ 43″; but the distance of the limb of the same spot from the moon's remoter limb was 6575 = 32′ 48″. Also by means of the same tube I obtained the times, when the obscuration reached the moon's centre, and when the radius subtended its arcs in the periphery that were either deficient or after they were restored: from whence the middle may be deduced, perhaps not less accurately, than from the compared observations of the beginning and end, the immersion and emersion.

The principal phases observed by Mr. Flamsteed and Mr. Halley at Greenwich, and by Mr. Haines at London, were as follow: the times, by pendulum clocks, corrected.

Phases, by Mr.	Flamsteed.			Halley.			Haines.		
	h.	m.	s.	h.	m.	s.	h.	m.	s.
The beginning of eclipse	9	12	32	9	13	4	9	12	18
Sixth part of the periphery obscured	9	18	10						
The moon's centre, or 6 digits	9	38	48						
Sixth part of the periphery bright	10	7	28						
Total immersion	10	10	14	10	10	11	10	9	48
Beginning of emersion	11	47	38	11	47	9	11	47	48
Sixth part of the periphery bright	11	50	2	11	50	7			
The moon's centre, or 6 digits	12	18	28						
Sixth part of the periphery obscure	12	39	6						
The end, doubtful	12	45	10				12	44	48
Ended certainly	12	45	38	12	44	35	12	45	18

Account of a Murrain in Switzerland, and its Cure. By Dr. Wincler. N° 145, p. 93.

In 1682, on the borders of Italy a murrain infected the cattle, which spread into Switzerland, the territories of Wirtemberg, and other provinces, making great destruction among the cattle. The contagion seemed to propagate itself in the form of a blue mist, which fell upon those pastures where the cattle grazed, insomuch that whole herds returned home sick; being very dull, forbearing their food, and most of them would die in 24 hours. Upon dissection there were discovered large and corrupted spleen, sphacelous and corroded tongues, and some had angina maligna. Those persons who carelessly managed their cattle, without a due regard to their own health, were themselves infected, and died like their beasts.

This contagion may perhaps proceed from some noxious exhalations emitted from the earth, by three distinct earthquakes, perceived here in the space of one year.

The method of cure for the cattle was this.—As soon as there was any suspicion of the contagion on any one of the herd, the tongue of that beast was carefully examined. If they found any aphthæ or blisters, whether white, yellow or black, they were obliged to rub, scratch and tear the tongue with a silver instrument, till it bled; then they wiped away the blood and corruption with new unwashed linen. This done, a lotion for the tongue was used, made of salt and good vinegar.

The antidote for the diseased cattle, and the medicine for the sick, is the same, and is thus described. Take of soot, gunpowder, brimstone, salt, equal parts, and as much water as is necessary to wash it down; give a large spoonful for a dose.

In a Postscript it is further observed as follows:—

I lately received an account from two ingenious travellers, who assured me the contagion had reached their quarters on the borders of Poland, having passed quite through Germany, and that the method used in our relation preserved and cured their cattle. They told me the contagion was observed to make its progress daily, spreading near two German miles in 24 hours. This they say was certainly observed by many curious persons, that it continually without intermission made its progress, and suffered no neighbouring parish to escape; but that it did not at the same time infect places at great distances. They added that cattle secured at rack and manger were equally infected with those in the field.*

* In some of the subsequent vols. of these Transactions we shall have occasion to notice other communications on the subject of the pestilential disorder or murrain among cattle; for abating the virulence of which, inoculation has of late years been resorted to in Holland and other parts of the continent, with marked success. This practise is strongly recommended by Dr. Layard, in a paper inserted in the 50th volume of the Transactions. The spreading of the epizootic contagion is to be prevented, and its suppression effected, by means similar to those employed in the case of human contagions; viz. by an immediate separation of the infected from the healthy cattle; by fumigation of the stalls; and by strict attention to cleanliness and ventilation.

On the formation of Salt and Sand from Brine. Communicated by Dr. Robert Plot, S.R.S. N° 145, p. 96.

Having strained a quantity of the brine, from the pits in Staffordshire, through 8 folds of fine Holland, and as many more of finer cambric, yet nothing was left in this very close colander, but a little black dust, which was imputed only to the foulness of the water, it being nothing like sand; for having examined the cloth both with the fingers and a microscope, there was found no more of sand than if they had percolated the clearest spring water; and yet this brine is found to hold in boiling at least $\frac{1}{4}$ of as much sand as salt, i. e. the brine that suffices to make a bushel of salt, yields also at least a peck of sand.

But notwithstanding this experiment, it did not seem necessary to suppose that the sand was generated in the boiling, but might rather be originally there; for before straining it there was observed in the water, by the help of a microscope, a great multitude of very minute animals, much smaller than those in pepper-water, swimming about in it, together with many small transparent plates, some a little larger than the animalcules, and some less, but all of a rectangular oblong figure, though some indeed seemed very near a square, which were found also in the water after straining, as thick as before, the pores or rather interstices between the threads of the Holland and cambric, though they were extremely fine, appeared in the microscope to be 20 times greater than either the animalcules or plates. And these were judged to be the original particles both of the salt and sand; which, as the water evaporates in boiling might gather together till they made up such a visible course body, as the greater corns of each are. And this suspicion was confirmed in a little time; for observing with an excellent microscope some of the strong brine which drops from the baskets or barrows, when the salt is first put into them, though at first it looked like clear water, yet on a more accurate observation it appeared exceedingly full of these oblong particles, which were perceived to gather together and unite to form larger parcels; and, as the water dried off from the glass to grow far larger till they appeared to equal, and not much unlike a large sized diamond: which induced a suspicion that the sand might be also generated after the same manner, it appearing to be only an insipid salt, composed of parts not so sharp pointed as the others, but rounder and blunter angled, and consequently not so pungent on the tongue. Yet this sand could not be made to dissolve in water to any considerable quantity: and though the salt might dissolve in a small degree, yet it did not form itself again into plates.

Account of New Books.

1. *Hortus Indicus Malabaricus, continens Regni Malabarici apud Indos celeberrimi omnis generis Plantas Rariores, &c. Amstel. Anno,* 1678. N° 145, p. 100.

This excellent work, giving an account of most rare and curious trees and shrubs of the fruitful and flourishing country of Malabar in the East Indies, by their descriptions, virtues, and whatever else has been observed remarkable, has been especially promoted by the ingenious governor of the same H. Henry Van Rheede, assisted by father Mathew, a Carmelite, and by the present author John Casiarius, who supplying former defects, has compiled this work, with very apt descriptions, and caused the most essential parts of each, viz. the flowers, fruits, and their cases, to be also engraved in large copper cuts, generally as large as nature, 57 in number, each whole sheet opened containing but one plant and its parts, most excellently done, having had the aid of most skilful painters on the place; to which are added the Latin, Malabaric, Arabic and Braman's names. And for that part concerning the virtues, the author has set forth in their own language, and with the translations, the testimonies of the most learned men of the country.

And for the better completion of this laudable work, that most curious and exact person Dr. A. Seyn, physic and botanic professor lately at Leyden, has been pleased wonderfully to satisfy the world and show his great learning in illustrating this work, with his notes upon each plant, and his thoughts of the same, wherein he has shown what authors have formerly written of it, and what is new, and giving a great character of the work.

Horti Indici Malabarici pars secunda, &c. Amstel. 1679. N° 145, p. 104.

The second part of this work has been especially collected and promoted by that great senator Hen. Van Rheede, to whose counsel and assistance the beforementioned John Casiarius being called, has continued his expressions of skill, in describing the rare and unknown plants of that country. But the loss of that curious and learned annotater D. A. Seyn, whose ingenious notes so illustrated the former book, being snatched away by untimely death in the flower of his age, had rendered the work imperfect, had not an ingenious successor D. I. Commelin risen up in his stead, who has given good demonstration of his reading and learning, by comparing and adjusting these exotic rarities to what others have formerly said of them, and added his notes upon the same.

In this part are 56 large and excellent figures, drawn and cut exactly to nature, each one employing a whole sheet, holding but one plant or part of a tree, as in the first part. When this writer has occasion to express a tree, he makes a part of a large trunk or body cut off, with a limb or branch springing out, bearing the leaves, flowers, or fruits, or what is observable, which seems a much more expressive way, than to contract a whole large tree to so small a compass as other sculptures usually do.

Horti Indici Malabarici pars tertia, &c. Amstel. An. 1682. N° 145, p. 106.

This third part the before-mentioned H. Van Rheede has dedicated to the Indian King, in whose dominions he lived several years, and there collected, and made his observation of what that country afforded, hoping by his accuracy that his account of so many new and wonderful plants and trees may not be apprehended an ungrateful work, since the most part are such as have not been mentioned by any European author.

The two first parts of this work were especially compiled by the reverend and skilful D. J. Casiar, a divine in the city of Cochin, where then lived D. P. Herman, the present botanic professor at Leyden, a person of excellent experience and skill, and no small instrument in this work by his assistance; and since the death of D. Casiar, before the finishing of the work, D. Van Rheede took to his assistance the well experienced D. J. Munnicks, now professor at Utrecht, for the prosecution and completion of his design, to which D. J. Commelin has added his curious notes, but tells us that this third volume should have been more properly the second, consisting mostly of trees as the first did; and the second should have been the last, containing shrubs and plants, in which are 64 whole sheet plates of sculptures well executed.

II. Epistola ad D. Joelem Langelottum De Alcali et Acidi Insufficientia pro principiorum corporum Naturalium munere gerendo, conscripta à Johanne Bohn Phil. ac Med. Doct. et in Acad. Lips. Prof. P. N° 145, p. 110.

III. Jacobi Barneri, D. Spiritus Vini sine Acido, &c. Demonstratio curiosa. N° 145, p. 111.

The Lumbricus Latus; or, a Discourse read before the Royal Society, concerning the jointed Worm. By Edward Tyson, M. D. Col. Med. Lond. nec non Reg. Societ. Soc.* N° 146, p. 113.

The consideration of insects, and their manner of generation, as it is a subject of curious speculation; so of late it has been much illustrated by the laborious researches of many inquisitive persons: whose endeavours, though they have

* Tænia Solium. Lin.

much advanced the doctrine of univocal generation, and bid very fair for the exploding of that common error, of their production from putrefaction, yet one great difficulty still remains with me, how to account for several of those that are bred in animal bodies; not such as we may suppose to be hatched from the eggs of the like kind, that are received with the food or other ways, but of whcih we cannot meet with a parallel, or of the same species, out of the body, in the whole world as is known besides. I shall instance only in two, the Lumbricus Latus, and Teres. Of the former I shall give at present only these remarks; wherein its difference from any other does more remarkably appear; 1. being flat; 2. jointed after a peculiar manner; 3. the great disproportion of both extremes; 4. the vast length it is often of; 5. the head so remarkably beset with hooked spikes; 6. what has never that I know of, been remarked of this, or any other insect or animal in the world beside, the great number of mouths it has; more than the poets feigned Briareus had hands, or Argus eyes, viz. in every joint one. 7. That any part of the body being broken off from the rest, should still remain alive, and thrive.

My other instance is the lumbricus teres intestinalis; which, though it holds more analogy with those called intestina terræ, or common earth-worms, as to the outward shape and form; yet within are vastly different; as by comparing the anatomy of both will appear. And I very much question, as to the contrivance of the inward organs, whether we can meet with any parallel of it in the world, out of the microcosm, or animal bodies, besides. There is nothing more plain, than that these worms are propagated by an univocal generation; there being so perfect a distinction of sexes, male and female; and the organs belonging to each so curiously contrived, so conspicuous and plain, that they may further illustrate the late inventions of some, and seem to show how solicitous nature is in preserving and propagating the meanest species. But as in this worm, there is a most evident discrimination of sexes; so in earth-worms, there is nothing of this nature to be found, but they are perfect hermaphrodites, each worm having the organs belonging both to male and female; which is common to them with leeches, snails, &c.

I shall begin with the jointed worm; and shall pursue the method I have already given, in more strictly considering those particulars, which I have laid down, to discriminate this from all other sorts of worms. And the first is, its being flat; hence called lumbricus latus, and by Hippocrates, *ταινία*, i. e. fascia, and by some in English, the tape-worm. This flatness of the body, sufficiently distinguishes it from the others, which are usually bred in the body; and are either short and small, and then called ascarides; or longer, as the teretes;

which are so called κατ' ἐξοχὴν, though the former likewise are round. Nor is there any out of the body that I know of, that are thus flat.

The second particular I observed was its joints. From these large incisures of the body, a great number or genus of animals have the name of insects. In earth-worms, leeches, &c. the annuli, which make up the length of the body, are very remarkable; but much different from those in our present subject; those being more proportionate to one another, and not of so unequal a length. And in the teretes and ascarides, the surface of the body seems more even and smooth, and not divided after this manner. These joints are so connected, that the lines or extreme edges of the former come over the latter; which is to be well observed, and may direct us to that part or extreme, where we may suppose the head to be placed. These edges of the former joint, which shoot over the latter, in some I have observed to be plain and smooth; in others crenated and indented; in all, by drawing it through one's fingers from the tail to the head, there is found a great roughness; but if the other way, from the head to the tail, it seems smooth.

3. Many who have observed this worm, take notice of the difference of its extremes; how much larger one is than the other; but not well considering the setting on of the joints, they have often mistaken the head for the tail; for in all I have hitherto observed, I constantly found that extreme where the head is set on, if we may allow it to have any, much smaller than the other; sometimes not half a quarter so broad; in others less or greater, often according to the length of the worm; but in all I take notice, if they are of any considerable length, that the joints towards the head are vastly shorter than towards the tail. For in one I have by me (fig. 1, pl. 19,) 24 feet long, there about five joints make an inch; whereas the latter joints here are above an inch long; but in some I have taken out of dogs, there where 30 or 40, sometimes above 60 annuli, which, towards the head, make up but the length of an inch; whereas towards the tail 6 or 7 joints equalled that measure, and sometimes 3; so that gradually the joints seem to increase, both in length and breadth, as they approach the tail. But withal it must be observed, that according to the corrugation or extension of these joints, their dimensions will be altered; which is most apparent in them when alive. That likewise there is a great difference of these joints in the various species of this worm; for I think there are more sorts than one. And as to the differences of them, there are these I have taken notice of. 1. That in most the joints gradually and very sensibly increase in length. 2. In some, those orifices, which I take for mouths, were placed about the middle of the joints, on the edges; in others, about the middle of the flat of the worm, near the jointings. 3. These juttings, or lips of the upper joints,

over the lower, in some were plain; in others crenated; in others, the great protuberatings at the side rendered the whole worm serrated. 4. Usually the same joint is much of a size throughout; but the upper extreme something less than the lower. But in one I took out of a dog, I observed that towards the tail, the upper part of the joint, by which it was fastened to the foregoing joint, was very slender; in the middle broad; and towards the other extreme grew taper again; so that it well enough resembled the figure, (fig. 6, pl. 19,) which Cornel. Gemma has given of it. Not that the whole worm, as he has made it, was so; but only some of the last joints. And in another I took out of the same dog I could not observe the same thing; as neither did I in a third, I took out of another dog, which was about two yards long; whereas these were each but about a foot or a foot and half long.

4. As to the length of this worm, it is sometimes as long as all the guts; not that it lies extended straight the length of the guts, as those might think, who fondly imagined it was nothing else but a mucous skin, or spolium of the same: but it lies convoluted in several places; so that it often vastly exceeds the whole length of the guts themselves. Platerus observed one 40 feet long; and Pliny says, they are sometimes 300 feet or more. Thaddæus Dunus saw voided by a woman one piece of this worm 5 yards long; and another, above 20 yards long. Yet in neither he could observe either the head or the tail. But what Olaus Borrichius tells us is remarkable; that a patient of his, in a year's time, has voided 800 feet of this sort of worm, but in several pieces; and that 200 feet of it he kept by him; and that hitherto he has not met with the head. For the patient observed, that always in the voiding it, he perceived it break off; that he has not yet come to the end; and still goes on in voiding the same. Which I could parallel with an instance of a person here in town, once my patient, who has voided vast quantities of this worm, for several years together; but in several pieces, 2, 3, 4, 6, or more yards long; but all put together, would much exceed the length of that of Borrichius. Tulpius says he showed, in the Anatomy Theatre, 40 yards of this worm; which was voided by one in two days time. However I question whether all those pieces which are voided by the same person, may be always reputed parts of the same worm, or of different. Hippocrates, or whoever was the author of that book ascribed to him, asserts, that it is bred in the child whilst yet in the womb. But Spigelius, on inquiry both of the midwives, in Germany and Italy, could never be informed that they observed these worms in new-born infants. But as to Spigelius's reasons why there should be no more than one in the body at a time, I shall only give this answer, that on dissection of a dog I observed two entire ones; but each not much above a foot long. So that the answer he gives to that

observation of Forestus, who mentions twelve voided at the same time, that they were only parts of the same worm, though often it may be true, yet sometimes it may be otherwise; especially, where we see such prodigious quantities are voided of them. However this is undeniable, and must be allowed, that this worm is vastly long, which plainly appears even by those pieces we see of them; for, besides the instances already given, Simon Schultzius mentions one 7 yards long, and another 9 yards. Clusius tells us, that the Duke of Austria's cook voided pieces of this worm, 6, 12, and 15 yards long. Jacobus Oethæus measured one 18 yards long. Alexander Camerarius has seen them above 20 yards long. In the palace at Tiguri is kept the figure of one 18 feet long: and abundance of more instances I could give were it needful.

But I shall describe that piece of one (fig. 1, pl. 19,) I have by me, voided by a young man about 20 years of age, on the use of an emulsion of the cold seeds, who plainly perceived it alive, and to move; and having put it in a wide mouthed glass, it often endeavoured by raising its body to get out; but the cold water into which it was put afterwards soon killed it. I measured it, and found it 24 feet, or 8 yards long. In it I numbered 507 joints. Its colour was extreme white, being turgid with chyle; its body flat, about the thickness of half a crown, where thickest; and the joints towards the tail about $\frac{1}{4}$ of an inch broad; those toward the head about $\frac{1}{4}$ as broad as those towards the tail, and here the joints were not $\frac{1}{4}$ of an inch long, whereas those at the tail were of a full inch long, and something more; and from the head they seemed gradually to increase in length. The joints much of a wideness throughout; and the jetting edges of the former over the latter usually plain and even; unless where the contraction of the body had rendered them a little crimpled. The flats of both sides just alike; and without any spots, protuberances, or any thing remarkable, which might distinguish them, or be observed, only a smooth surface; but about the middle of the edges of each joint I observed a protuberating orifice, which would easily enough admit a hog's bristle, and was open, and apparent to the naked eye. These orifices were placed for the most part alternately, in one joint on the right side, in the following on the left. But sometimes I have observed them in 2, more seldom in 3 succeeding joints of the same side; but never in one joint more than once. These orifices I take at present for so many mouths. But since I have here mentioned of what length they have been observed in man, I shall also add how long those were I have seen in dogs. For though they are to be met with only in the animal kingdom; yet in abundance of the subjects of this, and those too of different species, they are very frequent; in fishes, as in the pike, whitings, bleakes, crabs, herrings, &c. and upon this score sometimes they prove a great damage to the merchants, as Platerus observes, they being forced to throw them away. In bleakes in the summer time, if you open those that leap and tumble on the water, from the torment they feel within, you almost constantly meet with this worm, which is a thing well known to the watermen. In oxen often they are observed likewise, not so much in calves; but in dogs very frequently; which Platerus makes to be another sort of the tænia, and calls it ligula. Simon Schultzius mentions a lap dog, that in a short time voided 9 yards of this worm in several pieces.

I have oftentimes here seen them myself, but shall mention those only I found in dissection; as I met with the first time two. There was indeed another piece, which I take only as broken off from one of the former, because here both extremes were pretty large, and the joints throughout proportionably long. But in the two others the disproportion was very remarkable; for besides observing here their heads thick beset with hairs or small spikes, which I shall afterwards describe, I took notice that this extreme if extended, was very slender; and when a little contracted, the joints so very small, that they were scarcely discernible by the naked eye; but where I could better distinguish them, between 30 or 40 made the length of an inch; but towards the other extreme or tail, in one 4, in the other 6 or 7 joints made that length; one of these worms was scarcely a foot long; the other not a foot and a half.

In another dog I since dissected, I found another worm, (fig. 2, pl. 19,) with just the same head, but about 5 feet long; towards the head in this 60 joints scarcely made an inch, but at the tail about 3 equalled that space; and the joints here were about a quarter of an inch broad; and in the sides of the joints in this, I plainly perceived those orifices I at present call the mouths.

5. The head of the Nile does not seem to be more perplexed and obscure to the ancients than that of this worm, which has created as many controversies among anatomists of late, as that has with geographers of old.

Welschius tells us, that the first that discovered and gave these worms a head, was Tulpius; and since that, Jo. Mich. Fehr. I shall therefore give their observations of it; and then deliver what I have observed myself. Nic. Tulpius, a noted physician, and burgomaster at Amsterdam, in the former editions of his Medical Observations, makes this worm to be biceps; (fig. 3, pl. 19,) and to have two heads, placed like the horns of a snail; not that he observed it so himself, but had the relation of it, and figure, from Henricus à Rugen, a kinsman of Augerius Clusius, who voided it. To this I shall add, what Joh. Rhodius has delivered; which favours this opinion of a double head; where he tells us, that Adrianus Spigelius on dissection of a lap-dog, which died of an epilepsy in the year 1622, found the intestines full of this sort of worms, sed imprimis latus lumbricus iis adhæsit, capite bifido, qui veram candidi coloris fasciam referebat; but notwithstanding these authorities, I shall still suspend my belief of these double heads till better information. And indeed Tulpius himself seems to me to suspect the truth of the observation, having in the last edition of his book left it out, without taking any notice of it, and given another very different; and in the same figure he has erased the former heads, and clapped on a new one, to the old body (fig. 4, pl. 19,) quite different from what was before. But by all I can see in his figures, I cannot but think he is at the wrong end; for by considering the prominence of the joints, the placing of its spots, and the difference of its ends, I should rather look for it at the smallest extreme, which he makes the tail, than the other where he has now placed it.

Jo. Michael Fehr, a German Curioso, in his Treatise de Hierâ picrâ sive de Absinthio, in the year 1644, observed in a piece voided by a patient, about 6 yards long, a head much different from that of Tulpius; he describes this worm cum collo sensim angustiore, et rotundiore in minutissimum capitulum atrum, et verrucosum, trium papaveris seminum aptè conjunctorum formam exprimens, desinentem: cujus iconem ob raritatem hic addidimus. (fig. 5.) Indeed I must confess that account I had from the women who first observed it, and the patient who voided that worm I mentioned to have by me 8 yards long, and was given me by my worthy friend Mr. Houghton an apothecary, seemed agreeable to this; though, when I first saw it, I could observe no such thing; and therefore am apt to think it was only some thrumbs of the inward coat of the intestine, which might stick to the hooks here, which might make this figure. For, in the heads of all I have yet had an opportunity of seeing, I could never observe any such thing.

I shall therefore now deliver my observations of the heads of this worm as I have seen them, in three several ones I have taken out of the bodies of dogs on dissection. One was in a dog I opened at our private meetings, at the Anatomical Theatre of the College of Physicians, where I observed this worm alive in the ileum; not lying straight, but in many places winding and doubling. Having observed how the joints were, I traced it up, by carefully opening the intestine to the smallest extreme, where I expected the head to be; and which lay towards the duodenum; whereas the broader end was downward towards the rectum; and this broad end was free, without adhering; whereas that smaller extreme stuck so firmly, and had fastened itself to the inward coat of the intestine, that it was not without some trouble, by gently raising it with my nail, that I freed it from its adhesion. Having lifted it up, I carefully viewed it; and observed neither that biceps in Tulpius's first figure, nor the head like a tricoccos as in Mich. Fehr, but a very slender body; which being alive, it would sometimes shoot out a considerable length; at others retract it in again, and so very much alter its figure, by becoming broader.* But whilst I was doing this, by its riggling its body it happened to fall off my finger; it presently took hold again, and gave me as much trouble to free it a second time from its adhesion as at the first. For the present I put it into spirit of wine, that I might more carefully view it with a microscope at home. And in doing this, making use of some extraordinary good ones, it very plainly appeared as is represented in my 8th fig. thick beset with two orders of spikes, or hooks, whereof the larger rose from the middle, spreading themselves over the edges of the circumference; the other, which were less, issuing out about the midway, and were shorter, as is seen in this figure, and are represented sideways in the 9th fig. I could not on the strictest inquiry, and with extraordinary glasses too, perceive any orifice here, which we may suppose to be the mouth; only a little indenting in the centre, occasioned by the issuing out of the spikes there. This end was not perfectly flat, but a little globous; and I could perceive by the swelling a little below on the neck, and wrinkling of the skin, how it shot out, and contracted its neck, as I observed it when alive: for some little space here, I could not observe with the glasses any joints at all; but after, very thick set and small, and gradually increasing in length, as they descended towards the tail. The heads of the other two worms exactly appeared the same in the microscope, as this. And afterwards by carefully viewing them by my naked eye, I could observe these hairs or spikes.

It was objected by some ingenious persons, who had been acquainted with what I observed concerning this head, whether these spikes or hairs might not be like the small feet of the tick or recinus, for its fastening itself the better to help its suction. And indeed were it blood it lived on, the case were plain; but since it is chyle, I do not see what service they could do it in this, for when they fasten, the head is deep immerged in the inward coat of the intestine, and so may be thought for that time to get but a very inconsiderable soop, if any, and nothing in proportion to what is requisite for so vast a long body; and what it is often observed to be turgid with. On the whole, what seems most

* It is remarkable that Linnæus, who does not appear to have examined the tænia with much accuracy, denies the existence of the head. ‘*Caput serpentiforme ad extremitatem crassiorem finxerunt Tyson, Andry, Tulpius, sed fallunt, cum omnis articulus propria vita gandeat, nec ullus vermis capite instruatur, quod canicidiis constat.*” Syst. Nat. p. 1323.—The real structure of the head is now well known: it is surrounded by a double row of claspers or hooked processes, as truly described by Tyson, and with four round orifices for suction, which Tyson does not appear to have discovered. The species of the tænia are very numerous, and they chiefly inhabit the intestines of quadrupeds, birds, or fishes.

agreeable to me, and to be the true use of this part we call the head, is this: that by means of these hooks and spikes it might fasten itself, and so prevent its too easy ejection out of the body. For it being so very long and large too, and its body in many places winding, and convoluted, the descent of the fæces upon all occasions would be apt to carry it out with them, had it not this hold, which is so fast, that rather than loosen itself, parts of the body are sooner broken off, which we frequently see in the stool. When it penetrates the coat of the intestine, it contracts its hooks in, and draws up its head to a point, then expands them, and takes firm hold of the membrane, by darting its several poniards into it, which excites those intolerable pains which those that are troubled with them so much complain of; that I have known it to that extremity, that some have been scarcely dissuaded from offering violence to themselves, to free themselves, as they thought, from a great misery; and hence it is, that this worm is of so difficult a cure, that though by medicines and purges vast quantities at times may be brought away, yet some can hardly get a perfect cure all their life time, as I know of one who for above 20 years has been afflicted with it.

I have already observed the orifices in every joint to be of 2 sorts; that in several worms, both from human bodies, as also in those of brute animals, they are placed much about the middle of the joints on the edges, most frequently alternately, in one joint on the right hand, in the other on the left, sometimes in two, seldom in more on the same side; they are protuberant, something like a papilla, (fig. 7) and in the middle a foramen easily enough to be perceived by the naked eye, and will readily admit a hog's bristle. In the other sort, these protuberances are placed about the middle of the flat of the worm, towards the upper part of the joint. At present I shall chiefly insist upon the former sort, which has occurred most commonly to me, and a short black line here, placed transverse to the body, I think was the first that gave me notice of them; though since in others I have not so constantly seen it, but only a protuberant orifice about the middle of the edges of the joints. That they are so many mouths, I shall argue, first, from the great quantity of chyle they are often turgid with. Secondly, from the great appetite, but more often thirst, but almost always that emaciation which they occasion. Thirdly, that there is no other mouth besides observed. Fourthly, that no uses can so fitly be assigned to these orifices as their being mouths.

As to the first, all that have observed them, cannot but confess that they are often very turgid; as that I have by me 8 yards long, at first did very plainly appear; and having put it into spirit of wine, I found after a little while it had muddied it, by spewing out a large quantity of a chylous juice, which made a deep sediment at the bottom, as likewise it did a second time, having changed the old and put it in fresh spirits. Whence all this should issue I cannot see, but by these orifices at the sides, which first I supposed had received and licked it in. And being in so large a quantity, how otherwise could it be well received into the body, but by these many mouths? which being always open, and lying of all sides too, do greedily exhaust and devour the best part of the chyle, and nutritious aliment. That hence may be well accounted for, that appetitus caninus, that great thirst, that atrophy, that are often observed in those that are afflicted with this worm. Indeed Spigelius thinks this bulimy and atrophy are occasioned not so much by the worms devouring the chyle, as 1. Corrupting it. 2. Hindering its distribution by occluding the meseraick veins. 3. By its creating a false sense by the motion in the guts. All which reasons do nothing move me. And Galen is express that it is by devouring the nourishment, and so is Aetius, and 20 more that I could name. But had they but one mouth, how could they do this? But having as many, it may be as the lacteals themselves, it is no wonder that they rob them, and by their speedy drawing it up, prevent its passing into them. That thence we must necessarily expect an extenuation of our own bodies, in proportion to the increase of theirs, since the nourishment we receive is but what they leave us, and that too none of the best, and corrupted likewise with their excrements. 3. I argue that these orifices are so many mouths; for if we do not admit them to be such, I know not where in the whole body to find them besides. For in that part we call the head, even our microscopes cannot discover any; and those too, who guessed it to be there all acknowledged it to be very small; and it being so, and but single too, I cannot see how it can take in so great a quantity of chyle, which would be necessary for maintaining so great a body of so vast a length; for as it can only lick up what just comes in its way, the open mouths of the numerous lacteals, would be too hard for it, and quickly starve it. Besides, since it nousels its head so deep in the coats of the intestines, at that time at least it may be thought incapable of getting scarcely any thing at all. Therefore 4. why I think those orifices mouths is, because I cannot think what they are besides. For to take them for so many vents of their excrement, would be more unreasonable, since it is pure chyle which they receive, which will not afford much, at least so gross an excrement as to need so many and large orifices for the voiding it. And why so many anuses when but one mouth? it is easier to imagine them bronchia or lungs, which in insects are observed in all the annuli, or joints of the body; but with very great difference from our subject. For in them you shall constantly see these orifices of both sides in each annulus; but in our worm never but on one side; in those they are not near so open and large as in this worm, even so much that I cannot see how it can be avoided, but that the chyle must slip into them, and so spoil them for being lungs.

Upon the whole, what I have here offered I think is sufficient to render my conjecture probable. And yet I have more reason to add why these orifices should be mouths, because the joints when broken off yet still do live, and that too as may be thought for some considerable time, which they could not unless they had mouths in each, which might receive the aliment for the support of it. Which brings me to the last particular I proposed for discriminating this worm from all others out of the body. But since it has been so stiffly maintained by authors of great note, both modern as well as ancient, that the worm itself scarcely lives, but is only a spolium of the intestine, or at least that it is not one but many worms included in that membrane; I shall consider how unlikely all such opinions are, and wide of the truth, and then deliver my own observations concerning it.

Hippocrates, or whoever was the author of that book, amongst his works, tells us that this worm is ὀκοῖόν περ ἐντέρȣ ξύσμα λευκόν, quasi album ramentum intestinorum. And Aetius, and Paulus Ægineta are express that it is only the inward coat of the intestine turned, and changed into the figure of an animal, and many more are of the same opinion not worth mentioning, it seeming so absurd, as Mercurialis observes. He rather thinks it to be the mucus, which lines the inside of the intestines, and defends them from any asperities of the fæces. And abundance there are of this opinion. But Franciscus Valleriola seems the most of any to play the philosopher, and labours to show how this pituita of the guts may be formed into a membrane, and then endeavours to explain how these incisures or jointings of the body might happen likewise. Felix Platerus is very positive that they are no animals at all. But those many physicians who have observed them to move, and therefore to be animals and alive easily confute him; and Gabucinus mentions one voided by a child 2 years and 4 months old; that being put into water lived almost a day. And a remarkable instance I had which I met with on the dissection of a dog in the theatre of our college, where several of the members were present. I shall therefore mention what I particularly observed of it, and how it moved itself, which was very pleasing, and in different forms. For though all was performed by contracting and shortening the joints, yet sometimes it rendered the body that was flat round, and a cylinder; other times it made a deep hollow or concave on one side, and a convex on the other: but most times there was a bellying out at the edges, about the middle of the joints; and though that part towards the head was very slender, yet upon contraction it would become as broad as the last joints. This contraction of the joints I sometimes observed at several places at the same time, at some distance from each other, which must needs greatly promote its progressive motion, since, being of so great a length, it could make but small advance, which is perhaps requisite, that it might recover itself, when the descent of the fæces drive it downwards. And for the advancing too of its motion, at every joint there is a prominence of the former over the latter, which like so many scales on the belly of other reptiles do perform the use of feet.

But I find that those who admit this worm to be alive, have several of them very different thoughts of it; and many assert that it is not one, but many worms linked together. Gabucinus denies the lumbricus latus, i. e. that spolium of the intestines, as he calls it, to be an animal; but that it receives all its sense and motion, from those cucurbitini included in it. This he very plainly, as he tells us, discovered in a part of this worm, showed him by a person that voided it. In that I met with in a dog in the college theatre, whilst alive, and in my hand, a joint or two fell off, but I could no ways observe any membrane hanging to the foregoing joint, out of which it might slip, but it broke off entire. And although there were 2 single joints, which I found in the intestine, on the first opening it, yet there was nothing I could see affixed to the last, which might include them. And indeed the setting on of the joints here is such, that seems to me sufficiently to show, that this worm cannot be a continued membrane, articulated only by the several cucurbitini included in it, since there is so large a protuberance of the lower extremity of the foregoing joint, over the upper part of the following, which I plainly perceived in this worm. If only a membrane, why constantly and thus regularly a difference of both extremes, as to their length and breadth? How happen the hooks at the head? How are those orifices formed at the edges, or on the flat of the worm? And if it was so, as Gabucinus imagined, I cannot think but I must have perceived something of it in those several pieces of this worm, which I have observed, and especially in that 8 yards long, where I opened several joints, and could find no such thing. That mucous matter therefore, which is observed to be voided by those troubled with them, which he tells us the women there take for the beds of this worm, may be better accounted for; it being likely in a great measure to be but the mucus of the intestines themselves, or a slimy spolium cast off from these worms. Thus leeches I have observed being put into water cast out a slime, which covers their bodies, which afterwards they slip off and is found in the bottom of the glass in the form of a mucous coat. So earth-worms void a large quantity of a mucous liquor at several parts of their body; so snails, &c. of which more in my anatomy of those animals. Upon the whole, I see no reason why we may not justly ascribe that life we find here to the lumbricus latus itself, and not to any animals we may fancy it pregnant with. And what I give to the whole, I must attribute likewise to the several parts of it, even when separated from the rest of the body; and cannot but think that they live also. But they are only the joints or pieces broken off from the latus, and when they are voided in the stools, are a sure sign of a jointed worm. And the cure must accordingly be adapted. But that all these single joints whilst in the body do live, besides those considerations I have already delivered to prove that in every joint there is a mouth for receiving the food, (and no doubt answerable organs for the digestion and distribution of it) I am the farther induced to believe, because it has been often observed by myself and others, that both single joints and oftener larger pieces have been voided alive, and where vast quantities of this worm too have been voided at the same time; in abundance of pieces, I have observed them almost equally turgid, and alike filled with chyle, in proportion to the magnitude of the parts. Now I cannot think that in voiding, it can always be broken into so many pieces; and if it be done sometime before, and they lie dead in the body, they must be macerated, and different from what they appear. But that observation, I have already often mentioned of that worm I met with in the dog I dissected in the college theatre, furnishes me with something apposite to our purpose. For here about the middle of the worm, as it lay in the intestine, about a foot and a half from the tail, or lower extremity, I observed 2 single joints, about $\frac{3}{4}$ of an inch long, that were alive, and which continued their motion briskly for $\frac{3}{4}$ of an hour, or more in warm water. That these were broken off from the tail, I have no doubt, being in all respects so like them. And that it must be done some time before, I am apt to think, because they were so remote from it; for they could not otherwise easily, being but single joints, make so great an advance, being on all occasions liable rather to be driven down, not being able, as I could observe, any ways to fasten themselves, and so resist the force of the descending fæces. Which is the reason when broken off, that they are so frequently voided.

Upon the whole, I have been sometimes apt to think what analogy there may be between this jointed worm and knotted plants, of which each joint can so easily propagate itself. And whether it may not be thought a plant-animal, or zoophyton, bred in animal bodies, since so large and frequent detruncations of the body do not destroy the life of the whole. Which I think can scarcely be instanced in any animal besides.*

The Explanation of the Figures.

Pl. 19, fig. 1, represents that worm, or rather part of a worm, voided by a young man in London, 8 yards long, which I still keep by me. The less extreme is that part towards the head, the broader, the tail. The protuberances about the middle of the edges of the joints, are the orifices I take for mouths.

* The hydra and other of the polypus-tribe were at this time unknown.

Fig. 2, represents the worm I took out of a dog on dissecting it in the college theatre, which was about 5 feet long, and was alive. The small end shows the head, as it appeared then to the naked eye, and is represented magnified by the microscope in the 8th and 9th figures. The protuberances at the sides are the mouths. The broad end the tail, as in the first figure.

Fig. 3, 4, 5, represent the figure of the head of this worm, which are given us by Nich. Tulpius and Jo. Mich. Fehr.

Fig. 3, represents the 2 heads, which Tulpius in the former edition of his Observations gave to this worm, where he makes it a biceps.

Fig. 4, is the picture of the head of this worm, which Tulpius gives in the latter edition of his book of Observations.

Fig. 5, shows the head of this worm, as it is delineated by Jo. Mich. Fehr, which appears like a tricoccos.

Fig. 6, is the figure of this worm in Cornel. Gemma.

The following figures represent parts of this worm, as viewed by the microscope.

Fig. 7, represents the protuberance or papilla about the middle of the edges, and in it the orifice, which I take for the mouth of this worm.

Fig. 8, is the head of this worm as it appeared in the microscope, in 3 several ones I took out of the body on dissection, wherein is observable a double order of spikes or hooks; the longer arising from the centre, the other more towards the edges, which at pleasure it can draw in or protrude, and with them part of the neck also, as appears by the swelling out a little below, as it is very curiously delineated, as well as the other figures, by my most ingenious friend and accomplished gentleman, Rich. Waller, Esq.

Fig. 9, is a side view of the head and the hooks in it, of the same worm.

Total Eclipse of the Moon, Feb. $\frac{11}{21}$, *1682, observed at Paris and at Copenhagen.* N° 146, p. 145.

The Phases	At Paris. by Cassini. h. m. s.	At Paris. Picard & De la Hire. h. m. s.	At Paris. Fa. de Fonteney. h. m. s.	Copenhagen. Roemer. h. m. s.
The beginning	9 20 55	9 21 58	9 21 25	8 40 10
The centre immersed		9 51 10		9 8 40
The same by comparing the beginning and end........	9 50 24	9 50 44	9 50 10	9 8 50
Moon entirely immersed	10 19 53	10 19 30	10 18 55	9 38 10
Beginning of emersion......	11 57 51	11 56 0	11 58 0	11 13 40
Centre emerges		12 25 50		11 44 40
The same by comparing the emersion of the two limbs..	12 26 9	12 25 15	12 26 8	11 43 10
End of the eclipse..........	12 54 27	12 54 30	12 54 17	12 12 40

The same Eclipse observed at Dantzic by M. Hevelius. N° 146, p. 146.

Phases.	Corrected time.		
Beginning of penumbra, very faint	8^h	40^m	46^s
Beginning of the eclipse	10	25	5
Total emersion of the moon	11	27	30
Beginning of the emersion	13	0	8
The central emersion	13	28	40
End of the eclipse	13	59	17
End of the penumbra	14	15	0

Lumbricus Teres, or some Anatomical Observations on the Round Worm bred in human Bodies. By Edward Tyson, M.D. &c.* N° 147, p. 154.

I shall here give the anatomy of the lumbricus teres, that common round worm which children usually are troubled with: and in this more particularly make my remarks on the organs of generation in both sexes; and herein show how vastly different they are from those parts in the common earth worms, and perhaps most others. I had designed to have given also the anatomy of the earth worm; but have since altered my intentions: and at present shall refer to the account given of it by the famous Dr. Willis, reserving my farther observations of it to another opportunity. This sort of worm by Hippocrates is named στρογγύλαι; by Celsus, teretes; and is usually about a foot long, more or less; but I have hitherto observed that the male is generally less than the female: so that by their magnitude in the same body I have before dissection been able to distinguish the sex. They are about the size of a wheat straw, or a goose quill; their colour white; but being subjects so generally known to all, I shall forbear a further description of their outward parts; only as I remember I did not observe those feet, or asperities on the annuli, as in the earth worm. At both extremes they grow narrow. Their mouth is composed of three lips as in the figure. So the leech has three cartilaginous teeth set in a triangle, by which they make the wound in the skin in suction. The anus is a transverse slit a little before the extreme point of the tail.

In opening the body I found I cut through a large muscle under the skin: which muscle in earth worms I find is spiral; as in a good measure is their motion likewise; so that by this means, like the worm of an auger, they can the better bore their passage into the earth. Their reptile motion also may be explained by a wire wound on a cylinder; which when slipped off, and one end extended and held fast, will bring the other nearer it. So the earth-worm,

* Ascaris lumbricoides. Linn.

having shot out, or extended its body, which is with a wreathing, it takes hold by its small feet, and so contracts the hinder part of its body.

I observed also that dividing this part, there issued out a copious ichor; which is naturally discharged by some pores or small vents in the skin; which in the earth-worm is of great use, by rendering the surface of the body slippery, that so it might the easier glide into the earth. And in these other worms of the intestines this humor, as in leeches, makes a covering to the body, which is often cast off, and observed as a mucus, in the stools of those troubled with them.

In these teretes of animal bodies I never observed those transverse diaphragms which are so numerous in earth-worms, and do intersect or rather so deeply depress the intestine. But the cavity chiefly seems to be filled with the genital parts, which I shall now describe: only first remarking, that the passage from the mouth was somewhat straightened for a short space, and was distinguished as in the figure, from the following ductus; which was a straight intestine, continued to the end of the body, without any winding or other distinction of a stomach that I could observe.

As to the genital parts of the male, I could here observe a penis, a vesicula seminalis, and a testis: in the female a pudendum, vagina uteri, cornua uteri, and spermatic vessels. The penis in the male was placed at the tail, or opposite extreme to the head; and seemed to be able to exert itself almost the length of a barley corn, or proportionably to the length of the vagina in the female. At the root of the penis was inserted the neck of the vesicula seminalis, which gradually grew larger as it ascended in the body, and usually reached almost half way. It was filled and turgid with a milky juice; which it received from a slender vessel of the same colour inserted into it. Which after one turning, was afterwards very much convoluted; and so forms that body I call the testis. Although this part be so loosely contexed, as even to the naked eye it appears but as a continued vessel, and may easily be unravelled its whole length, which I measured above a yard: yet I make no difficulty of giving it the name of a testis; since it is now sufficiently known, that the testes in more compleat animals are only a congeries of vessels. And a rat, besides this worm, is not the only subject wherein I have found them thus loose and easily separable.

In the female worm, almost about the middle of the body, but more towards the head, I observed an orifice or pudendum, which led into the vagina uteri; which soon divided into the two cornua which were large, and remarkable. For descending something winding towards the tail, they were then reflected again, and did each of them terminate in slender vessels, white, as they were, but much smaller; and lay in several convolutions and windings among them. These I take for spermatic vessels. Having taken those vessels, with the cornua uteri and vagina, out of the body, and laid them on a paper to dry; I found from each cornu, to the end of the spermatic vessels which I had preserved, that they measured about four feet. I opened the cornua uteri, and found them turgid with a milky juice; having placed a little of it on a small microscope, I plainly perceived it was nothing else but an infinite number of small eggs; though, to the naked eye, it appeared only as a fluid body. These eggs when fresh, appeared, as is represented in fig. 13, pl. 19, covered with abundance of small asperities; but as they grew dry their surface appeared smooth. By comparing that small quantity I did observe, in which I could distinguish so many eggs, with the whole substance contained in both the cornua, I guess there must be more than 1000 eggs in each female worm.

How far different this worm is from common earth-worms as to these parts, I need only to refer to Dr. Willis's figures and account of it, to show. And I am yet to learn what worm out of the body has these organs thus formed. When once there, the case is plain how they propagate themselves. And Menjotius, and all before him, that were of that opinion, are mistaken, who say that these worms do not generate; nor have any distinction of sexes. Hippocrates is express, ἁι μεν στρογγύλαι τίκτȣσι. And I think nothing can be plainer than this distinction of sexes in them.

But I find on the other hand, there are many who do not only allow them to generate, but make them viviparous too. Thus P. Borellus tells us, Vermem crassum ab hominis corpore eductum, fortéque pedibus exenteratum, non sine admiratione vidi vermiculis innumeris refertum esse. So Amatus Lusitanus tells much such a story; that a girl voiding a large worm, and the father treading on it, ex eo alii prodierunt vermes. And Felix Platerus gives an observation of a boy that was hydropical, and voided all his excrements upwards; who dying in the hospital, and they observing a motion and palpitation in his belly, were afraid to bury him till they had sent for the doctor. He opening him found the intestines in some places swelled as large as his thigh; in others so convoluted, intorted, and twisted, that hindered any passage downwards, either of excrements or wind; Sed et vermibus vivis quamplurimis repleta erant, qui rursum aliis minoribus referti. You may see an instance likewise de vermibus fœtis in Salmuth Cent. 3. Obs. 24. But Dominicus Panarolus is very express; and tells us, he observed it thus in two several persons; In utroque expulsi fuerunt vermes colore carneo, longitudine circa sexdecim digitos, qui prægnantes erant, et ligno collisi cum fuissent, apparuerunt vermes parvi, subtiles, albi, longitudine sex digitorum, prope innumeri, qui tanquam serpentes parvi movebantur. But whatever is related of this nature, I cannot but think it is a mistake; and that they were imposed on by the genital parts of this worm; which not warily examined, might easily make them think they are so many small worms. For they are not viviparous but oviparous, as I have shown; and their containing so vast a number of eggs in the cornua uteri, as I have expressed, sufficiently accounts for that prodigious quantity, that are sometimes observed to be bred in animal bodies.

Panarolus tells us, he once saw the stomach and guts stuffed with them so that they ascended up to the throat. Baricellus, by the use of crude mercury, brought away from a patient above a hundred. Jo. Jadoc. Weckerus did the like, with the use of tansy seed and syrup of violets. Gabucinus saw voided by stool 177. Benivenius saw voided by a child 7 years old 152 worms. And Jacob Hollerius, out of Musa, gives us a history of a man of 82 years old, who voided above 500. And Petrus Paulus Pereda saw a nobleman's child in a few days void almost a thousand, and she voided 40 in 4 hours time.

Those animals are usually the most multiparous, whose young are the most exposed to danger; and were it not that the greatest part of the litter of this worm is usually carried forth by the fæces, it could not be avoided but we should be devoured by an enemy we breed in our own bowels. That caution therefore of Henr. ab Heers I think is necessary; to avoid the giving the powder of these worms for expelling others; since we cannot be secure, but that at the same time we may sow the seed for propagating more.

The Explanation of the Figures.

Plate 19, fig. 10, represents the male worm opened. Where, a shows the three lips of the worm; b the œsophagus, or gullet; ccc the large intestine; d the penis; ee the vesicula seminalis; f the testis.

Fig. 11, represents the female worm opened. Where, a shows the mouth; b the gullet; ccc the intestine, or gut; dddd the vagina uteri; e the two cornua uteri; fff the spermatic vessels; g the anus.

Fig. 12, represents the genital parts of the female explicated. Where, a shows the pudendum or foramen, as it appears on the outside of the skin; b the vagina uteri; cc the two cornua uteri; dd the spermatic vessels.

Fig. 13, represents the eggs of this worm, as they appeared when viewed by the microscope.

A remarkable Relation of a Man bitten by a mad Dog, and dying of the Hydrophobia. In a Letter from Martin Lister, Esq. dated York, March 26, 1683. N° 147, p. 162.

James Corton, a very strong and well made young man, was bit by a mad dog in the right hand. The wound healed of itself, and the thing was forgotten even by himself and his wife. After about five or six weeks he complained of pain all over his bones, but especially in his back and round about his stomach ; and he looked very pale, hollow eyed, &c. The third day after this complaint, (Sunday, March 11, 1682,) he called for burnt brandy, drank it, and went to bed, but vomited it up. After this he had a restless night, and in the morning found himself very ill, with a strong rising in his stomach ; and though no thirst, yet an inability to drink, and even to swallow his spittle, which was death to him as he often said. Diascordium and a bottle of cordial water was brought to him ; he took the former, but was not able to drink one spoonful of the cordial. About 1 o'clock that day, (Monday) I first saw him; his pulse was very slow, and sometimes unequal; his flesh cold, his tongue not dry, but flexible and moist, a little white. As he complained greatly of sickness about his stomach, I offered him of the cordial, but he started, and trembled at the approach of it. A tumbler of water was brought, which I gave him to drink ; but he vehemently started at it, and his stomach swelled and rose, after a strange manner ; and I could then find his pulse very trembling and disturbed. I still urged him to drink; but as I put it forwards to his mouth, he the more affrighted drew back his head, and sighed, and eyed it with a most ghastly look, not without shrieking and noise. This happening on several times repeating, it became plain the disease was the hydrophobia.

I forthwith ordered a vein to be opened in the arm which was bitten, caused the wound to be scarified and drawn with vesicatories, and the same plaister to be applied to the neck and legs, and the inside of the arms; I ordered the usual antidotes to be given him, as of theriaca, cinis cancrorum, ruta, agaricus, &c. in boluses ; for he could take solid things in a spoon, but yet not without much trembling, fear, and caution, and an earnest request that nobody would suddenly offer them to him, but give them into his hand gently; and then he would by degrees steal his hand softly towards his mouth, and of a sudden chop the spoon in and swallow what was in it, like a dog. Of these antidotes in bolus he took a drachm every hour, and always in this manner, for at least a dozen times taking; but when drink was offered him, he could not see it without horror, and the same motions from his stomach. And he affirmed that as oft as he by chance swallowed any spittle, it went to his heart, as if he should die that very moment. This night passed wholly without any sleep or rest.

Tuesday morning I viewed his blood, which was both as to the serum and cake well coloured, and in such proportion as is usual in healthful persons, and of good consistence. He had now a violent fever on him, and a very quick pulse. Water was again offered him, but in vain; he begging he might die unmolested, nothing being such a terror to him as the approach of any drink; and that none might come suddenly upon him, or offer him any thing more, for all things frighted him. I then with much difficulty persuaded him to cast himself across the bed upon his belly, hanging his head over the other side; persuading myself that this posture might be advantageous to his drinking, since that in the erect posture of a man he could not so much as endure the approach of liquor. In this posture then of a dog, he suffered a large bowl filled with small-beer to be brought under his head, and embracing it with raptures of joy, he declared he was infinitely refreshed with the smell of it; that he now saw it with delight, and assured us he should be able soon to drink it all off. And though he had just before thought himself dying, he now talked pleasantly, and said many passionate things to his brother, wife, &c. wonderfully extolling this invention, and thanking me for it. He endeavoured with great earnestness to put down his head to it, but could not; his stomach rose as often as he opened his lips; at length he put out his tongue and made towards it as though he would lap; but as often as his tongue touched the surface of the beer, he started back affrighed. And yet seemed pleased with the thoughts of drinking; and would not suffer it to be taken away from under his head; and if it was but a little withdrawn, he said he followed it by the smell with delight, snuffing with his nostrils. After a long time trying in vain, he alleged that the faint smell of the small-beer hindered him from drinking, and therefore desired a bowl of ale; which was brought him; but after much striving, and exerting his tongue a thousand times, he could not drink of it; and lapping with great affrights, as oft as his tongue touched it he started back with his head, bringing it down again gently to the bowl a hundred times, but all in vain. And in this posture, what upon his belly and what upon his hands and knees, he kept himself at least an hour thus tantalizing himself; but still it was not in his power to drink. We then gave him a quill which consisted of two or three joints, the one end in his mouth and the other in the liquor; but he could not manage it, nor suck any more than a dog. I persuaded him to cease trying, and lie down; which he did; and not long after my going away he fell into a convulsion fit, bit and snarled, and catched at every body, and foamed at the mouth. After this fit was over he took an elleborism in a bolus, which was taken like the rest, and very willingly by him; it wrought about 3 or 4 times very plentifully, and he declared himself wonderfully at ease by it; but yet now and then became convulsed, and then was always insensible.

After 4 hours I returned to him again. He was again solicited to drink, and he now readily enough put himself into the former posture, and with as much earnestness as ever used all the little shifts to drink, while the bowl was under his head; but all in vain. He had a little silver tumbler filled with drink put into his hand; which suddenly, when he had as it were stolen it near his mouth, he would have thrown it into his throat, as he did the boluses; but it hit against his teeth and fell into the bowl. As he never went to stool or made water all this time, a clyster was given him; but on parting with it, which he did immediately almost as soon as given, he died convulsed: but his not making water, as well as a troublesome priapism, which he complained of when on his knees, might proceed from the blistering plaisters, as well as from his disease.

The day after his interment I accidentally met his cousin Mrs. S. who told me that her daughter was in fear, for just that very day fortnight before his death she had been at his house, and he would go home with her to her mother's; that she remembered his hand trembled and his body shook, that he was in a cold sweat, and in a great disorder, so that she asked him what he ailed: he told her that after his work (for he was an upholsterer) it had been of late usual with him: and what was remarkable the very dog which bit him came at that time along with him to her mother's house; and was alive and well at the time of the man's death.

To this we add, that Mr. Widdow, a mercer, affirms, that about the very time that Mr. Corton was thought to have been bitten by Sutton's dog, a black dog, which he verily believes to be the same, came and bit a whelp of his in his shop. The next day the whelp ran mad up and down the house, and bit both him and the maid; him in the hand, and the maid in the leg, and died that very day. About a month after he was bitten he found himself unwell, and was troubled with a pain at his heart, and had a fearfulness and trembling upon him, and got no rest for 3 nights, upon which he had himself blooded, and found himself better; his maid does not yet complain of any harm.

[The author's speculations on this case are here omitted, as tending to throw little light on the subject. It may, however, be proper to remark, that Dr. Lister believed the patient's spittle to be infected with the canine poison; an opinion now generally received. Future opportunities will occur in the course of this work, of offering some observations on the nature and treatment of this most dreadful of all disorders.]

A Continuation of a Discourse on Vision, with an Examination of some late Objections against it. By William Briggs, M.D. and Fellow of the College of Physicians. N° 147, p. 171.

Having formerly given a specimen* of my thoughts on vision, I have been prevailed with to make this enlargement of the forementioned discourse, to the fuller explaining my thoughts, and the clearing some difficulties which have

* In Mr. Hook's Philosophical Collections, N° 6, p. 540, of this vol.

been propounded against it. In that small essay I endeavoured to show, 1. That the fibres of the optic nerve, as rising from the two protuberances of the thalami optici, were more concerned in vision then either the cornea, humours, or retina; not only because sensation is performed chiefly in the brain, and these other parts are but the transennæ to it; but also because in an amaurosis, or gutta serena, these parts are free from any indisposition, the eye appearing as naturally without any fault, though the sight is then wholly lost. 2. I show there that the superior fibre in each thalamus opticus had the greatest tension, and the inferior the least; as may appear from the former arising from the top of the thalami optici and having the greatest flexure thus ⌒; and the latter arising from the lower part of the aforesaid thalami, and having the least flexure thus ‿. 3. It may be further noted, that the intermediate or lateral fibres in the same eye, though diametrically opposite to each other, are said to differ in tension, by reason of a more considerable flexure of the external then the internal. 4. I observed that the optic nerves arose separately from those two moleculæ of the brain, and besides have a peculiar advantage in rising from these hillocks in this manner; whereas the other nerves arise from the basis of the brain in a flatter manner, and closer together, so that the extreme difference of their rise is very remarkable, and intended surely by nature for some extraordinary end. 5. In the position of the fibres of the optic nerves, I show that they keep their distinct order, and consequently that they are not mixed or blended together at the place of their connection, as was frivolously supposed by authors before to solve the union of vision. 6. I observed that in the insertion of these fibres into the eye, where the medullary part of them forms the retina, they still kept their distinct series, and that they are much kept in, not only by being fastened or terminating on the processus ciliares, but also by little transverse fibres, which serve to connect those that run longwise, and make the whole coat appear in a glass of clear water like lawn or tiffany, as I have shown. None that I know ever did it before me, and those that have mentioned the same experiment since, have mistook my intent in it. For the putting the retina in water is not to wash off the mucous substance, which is its proper substance; but to expand the fibres by the playing it up and down in warm water, and to magnify the image of it by a double refraction of the lucid rays, which pass through that and the glass that contains it. 7. But next of all I would have it observed, that whereas I say the intermediate fibres gradually differ in tension as they are nearer or further from the top of the thalami optici, it may be easily supposed that they do it by so minute gradations, that the difference of those that are higher to the top from the superior of all is very little, but from those that are further off great enough, and the difference of the highest fibres from the lowest, greatest of all. 8. Whereas I mention sometimes the parallelism of the correspondent fibres, I mean it not in a strict mathematical sense, but only their being as it were in æquilibrio or due poise in respect of their situation; and therefore if those fibres had been straight, and not of a curved figure as they are, I should have rather chose to have expressed my mind by the phrase of mathematicians, of their being in eodem plano. But my sense being understood, there need not be any exception to the word, since it was not so easy to express my meaning by a better; and therefore I shall pass by this, and proceed to more real objections that have been sent me by Mr. Newton, our worthy professor of mathematics at Cambridge, and other friends, relating to the opinion itself.

The 1st objection was made in the Royal Society when it was read there, which it seems was this; viz. That it seemed difficult to conceive how those soft medullary fibres of the nerve could have such a tension. But this is not harder to conceive than in that of a spider's web, whose mucous substance and expansion very well answer to that of the retina; and as the least breath of wind moves the one, so the least gale of the ethereal or lucid matter causes a vibration in the other. Further it was objected, that it was hard to conceive how so soft a body as the thalamus opticus could make such a difference in the stress or tension of the fibres. But it is apparent, that on drawing the nerve from it forward, the superior fibres are more on the stress than the lateral, and nature in these cases is finer in her operations then we are in our conceptions of them. It seems so especially in the formation of this organ.

2. It has been objected by others, that if the superior fibres were more tense than the inferior, we should see better by rays falling on the top than the bottom of the eye, or see an object better placed below our eye, when the rays passing in a straight line from it must terminate in the top fibres, than above it, when they must terminate in the lower fibres. To this I answer, that it does accordingly fall out so, and this is a more positive and direct proof of my opinion; for I appeal to any man's experience whether the characters in a book appear not better to him, or he reads not better in it, held about half a yard under his eye than so much above it; or whether he does not more readily discern or find out objects beneath than above him with the same light: and this may be further illustrated by my Lord Bacon's experiment, of a man's appearing better on the ground to him that is placed on a high steeple, than vice versâ.

3. It is urged that according to my scheme of the situation of the correspondent fibres, the rays of an object placed laterally, suppose toward the left eye, could not fall on the fellow fibres in the right eye; for if it were placed slantingly toward the left eye, the rays could not fall on the internal lateral fibres of

both eyes in that position, but on the internal of one, suppose the left eye, and the external lateral of the right; which would cause a double perception. This objection I foresaw when I hinted that whether the nerves decussated or not, it would be no prejudice to my opinion, nay perhaps might more fully confirm my opinion where they do. In that passage I had respect also to the inversion of the image in the eye being rectified in the brain; though that equally presses any other hypothesis, and the explication of the thing may be well enough understood by a blind man's judging of the position of an object above his head by touching it with one end of his stick, though the other end terminates under that object or in his hand: and so in our view of an object, the true situation of the respective parts is not distinguished so much by means of that end of the ray that terminates in the eye, as of the other end that touches the object, from whence the vibration or protrusion comes.

But to leave this, which does not so particularly concern me, I come to the objection itself, as it relates to my opinion; and though it seems at first view the most difficult of all to be answered, yet it may be determined by the experiment itself, better than by the scheme (in the Philos. Collections), where the eyes are not drawn in that position that is here required. Now let there be placed an object near the left eye of any person, but not so near that eye as that the nose might hinder the rays from falling on the right, because it is to be seen with both, and whilst that person looks on it let a by-stander observe the position of both eyes, and he shall see that the pupil of the right eye is turned in a very oblique manner to the object, whereas the pupil of the left is scarcely so at all, whereby there will be three parts to one more in the distance of the pupil of the right eye from the external canthus, than there will be in the other. Hereby it appears that if the object be so placed that it is seen with both eyes, the right eye accommodates itself to the position of the left, that the rays strike correspondent fibres, and the percussion or vibration being toward the bottom or papilla of the eye, a small turning of one eye to another will make that accommodation.

Moreover as this accommodation is made in an oblique position of the object, so is it more readily done in a direct position of the same; and this we may perceive in an object's retiring in a straight line from the eyes, whereby the pupils gradually divaricate; as on the contrary they converge when the object is seen very near them, and that so forcibly that it is painful to hold them long in that posture.

4. But to proceed to other objections: the case of cross-eyed persons by birth I have considered at the end of the forementioned essay, and shown why a

morbid strabismus, or more violent contortion of those muscles after great convulsions of the nerves, causes always a double vision.

5. It has been urged that the tension of all the fibres of the optic nerves might be uniform, notwithstanding the greater flexure of the superior; because these latter might be longer, and consequently might not have a greater stress upon the thalami optici then the lateral: as, if the arm of a tree grows bent, the fibres on the protuberant part seem not more stretched than on the concave side, but to take only a longer compass. To which I answer that sense evinces the contrary in our case, and if any one draws out the optic nerve straight forward from the thalamus opticus, he will plainly see that the top fibres press more on the subjacent medullary protuberances than the lateral, or make a deeper impression.

6. It has been urged that the action of vision was uniform, and therefore required an uniform tension of all the fibres. To which I answer, that though in the view of the entire object, or its place by both eyes, it ought to be so, and that therefore it was done by correspondent fibres; yet in a stricter view of the parts of the same object by one eye, there is a discrimination.

7. Lastly it has been urged that the fibres of the choroides seem more adapted to vision than those of the retina, because these last did not sistere species, as transmitting the colours of the former; and besides some blood vessels running among them would interrupt the image; and lastly sensation could be better continued to the tense fibres of the pia mater by the one, than to the brain by the softer of the other.

An Account of the Dissection of a Bitch, whose Cornua Uteri being filled with the Bones and Flesh of a former Conception, had after a 2d Conception the Ova affixed to several Parts of the Abdomen. By a Physician, F. R. S. N° 147, p. 183.

It would seem a needless thing to publish an observation, to confirm the opinion of the production of animals from eggs, which is almost universally received: but that some time since the learned Diemerbroeck, and very lately Mons. Verney have endeavoured to confute and expose it. The most considerable argument they use is taken from the narrowness of the Fallopian tubes, where they open into the womb, and at their extremities. But, though these authors lay a great stress on the structure of that passage, it cannot be accounted of any force, when ocular demonstration is brought against it; and the eggs are discovered in the entrance, and afterward to have made their way through them into the womb.

The sagacious Dr. Harvey was very near the discovery of the egg, and its use: he came within sight of it, but overlooked it. After many repeated dissections of impregnated deer, he asserts, that nothing for about 6 or 7 weeks can be seen in the cornua of their wombs: that there then appeared somewhat like an egg, a transparent liquor included in a very thin membrane, in which after a week he could plainly see the rudiments of a fœtus. He gives his opinion very expressly in his treatise de Partu; "vipera ovum unicolor et molli cortice (qualis muliebris conceptus est) intra se continet, &c." The viper has eggs, whose parts are of one colour, enclosed in a soft shell; and the very same is a woman's conception. But he could not inform himself, how the eggs in women or in deer come into the womb. He declares himself fully satisfied after several trials, that no liquor can be so forcibly injected into the womb, so as to make its passage into the place of conception. Nor did he suspect, that the seed of the female lay till the egg appeared, in any crannies or recesses of the cornua uteri, which he asserts are then as smooth and soft as the corpus callosum of the brain. Dr. Harvey having thus sufficiently confuted the opinion, which till his time prevailed, of the production of animals from the mixture of the seminal matter of both sexes, it was not so difficult to discover whence the egg came, which he saw about 7 weeks after impregnation. The Fallopian tubes which join to the cornua and terminate very near the ovaria, as the testes muliebres are generally now called, directed the ingenious and industrious de Graaff to make more accurate dissections of them. He presently saw that the limpid liquor which Dr. Harvey thought was designed to humect the parts adjacent, was contained in an entire membrane, and exactly answered the description he gives of the eggs he saw in the womb.

As he first discovered the use of those globules in the ovaria, so in that excellent piece de Mulieris Organis, he demonstrates the alteration of those which are impregnated, the way of excluding them by the glandulous substance swelling behind them, and the aperture through which they pass, remaining open all the time of gestation, &c. But more particularly he has very nicely observed the progress of the eggs in conies, the very time of their passing into the tubes, and appearing in the cornua of the womb, which comes very near that portion of time Dr. Harvey observed the eggs in his deer; so that nothing can be objected of force enough to shake an opinion confirmed by such numerous exact observations. He has prevented the objection which Diemerbroeck and Mr. Verney use against him, that the tube is too narrow at each end to transmit globules of that size: alleging that the hole by which it has its exit out of the ovarium is as narrow; that no force is used to open it, but it expands itself, as the os uteri before the birth: as nuts and peach-stones, &c. give way to the germinating plant, which is less able to make its way than the egg. But besides though the extremity of the tube be membranous in most quadrupeds, in which it is possible a seminal liquor might be transmitted to the womb; in women it is divided like a knot of ribbon, and is no more adapted to receive any thing but an egg, than the fingers expanded to receive and contain a fluid. The egg has not been able sometimes to get into the womb; Riolan speaks of a human fœtus seen in one of the tubes;* and Dr. Harvey assures you he has seen it himself. In the dissection of a bitch at Oxford, the embryoes either could not get into it, the membranous expansion being hindered from ascending to and clasping the ovaria, by the fulness of the womb; or from the same cause were forced back again. She had been with whelp: by a blow she received the fœtus died within her. She discharged by the pudendum a great quantity of putrid flesh and matter. She was afterward able to run in the pack. After the second impregnation she was observed to have a very ill shaped belly. When dead the owner sent her to Oxford. The cornua of the womb were so stuffed up with the bones and firmer muscles and thicker skin of the fœtus, (some of them lay in the usual posture, the skeletons of which were entire, the interstices of the bones only filled up with skin and flesh), that no seminal matter, or aura seminalis could possibly find a passage to the ovarium. The eggs affected in the second impregnation, finding no room in the cornua, were forced back into the abdomen: where they were found affixed to the mesentery, kidney, &c. Only two of the bags had a communication with the womb by a slender duct. These I suppose fell into the cornua first, and began to fasten to them, but growing larger were forced to retire. The other three had had no reception there at all. The membranes which contained the embryoes were all of them very thin, and the animalcules in them had wanted a due supply of nutritious matter. This seems to give as clear a proof of the truth of the modern opinion, as can be expected or desired.—[In the concluding part of this communication, the author subjoins some pointed reflections on the absurdity of the doctrine of equivocal generation.]

The Anatomy of a monstrous Pig. Communicated by an ingenious Student in Physic. N° 147, p. 188.

About the beginning of December 1682, among many pigs of a sow, there was one which had no passage for the fæces, either solid or liquid, although the anus was not outwardly closed up; which, whether natural, or caused by the neighbours who had attempted a cure before I saw it, I cannot positively assert. There was likewise no visible sign of either sex. Being dissected, it afforded

* An instance of this is recorded in the first volume of this Abridgement, p. 358.

these following observations. 1st. The guts were very much distended and transparent, and through them appeared the fæces very liquid, accompanied with no small quantity of wind; the end of the rectum was entirely closed like a bladder, and sealed as it were hermetically, pendulous in the cavity, and not in the least continued to a sphincter, of which there was no sign. 2dly, There was no bladder to be found, nor uterus, nor any mark of what sex it was designed for. 3dly, To supply the place of a bladder, both the ureters were inserted into the rectum within an inch or thereabouts of the end. 4thly, The stomach was full, even to distension, of a hard substance, which being opened was exactly the same to appearance with hard pressed curds. 5thly, The chyle came freely enough out of the ductus Pecquetianus, where it was inserted into the jugular, on the smallest pressure of the intestines. 6thly, I could not urge the liquid or flatulent contents of the guts upwards within 2 inches of the pylorus, though I pressed them till they broke, which hindered the inquiry after a valve, that possibly might be there, to hinder the regress of any thing to the stomach.

Fleta Minor, or the Laws of Art and Nature in knowing, judging, assaying, fining, refining, and enlarging the Body of confined Metals, &c. By Sir John Pettus, Knight, &c. N° 147, p. 189.

This useful treatise contains in general the whole art of proving and melting all sorts of ores and metals, and is divided by the author into five books; of which the first treats of silver ores, assay furnaces, cupels, assay-weights, touch-needles, &c.; the 2d of gold ores, parting, cementation, &c.; the 3d of copper ores, the manufacture of brass, &c.; the 4th of lead ores, and briefly of tin, antimony, quicksilver, iron, steel, and the loadstone. The 5th and last book treats of mineral salts; viz. saltpetre, vitriol and alum. A dictionary of the terms employed in metallurgy is subjoined to the whole.

Benjamini à Brookhuysen Œconomia Animalis, &c. in 4to, 1683. N° 147, p. 194.

An attempt to explain the intellectual and corporeal functions, on the principles of the Cartesian philosophy.

An Abstract of a Letter from Mr. Anthony Leuwenhoeck, of Delft, to Mr. R. H. concerning the appearances of several Woods, and their Vessels. N° 148, p. 197.

I send you here some observations on wood. Fig. 1, pl. 20, BCD is a part of the circumference of an oak or ash tree, &c. being of 18 years growth, and therefore having 18 rings, one for each year; that which is made the last year being always the greatest, though not always proportionably great, but according as the year is more or less fruitful. The pieces described* in the following figures are such as E† in the 15th ring, and sometimes not so large; yet from such a part I doubt not but the constitution of the whole tree may sufficiently be understood.

When a tree is sawed across, and afterwards planed very smooth, we see lines‡ as it were drawn from the centre A, and reaching to the circumference B; these are vessels which carry the sap to the bark; as by the adjoined figures will appear.

In fig. 2, ABCD shows a piece of oak, which observed in a microscope was thus drawn from a piece of wood as large as H. FF where the brown strokes appear, are the separations of the growth of one year. For when the growth stops, the wood becomes firm and thick; and is supplied with many small vessels, such as are hardly to be distinguished, and therefore appear as brown rays or streaks. Between the said FF, FF, is comprehended that thickness of wood which has been added to the circumference of a tree by a year's growth. The wood has five sorts of vessels,|| viz. Three sorts going upwards, and two lying horizontally. EEE denote large ascending vessels made every year in the wood in the spring, when it begins to grow. These are filled within with small bladders, which have very thin skins, here expressed in one of the greatest vessels, cut longwise in the third figure by IKLM. The second sort of rising vessels are much smaller, which also are made of very thin skins, and are also specked with parts which by a common microscope appear like globules, as ON, fig. 4, where one of the said vessels is cut longwise. The third sort of rising vessels are very small, and in great number, being made also of very thin skins, as PQ, fig. 4, where they are drawn longwise.

All these ascending vessels in the aforesaid piece of wood, which is about $\frac{1}{90}$ of a square inch, are I guess about 20,000 vessels. Hence in an oak tree of 4 feet diameter are 3,200 millions of ascending vessels, and in one of 1 foot, there are 200 millions of vessels. If we suppose 10 of these great and small vessels in

* The same noted in Dr. Grew's Anatomy of Trunks, p. 24.—Orig.

† Note the figures as they are here engraved are not so large as those designed by the author, being sometimes but $\frac{1}{5}$ (or other part) of their length; and so must be supposed not to reach from ring to ring as E.—Orig.

‡ The same which Dr. Grew calls the insertions, Anatomy of Plants, chap. 2. And diametral rays, Anatomy of Tr. p. 20, 50, &c.—Orig.

|| See a figure of the same wood with all the same five parts, in Dr. Grew's Anat. of Plants. And the partitions of the great horizontal parts, hereafter mentioned, see in his Anat. of Roots, tab. 7.—Orig.

a day to carry up 1 drop of water, and that 100 of these drops make one cubic inch, there will be 200,000 cubic inches. These inches reduced to feet, amount to full 115 cubic feet of Rhinland measure, of 12 inches to the foot; and one cubic foot weighing 65 lb. of our Delpht water, the whole will amount to 7475 lb. or 14 Bordeaux hogsheads of water, which a tree of one foot diameter in one day can bring up. Whereby it appears, that how small soever the quantity of water is which a pipe or vessel may be supposed to carry up, yet if all the vessels were employed to that use, how much the total would amount to. But I conceive that several of these vessels convey of the same moisture downwards again to the root, and so cause a circulation: as I have formerly said.

These forementioned uprising vessels empty constantly their sap into an incredible number of vessels, which lie horizontally in the body of the tree, to cause a continual growth in thickness. Fig. 2, GGG are a sort of vessels which run horizontal, beginning from the pith of the tree, but afterwards in great numbers taking their rise from the ascending vessels. These vessels appeared like dark streaks running crooked, and winding for the most part along the sides of the great vessels. To observe these vessels better, I caused the wood to be cut in length in such manner that I came to divide the said vessels across very neatly. These vessels lie not above 5, 6, or 7, one above another, as they are here drawn between the ascending vessels PQON, fig. 4.

The second sort of horizontal vessels which lie in great numbers together, but in some places much more than others, are described fig. 2, AB or CD: but when we cut the wood in pieces longwise, and thereby cut across these vessels, then they appear to our naked eye as RS, fig. 5. I have also drawn the same in many places at their length, with crooked partitions, which I judged to be valves* though I have not been able to see them always so clearly as they are here expressed, but after I had found them sometimes, I concluded them to be generally so, both because I have seen them in elm wood, as also that I concluded without these valves it were impossible the tree should increase in thickness, because of the force that is necessary not only to separate the bark in the spring from the wood,† and keep it loose; but also to cleave and open the bark all the time the tree is growing; and thereby make room for its increase in thickness. Now if there were none of these valves, then the sap which was impelled by the heat of the sun against the bark, with the setting of

* Sig. Malpighi and Dr. Grew both make these partitions to be the terminations of the bladders of which these radiated parts consist, and not valves. See the forementioned Anatomy of Tr. fig. 21, p. 21, 22, and Anat of Roots, tab. 7.—Orig.

† Dr. Grew thinks that the bark is never separated from the wood. See Anat. of Tr. p. 52, &c. —Orig.

the sun, when I conceive the sap ceases to rise,* would fall back again, and cause a labour in vain. In this small piece of wood TV, fig. 4, (described by a magnifying glass which augments more than that by which the ascending vessels are drawn, that so the vessels might appear more distinct) there are more than 2000 vessels, each of which, if it be allowed but $\frac{1}{4}$ of a grain of force in the protruding its moisture, the force which all these horizontal vessels together must use towards the separation of the bark from the wood, will appear by this calculation. Suppose then that fig. 4 is $\frac{1}{700}$ of a square inch, and that in the whole piece are but 2000 vessels, which make in a square inch 1,400,000 vessels, that is 350,000 grains; this number divided by 10,240, the grains in a pound, makes full 34 pound, for the force which the vessels in so small a piece of wood come to exercise upon the bark.

In fig. 6, ABCD, is represented a piece of elm-wood, which to the naked eye is the size of fig. E, and is what this tree increased in one year in thickness. AD and BC show the ceasing of the growth in the year's end. The small rising vessels, which lie together between the great vessels, are in this wood smaller than in oak, and each pipe consists of a thicker film than the pipes of the oak. AB and CD show the horizontal vessels at full length.

Fig. 7, FFFF show the horizontal vessels cut across; of which, those that lie but few together, I conceive to be vessels lately proceeding out of the rising vessels: whereas on the contrary, the other vessels which lie many together, have subsisted many years, and are as many in number as they ever will be. GGGG show the small rising vessels at length.

HH show one of the great rising vessels in length, cut cross in the middle; yet when we observe the same more exactly, we see that they consist of very thin films, beset with helical threads, exhibiting obscure spots on the bends, fig. 3.

Among these trees there are several which upon tapping yield a sap. This sap I have several times, and several years observed, and taken notice in it of divers small animals, which I could not imagine should have come out of the wood, but rather that they might have come from rain water or dew.

Fig. 8, ABCD is a piece of beech, as large as fig. F to the naked eye, the thickness of one year's growth; wherein one may plainly see at AD or BC the beginning and end of the year's growth. In this wood there are represented two sorts of rising vessels, viz. greater and smaller; and I imagine that there are also two sorts of horizontal vessels, which are very small: one of which, viz. EEE lies but 1, 2, 3, or more together, as fig. 9, which shows those vessels

* Yet vines and other plants bleed day and night.

cut through, and marked by HHH. The 2d sort which lie far asunder, as in proportion D from G, and are marked out in their length by DC: these vessels are likewise very small, and accordingly the rising vessels lie very close to each other; they are likewise cut asunder across, as I, I, fig. 9. Also, KKK are the great rising vessels cut asunder longwise: which I generally observed to be beset with small parts, that through a common magnifying glass seemed to be globules.

Fig. 10, ABCD represents a little piece of willow wood, as large to the naked eye as fig. F. The same consists also of two sorts of rising vessels, small and great; the large ones beset with little parts, seeming globules. In the same I saw bending lines which heretofore I imagined to have been valves, as is represented fig. 11, in one of the same rising vessels marked by G. These small rising vessels have exceedingly thin films, which it was impossible for me to delineate with red chalk, so finely as was requisite. In this wood I saw but one sort of horizontal vessels, marked by EEE. These vessels are but few in comparison of the horizontal vessels, which I hitherto have observed in other woods. The same are not far extended in length, and for breadth they lie single.

Fig. 11, HHH is the same vessels cut across, represented between the rising vessels, being at their length. In one of the great rising vessels GG, I likewise showed that they are beset with little parts seeming globules, but they are exceedingly small.

Fig. 12, is a piece of alder-wood, the breadth of which is about the thickness of a hog's bristle to the naked eye. It consists likewise of two sorts of rising vessels; the smaller of which consists of exceedingly thin films, and the larger sort of films beset with little parts extremely small, to which you can give no other names than globules. Between AB and CD is the increase of the tree in a year's time. EEEE are the vessels which run horizontal.

Fig. 13, FF are the great rising vessels cut through longwise. HH are the small rising vessels. GG the horizontal vessels cut across.

Fig. 14, ABCDEF is a little piece of black Mauritius ebony wood, exhibited by a microscope magnifying more than any of the former; because these vessels could not well be observed by the ordinary glasses; and this little piece of wood, wherein are shown about 1100 rising vessels, is no larger than to be covered by an ordinary grain of sand. I purposed at first to have drawn this wood more at large, having intended to have shown its decrease in the latter end of the year, and increase at the beginning of the year or spring: but in my attempting I found that my labour would be frustrated, because that wood grows in a climate where it increases always: for the island of Mauritius lies in a few degrees north of the tropic of Capricorn. In this wood I am persuaded there are four sorts of rising vessels. GGG are great rising vessels, whereof some appear to have contained a fluid matter, which in drying seems to have closed them together in several places, as II, fig. 15, where one of the vessels is plainly exhibited at length.

Fig. 16 is likewise one of the greater vessels, magnified more by a yet greater glass, which was much more perspicuous than the former, beset also with exceedingly small parts. The 2d sort of rising vessels, which are placed generally between the horizontal vessels ABC and HH, are likewise in many places partly filled with black matter. The 3d sort of rising vessels, which follow the circumference of the tree, are B and CDE. The 4th sort of rising vessels are those which go checkerwise between the great rising vessels. These vessels are of a firm wood, in proportion to the vessels of other wood; for the small round which is placed in the white is only the openness of every rising vessel, and the white wherein the round is placed is the wood that forms the vessel; and these vessels are so close and firm joined to each other, that they seem to be but one, as if we should imagine that small holes were bored regularly in firm wood.

Fig. 15, KKK are the horizontal vessels cut across through, of which I have represented 2 in their length, fig. 14, ABC and HH.

Fig. 15, LL are small rising vessels in their length, cut from each other.

Fig. 17 is a piece of palm-wood, drawn by the same magnifying glass that the Mauritius ebony wood was. Of this wood, though I examined a great deal of it, I could find little difference in the several parts, and therefore I have here contented myself with describing a smaller portion. It consists of 2 sorts of ascending vessels, viz. great vessels, and smaller ones lying among the larger. The great vessels consist also of skins, being beset with small rising particles, as in fig. 18, where the vessels are opened longwise, and represented by EEEE.

The small vessels have their cavities very like those of the small vessels in Mauritius ebony wood, and are described cut longwise in fig. 18, FF.

AB or CD are the horizontal vessels, which in their length show themselves thus, but being cut across they are as GG in fig. 18.

Fig. 19, ABCDEF is a description of a very small piece of straw cut across, in which the part of the circumference AF may be discerned how great it is.

ABEF is the rind of the straw, which to outward appearance is smooth and shining, though for the most part it is made of extremely small vessels, and of some greater, which I have represented as near as possible. GGGG are the vessels of which the innermost parts of the straw are made; these vessels are 4, 5, and 6 sided, according as they come to fit themselves. HHH are other vessels which run in between the forementioned ones, and are beset round with small vessels. In these vessels I have seen the sap sink down suddenly at the time of the growing of the straw, when at the same time I saw the sap rise up in the vessels GG, which sap was made mostly of globules; and when the globules came to pass the valves, where the vessels were narrowest, these globules then changed into the form of cones, till they obtained a larger room, and then they retook their former globular shape.

Fig. 20 are the rising vessels described in their length, being the same with those represented fig. 19, GG. IIII are the places where these valves are, and where the vessels are narrowest.

A Theory of the Variation of the Magnetical Compass. By Mr. Edm. Halley, F.R.S. N° 148, p. 208.

The variation of the compass, by which I mean the deflection of the magnetical needle from the true meridian, is of that great concernment in the art of navigation, that the neglect of it renders almost useless one of the noblest inventions mankind ever yet attained to. Now although the great utility that a perfect knowledge of the theory of the magnetical direction would afford to mankind in general, and especially to those concerned in sea affairs, seem a sufficient incitement to all philosophical and mathematical heads, to take under serious consideration the several phænomena, and to endeavour to reconcile them by some general rule: yet so it is, that almost all the authors, from whom a discourse of this kind ought to have been expected, pass by in silence the difficulties they here encounter. And those that mention this variation by affirming it to proceed from causes altogether uncertain, as are the casual lying of iron mines and load-stones in the earth, put a stop to all further contemplation, and give discouragement to those that would otherwise undertake this inquiry. It is true that not long since one Mr. Bond, an old teacher of navigation, put forth a small treatise, wherein he pretends to calculate the variation: but he limits his hypothesis to the city of London, affirming himself, that the same calculus is not sufficient for other places, whereby it appears that his rule is far short of the so much desired general one.

Now although I cannot pretend perfectly to establish the numbers and rules of a calculus which shall precisely answer to the variations of all parts of the world, yet I suppose it will not be unacceptable to the curious to propose something of a light into this abstruse mystery, which, if no other, may have this good effect, to stir up the philosophical geniuses of the age to apply themselves more attentively to this useful speculation. But before I proceed, it will be necessary to lay down the grounds on which I raise my conclusions, and at once to give a synopsis of those variations which I have reason to look upon as sure, being mostly the observations of persons of good skill and integrity.

A Table of Variations.

Names of Places.	Longitude from London.	Latitude.	Anno Dom.	Variation observed.
London	0° 0'	51° 32' N.	1580	11° 15' E
			1622	6 0 E
			1634	4 5 E
			1672	2 30 W
			1683	4 30 W
Paris	2 25 E.	48 51 N.	1640	3 0 E
			1666	
			1681	2 30 W
Uraniburg	13 0 E.	55 54 N.	1672	2 35 W
Copenhagen	12 53 E.	55 41 N.	1649	1 30 E
			1672	3 35 W
Dantzic	19 0 E.	54 23 N.	1679	7 0 W
Montpelier	4 0 E.	43 37 N.	1674	1 10 W
Brest	4 25 W.	48 23 N.	1680	1 45 W
Rome	13 0 E.	41 50 N.	1681	5 0 W
Bayonne	1 20 W.	43 30 N.	1680	1 20 W
Hudson's Bay	79 40 W.	51 0 N.	1668	19 15 W
In Hudson's Straits	57 0 W.	61 0 N.	1668	29 30 W
In Baffin's Bay at Sir Thomas Smith's Sound	80 0 W.	78 0 N.	1616	57 0 W
At Sea	50 0 W.	38 40 N.	1682	7 30 W
At Sea	31 30 W.	43 50 N.	1682	5 30 W
At Sea	42 0 W.	21 0 N.	1678	0 40 E
Cape St. Augustine off Brasil	35 30 W.	8 0 S.	1670	5 30 E
Cape Frio	41 10 W.	22 40 S.	1670	12 10 E
At sea, off the mouth of the river of Plate	53 0 W.	39 30 S.	1670	20 30 E
At the east entrance of Magellan Straits	68 0 W.	52 30 S.	1670	17 0 E
At the west entrance of Magellan Straits	75 0 W.	53 0 S.	1670	14 10 E
Baldivia	73 0 W.	40 0 S.	1670	8 10 E
At Cape d'Agulbas	16 30 E.	34 50 S.	1622	2 0 W
			1675	8 0 W
At sea	1 0 E.	34 30 S.	1675	0 0
At sea	20 0 W.	34 0 S.	1675	10 30 E
At sea	32 0 W.	24 0 S.	1675	10 30 E
At St. Helena	6 30 W.	16 0 S.	1677	0 40 E
At Ascension	14 30 W.	7 50 S.	1678	1 0 E
At Johanna	44 0 E.	12 15 S.	1675	19 30 W
At Mombasa	40 0 E.	4 0 S.	1675	16 0 W
At Zocatra	56 0 E.	12 30 N.	1674	17 0 W
At Aden, at the mouth of the Red Sea	47 30 E.	13 0 N.	1674	15 0 W
At Diego Roiz	61 0 E.	20 0 S.	1676	20 30 W
At sea	54 30 E.	0 0	1676	15 30 W

Names of Places.	Longitude.		Latitude.		An. Dom.	Variation	
At sea	55°	0′ E.	27°	0′ S.	1676.	24°	0′ W
At Bombay	72	30 E.	19	0 N.	1676.	12	0 W
At Cape Comorin	76	0 E.	8	15 N.	1680.	8	48 W
At Ballasore	87	0 E.	21	30 N.	1680.	8	20 W
At Fort St. George	80	0 E.	13	15 N.	1680.	8	10 W
At the west point of Java	104	0 E.	6	40 S.	1676.	3	10 W
At sea	58	0 E.	39	0 S.	1677.	27	30 W
At the Isle of St. Paul	72	0 E.	38	0 S.	1677.	23	30 W
At Van Diemen's Land	142	0 E.	42	25 S.	1642.	0	0
At New Zealand	170	0 E.	40	50 S.	1642.	9	0 E
At Three King's Isle in New Zealand	169	30 E.	34	35 S.	1642.	8	40 E
At the Isle Rotterdam in the South Sea	184	0 E.	20	15 S.	1642.	6	20 E
On the Coast of New Guinea	149	0 E.	4	30 S.	1643.	8	45 E
At the west point of New Guinea	126	0 E.	0	26 S.	1643.	5	30 E

We must now make some remarks on the foregoing table, and first: that in all Europe the variation at this time is west, and more in the eastern parts than the western, as likewise that it seems throughout to be upon the increase that way.—Secondly, that on the coast of America, about Virginia, New England, and Newfoundland, the variation is likewise westerly; and that it increases all the way as you go northerly along the coast, so as to be above 20° at Newfoundland, nearly 30° in Hudson's Straits, and not less than 57° in Baffin's Bay; also that as you sail eastward from this coast the variation diminishes. From these two it is a legitimate corollary: that somewhere between Europe and the north part of America, there ought to be an easterly variation, or at least no westerly; and so I conjecture it is about the easternmost of the Tercera islands.—3. That on the coast of Brasil there is east variation, which increases very notably as you go to the southward, so as to be 12° at Cape Frio, and over against the river of Plate 20½°, and from thence, sailing south-westerly to the Straits of Magellan, it decreases to 17°, and at the west entrance but 14°. 4. That at the eastward of Brasil properly so called, this easterly variation decreases, so as to be very little at St. Helena and Ascension, and to be quite gone, and the compass point true about 18° of longitude west from the Cape of Good Hope.—5. That to the eastward of the aforesaid places a westward variation begins, which obtains in the whole Indian Sea, and arises to no less than 18° under the equator itself about the meridian of the northern part of Madagascar, and near the same meridian, but in 39° south latitude it is found full 27½°, from thence easterly the west variation decreases, so as to be but little more than 8° at Cape Comorin, and than 3° on the coast of Java, and to be quite extinct about the Molucca islands, as also a little to the westward of Van Diemen's Land, found out by the Dutch in 1642.—6. That to the eastward of the Moluccas and Van

Diemen's Land, in south latitude, there arises another easterly variation, which seems not so great as the former, nor of so large extent; for that at the island Rotterdam it is sensibly less than on the east coast of New Guinea; and, at the rate it decreases, it may well be supposed that about 20° farther east, or 225° east longitude from London, in the latitude of 20° south, a westerly variation begins.—7. That the variations observed by the honourable Sir John Norborough at Baldivia, and at the west entrance of the Straits of Magellan, plainly show, that that east variation noted in our third remark is decreasing apace; and that it cannot reasonably extend many degrees into the South Sea from the coast of Peru and Chili, leaving room for a small westerly variation in that tract of the unknown world that lies in the midway between Chili and New Zealand, and between Hounds Island and Peru.—8. That in sailing north-west from St. Helena by Ascension, as far as the equator, the variation continues very small east, and as it were constantly the same; so that in this part of the world the course, wherein there is no variation, is evidently no meridian, but rather north-west.—9. That the entrance of Hudson's Straits and the mouth of the river of Plate, being nearly under the same meridian, at the one place the needle varies $29\frac{1}{2}°$ to the west, at the other $20\frac{1}{2}°$ to the east. This plainly demonstrates the impossibility of reconciling these variations by the theory of Bond: which is by two magnetical poles and an axis, inclined to the axis of the earth; from whence it would follow, that under the same meridian the variation should be in all places the same way.

These things being premised may serve as a sure foundation to raise the superstructure of a theory upon. But first it would not be amiss to show hereby the mistake of Gilbert and Descartes; the first whereof supposes, that the earth itself being in all its parts magnetical, and the water not, wheresoever the land is, thither also should the needle turn, as to the greater quantity of magnetical matter. Now this in many instances is not true, but most remarkable on the coast of Brasil, where the needle is so far from being attracted by the land, that it turns the quite contrary way, leaving the meridian to lie N b E, which is just along the coast. As to the position of Descartes, that the iron and loadstones hid in the bowels of the earth, and the bottom of the sea, may be the causes that the needle varies; if we consider for how great a part of the earth's surface, ex. gr. in the whole Indian sea, the needle declines the same way, and that regularly; it will follow, that the attracting substance that occasions it must be very far distant. Now by experience we find the little force that iron guns have on the compass in ships, and the experiments now before the Royal Society do plainly show, how little a magnetism there is in most crude iron ores, what quantity thereof must be then supposed to make so powerful a diversion at 2 or 3000 miles distance? Yet I cannot deny that in some places near the shore, or in shoal water, the needle may be irregularly directed from the aforesaid causes, and that not a little, as Gassendus gives a notable instance of the island Elba in the Mediterranean sea; but these differences from the general direction are always signs of the nearness of those magnetical substances, for the production whereof that island Elba has been famous from all antiquity. Besides, against both Descartes and Gilbert, the change of the variation, which has been within this 100 years last past more than 15° at London, is an entire demonstration; though Descartes does not stick to say, that the transportation of iron from place to place, and the growth of new iron within the earth, where there was none before, may be the cause thereof. The same holds likewise against the hypothesis of magnetical fibres, which Kircher maintains.

Now to propose something that may answer the several appearances, and introduce nothing strange in philosophy, after a great many close thoughts I can come to no other conclusion than that, the whole globe of the earth is one great magnet, having 4 magnetical poles or points of attraction, near each pole of the equator 2; and that, in those parts of the world which lie near adjacent to any one of those magnetical poles, the needle is governed thereby, the nearest pole being always predominant over the more remote. The parts of the earth wherein these magnetical poles lie cannot as yet be exactly determined, for want of sufficient data to proceed geometrically; but, as near as conjecture can reach, I reckon that the pole which is at present nearest to us lies in or near the meridian of the land's end of England, and not above 7° from the north pole. By this pole the variations in all Europe and Tartary, and the North Sea are principally governed; though with regard to the other northern pole, whose situation is in a meridian passing about the middle of California, and about 15° from the north pole of the world; to this the needle has chiefly respect in all the North America, and in the two oceans on either side thereof, from the Azores westwards to Japan, and farther. The 2 southern poles are rather farther distant from the south pole of the world. The one, about 16° from it, is in a meridian about 20° to the westward of Magellan's Straits, or 95° west from London; this commands the needle in all the South America, in the Pacific Sea, and the greatest part of the Ethiopic Ocean. The fourth and last pole seems to have the greatest power and largest dominions of all, as it is the most remote from the pole of the world, being little less than 20° distant from it, in the meridian which passes through Hollandia Nova and the island Celebes, about 120° east from London; this pole is predominant in the south part of Africa, in Arabia, and the Red Sea, in Persia, India and its islands, and all over the Indian Sea from the Cape of Good Hope eastwards, to the middle of the great South Sea that divides Asia from America. This seems to be the present disposition of the magnetical virtue throughout the whole globe of the earth. It remains to show how this hypothesis makes out all the variations that have been observed of late, and how it answers to our several remarks drawn from the table. And first it is plain that (our European north pole being in the meridian of the land's end of England) all places more easterly than that will have it on the west side of their meridian, and consequently the needle, respecting it with its northern point, will have a westerly variation, which will still be greater as we go to the eastward, till we come to some meridian of Russia, where it will be greatest, and from thence decrease again. Thus at Brest the variation is but $1\frac{3}{4}$°, at London $4\frac{1}{2}$°, but at Dantzic 7° west. To the westward of the meridian of the land's end, the needle ought to have an easterly variation, were it not that (by approaching the American northern pole, which lies on the west side of the meridian, and seems to be of greater force than this other) the needle is drawn thereby westward, so as to counterbalance the direction given by the European pole, and to make a small west variation in the meridian of the land's end itself. Yet I suppose that about the meridian of the isle Tercera, our nearest pole may so far prevail as to give the needle a little turn to the east, though but for a very small space; the counterbalance of those two poles permitting no considerable variation in all the eastern parts of the Atlantic Ocean, nor upon the west coasts of England and Ireland, France, Spain, and Barbary. But to the westward of the Azores, the power of the American pole overcoming that of the European, the needle has chiefly respect thereto, and turns still more and more towards it as we approach it. Whence it comes to pass, that on the coast of Virginia, New England, Newfoundland, and in Hudson's Straits, the variation is westward: that it decreases as we go from thence towards Europe, and that it is less in Virginia and New England than in Newfoundland and Hudson's Straits. This westerly variation again decreases as we pass over the North America, and about the meridian of the middle of California the needle again points due north; and from thence westward to Yedzo and Japan I make no doubt but the variation is easterly, and half sea over not less than 15°, if there be any truth in this hypothesis of mine. Therefore I propose this as a trial, that the whole may be scanned thereby, and I conceive it will not be hard to know of the Spaniards how it is, who so frequently sail through that ocean, in their return from the Manilla isles. This east variation extends over Japan, Yedzo, East Tartary, and part of China, till it meet with the westerly, which is governed by the European north pole, and which I said was greatest somewhere in Russia.

Towards the southern pole the effect is much the same, only that here the south point of the needle is attracted. Hence it will follow, that the variation on the coast of Brasil, at the river of Plate, and so on to the Straits of Magellan should be easterly, as in our third remark; if we suppose a magnetical pole situated about 20° more westerly than the Straits of Magellan. And this easterly variation extends eastward over the greatest part of the Ethiopic Sea, till it be counterpoised by the virtue of the other southern pole, as it is about midway between the Cape of Good Hope and the isles of Tristan d'Acuntia. From thence eastward, the Asian south pole, as I must take the liberty to call it, becoming prevalent, and the south point of the needle being attracted by it, there arises a west variation very great in quantity and extent, because of the great distance of this magnetical pole of the world. Hence it is, that in all the Indian Sea, as far as Hollandia Nova and farther, there is constantly west variation, at that under the equator itself it arises to no less than 18°, where it is most. About the meridian of the island Celebes, being likewise that of this pole, this westerly variation ceases, and an easterly begins, which reaches, according to my hypothesis, to the middle of the South Sea, between Zelandia Nova and Chili, leaving room for a small west variation governed by the American south pole, which I showed to be in the Pacific Sea, in the 6th and 7th remark.

What I have now said, plainly shows the sufficiency of this hypothesis, for solving the variations that are at this time observed in the temperate and frigid zones, where the direction of the needle chiefly depends on the counterpoise of the forces of two magnetical poles of the same nature; and I suppose I have shown how it comes to pass, that under the same meridian the variation should be in one place $29\frac{1}{2}$ west, and in another $20\frac{1}{2}$ east, as I noted in my 9th remark.

In the torrid zone, and particularly under the equinoxial, respect must be had to all the four poles, and their positions well considered, otherwise it will not be easy to determine what the variations shall be, the nearest pole being always the strongest; yet not so as not to be counterbalanced sometimes by the united forces of two more remote, a notable instance whereof is in our 8th remark, where I took notice, that in sailing from St. Helena by the isle of Ascension to the equator, on a NW course, the variation is very little easterly, and in that whole track unalterable. For which I give this reason, that the South American pole, which is considerably the nearest in the aforesaid places, requiring a great easterly variation, is counterpoised by the contrary attraction of the North American and the Asian south pole, each whereof singly is in these parts weaker than the American south pole, and upon the NW course, the distance from this latter is very little varied; and as we recede from the Asian-South-pole, the balance is still preserved by the access towards the North-American-pole.

Thus I hope I have not lost my pains and study in this difficult subject: believing that I have put it past doubt, that there are in the earth four such magnetical points, or poles, which occasion the great variety and seeming irregularity which is observed in the variations of the compass. But to calculate exactly what it is, in any place assigned, is what I dare not yet pretend to; though I could wish it were my happiness to be able to oblige the world with so useful a piece of knowledge. There are difficulties that occur which render the thing as yet not feasible; for first, a great many observations are requisite, which ought to be made at the same time; not at sea, but ashore; with greater care and attention than the generality of sailors apply. And besides, it remains undetermined in what proportion the attractive power decreases, as we remove from the pole of a magnet; without which it were a vain attempt to endeavour to calculate. There is yet a further difficulty, which is the change of the variation, one of the discoveries of this last century; which shows, that it will require some hundreds of years to establish a complete doctrine of the magnetical system. From the foregoing table it should seem, that all the magnetical poles had a motion westward: but if it be so, it is evident that it is not a rotation about the axis of the earth; for then the variations would continue the same, in the same parallel of latitude, the longitude only changed, as much as is the motion of the magnetical poles. But the contrary is found by experience; for there is no where in the latitude of $51\frac{1}{2}$ north, between England and America, a variation of 11° east at this time, as it was once here at London. It seems therefore that our European pole is got nearer the north pole, than it was heretofore; or else that it has lost part of its virtue. But whether these magnetical poles move altogether with one motion, or with several; whether equally or unequally; whether circular or libratory; if circular, about what centre; if libratory, after what manner; are secrets as yet utterly unknown to mankind; and are reserved for the industry of future ages.

An Account of a Book, viz. Wilhelmi ten Rhyne, M. D. &c. Transisalano-Daventriensis*; 1. *Dissertatio de Arthritide.* 2. *Mantissa Schematica.* 3. *De Acupunctura.* 4. *Orationes tres*; *sc. de Chymiæ et Botanicæ Antiquitate et Dignitate. De Physiognomia. De Monstris. Lond.* 8vo. 1683. N° 148, p. 222.

This author asserts that flatus or wind, included between the periosteum and

* William ten Rhyne, a celebrated naturalist of the 17th century, was physician to the Dutch settlement at Batavia, in the island of Java. Besides the abovementioned dissertation, he wrote other

the bone, is the genuine producer of those intolerable pains afflicting gouty persons; and that all the method of cure ought to tend toward the dispelling of the said flatus.*

He further asserts that head-aches, palpitations of the heart, tooth-ach, pleurisy, convulsions, numbness, epilepsy, colics, &c. arise from the same cause.

In treating of the cure of the gout, he recommends burning, though a severe, yet as an adequate cure to so stubborn a disease; showing that fire, either potential or actual, is the genuine dispeller of the wind, which he considers to be the cause of this disorder.

That the efficacious way of burning has been disused, he ascribes to the soft education of these latter ages, whereby men are rendered averse to a method of cure too harsh and cruel; to sense grown tender by luxury, ease, and pleasure: and therefore he wishes a more gentle mode of cautery could be invented than bare fire; yet commends that before some potential cauteries, whose activity often produces unexpected symptoms, as not being so much at the command of the applier. However, the frequent use of cauteries in Japan appeared sufficiently to our author, from the numerous scars he saw all over the bodies of many persons among that people, on no other account but burning with moxa; which he asserts, they use in all ages and sexes, and in all seasons of the year with desired success.

All the surgery of the Japanese (he remarks) consists in the use of moxa; and pricking with a long sharp needle. They had rather die than implore assistance from christians.

But nature (he continues) is kind in affording great quantities of iomongi, which our author calls broad-leaved mugwort,† growing without culture every where; known to the most ignorant Japanese; and being prepared is called moxa. Our author passing by its other virtues, at present only shows its use in the gout: which, after a digression concerning cauteries, whose efficacy is asserted by Hippocrates, Celsus, and others there quoted, he describes in this manner: the plant dried in the shade, freed from filth and the harder rougher stalks, and rubbed between the hands till it become like cotton, is their moxa.

medical works, such as Meditationes in Hippocratis Textum, a treatise (in Dutch) on Asiatic diseases, &c. He afforded great assistance to Van Rheede, in the composition of that superb botanical work entitled Hortus Malabaricus; (of which an account has been given at pp. 590-591, of this vol. of these abridgements;) and he transmitted many notices concerning East India plants to Jacob Breyne, who inserted them in his splendid publications on rare exotics.

* This is a most absurd theory, derived in all probability from the native physicians of the East; and adopted by them for the purpose of explaining the manner in which they suppose their favourite remedies, burning with moxa and acupunctura, produce their effect.

† A species of Artemisia.

This cotton-like substance they form into a little cone, about the bulk of a pea; or else in a paper roll it between their hands into a cylinder, to be divided into little pellets for their purpose. The pulse of the place being felt, upon it they place the moxa, the basis of the cone next the skin, then taking care to keep the body in a settled position, they light the apex of the pellet either with ordinary wood, or with an aromatic stick. The pellet does not wholly consume into ashes, but leaves a little segment of its base on the part: a little blister is hereby raised, of a cineritious colour, without much pain, giving vent to the humours and wind. They burn sometimes even to 50 pellets on a place without danger; and at last with success. To separate the eschar, garlic is applied for 24 hours. The rough side of a plantain leaf keeps the ulcer open; the smooth side heals it: cole-worts, betes, colts-foot, ivy, &c. are substituted for want of plantain.

Many of the Japanese use moxa twice or oftener in a year to prevent sickness, as solemnly as Europeans purge or bleed. In the most grievous chronic diseases, and even in persons emaciated by consumptions, as an arcanum they burn in four points near the os sacrum, two on each side, lying in a direct line crossing the spine at right angles. Analogous to this practice, in a long digression, the author quotes out of Hippocrates many instances and precepts for burning in most chronic cases.

The whole art lies in designing the points to be burnt for each distemper; which secret is in the hands of particular surgeons, who have formed rules to direct them; as also images in their houses marked in all those places that are proper to be burnt. And to show the necessity of a due observing of proper places, he asserts, on his own knowledge, that burning on the linea alba, a finger's length below the navel, infallibly causes barrenness, especially in men; wherefore that line is always avoided. Aside from it above the navel, burning, they say, restores lost appetite, as also if moxa be applied to the shoulder blades. For a gonorrhæa and weakness of the spermatic vessels, the parts about the loins and the os sacrum are to be burnt; for the colic, on the abdomen; and for the tooth-ach, on the chin at the commissure of the under jaw-bones. With these and such like instances, and a receipt for hysterical lozenges, made of this artemisia or mugwort, of constant use among the women of Japan, our author concludes his discourse.

Mantissa Schematica. Here the author gives four drawings of those images the Japanese physicians keep in their houses, marked in those places which they burn with moxa and perforate with their needle; annexing the inscriptions belonging to those images, which contain a brief account of their physic and anatomy. They use inwardly three plants, much extolled for their virtues, above all others, 1. rockqualiph, bearing fruit like kidney beans, and a root yellow, bitter and odoriferous; 2. xinkiu, a tuberous root smelling like lovage, remarkable among them for its enticing fish by the scent; and, 3. nixziu, whose root they make the basis of all decoctions, in which form they exhibit all inward medicines; neither do they give many more than these named. Their internal medicines are calefactive and discussers of wind; and if those fail, they presently have recourse to moxa or their needle.

De Acupunctura. The needle is made long, slender, sharp, of gold, or at least silver, with a wreathed handle.

It is to be conveyed, either by the hand or a little mallet, into the part gently, a finger's breadth or more, as the case requires, and to be held there for the space of 30 breathings, if the patient can bear it, otherwise repeated punctures are rather used. The puncture must be when the party is fasting, deeper in a great than less disease; in old than young men; in grown persons than in those that are lean and tender; in fleshy parts than in nervous. The needle is chiefly used in diseases of the head and lower belly; and is applied to the head in head-aches, lethargies, convulsions, epilepsy, diseases of the eyes, &c.; to the abdomen in colic, dysentery, want of appetite, hysterical disorders, surfeits, pains of the belly and joints, obstructions of the liver and spleen, &c. The womb itself may be perforated, the Japanese affirm, and the fœtus wounded, when its motions are enormous and threaten abortion. In these cases the needle must be applied to the part whence the distemper arises; to the stronger, on the back; to the weaker, on the abdomen: where the pulse scarcely is perceived, there the puncture must be made in the arms, a little beside the veins. The surgeons keep by them images, wherein all the places in the body proper for the needle are designed by marks. The author himself was an eye witness of the use of this puncture on a soldier, who being afflicted with violent disorders of the stomach, and frequent vomitings, at sea, suddenly relieved himself by pricking a thumbs-breadth deep into four different places, about the region of his pylorus.

In his discourse of chemistry, he asserts its antiquity as far back as Tubal Cain, whence he thinks the Vulcan of the heathens by some small change of letters took his name; that therefore Hermes Trismegistus was not the inventor of it, but learnt it of Abraham in Egypt, &c. &c.

In his discourse of physiognomy, after making some remarks on the vanity of astrology and chiromancy; he observes, that the countenance is the epitome of the whole man, representing (if dissembling intervene not) all the inward passions and motions of the soul: and this upon the account, that the temperament of the body, influencing the manners of the mind, also disposes variously the lineaments, complexion, features, and air of the face. Then describing the differences of complexions, with physiognomical signs thence arising; showing also what effects climate, custom, and dissimulation have, in altering the face; giving a brief account of choleric, phlegmatic, sanguine, and melancholic dispositions; recounting some signs of virtues and vices in men; and lastly, reckoning up the humours and manners of several particular nations, the author seems to have brought together all the most general rules, and to have given the reader a scheme and prospect of the whole art of physiognomy. The work concludes with a discourse on monsters.

Some Observations on the Ruins of a Roman Wall and Multangular-Tower at York. By Mart. Lister, Esq. N° 149, p. 238.

In carefully viewing the antiquities of York, and particularly what might relate to the Roman empire, of which this place had been a seat, and the residence of at least two of the emperors, Severus and Constantine, I found a part of a wall yet standing, which is undoubtedly of that time; it is the south wall of the Mint-yard, being formerly an hospital of St. Lawrence, looking towards the river, and consists of a multangular-tower, which led to Bootham Bar, and some yards of wall, which ran the length of Coning-street.

The outside towards the river, is the most worthy of notice. It is faced with a very small square stone, of about 4 inches thick, and laid in level courses, like our modern brick work; but the length of the stones is not observed, being such as they fell out in hewing: from the foundation 20 courses of this small squared stone are laid, and over them 5 courses of Roman brick; these bricks are laid some lengthwise, and some endwise in the wall, and were called lateres Diatoni: after these 5 courses of brick, other 22 courses of small square stone are laid as before, and then 5 more courses of the same Roman bricks are overlaid, beyond which the wall is imperfect, and topped with modern building. Note, that in all this height there is no casement or loop-hole, being one entire and uniform wall, from which it seems the wall had been built some courses higher after the same manner.

These bricks were to be as throughs, or as it were so many new foundations to that which was to be superstructed; and to bind the two sides together firmly; for the wall itself is only faced with small square stone, and the middle filled with mortar and pebble. And lest it should seem strange, that bricks should give a firmness to stone buildings, Vitruvius testifies, and therefore commends brick building before stone, even for the duration; and therefore in Rome, abatement was always made for the age of stone building; none for that of brick, provided it kept its level, and stood upright upon its foundation; and therefore to excuse it, he at large gives a reason why the Romans suffered not brick buildings to be made within the city of Rome, as a thing not of choice, but necessity, these brick buildings being certainly, in that great architect's opinion, to be preferred: the law, says he, suffers not a wall to be made to the street-ward above a foot and a half thick, and partition walls the same, lest they should take up too much room. Now brick walls of a foot and a half thick, unless they were diplinthii or triplinthii, cannot bear up above one story; but in so vast and majestic a city as old Rome, there ought to be innumerable habitations; therefore when a plain area or building of one story could not receive such a multitude to dwell in the city, the thing itself compelled them to raise the houses higher, and therefore they had strange contrivances of out-jetting, and over-hanging stories and balconies, &c.; which reasons if rightly considered are great mistakes; for at this day it is demonstrated that a firm building may be raised to many stories height upon a foot and a half thick wall. The oversight of the Romans was the vast size of their brick; for the smaller the brick the firmer the work, there being much greater firmness in a multitude of angles, as must be produced by a small brick, than in a right line; and this is the reason of the strength of buttresses and multangular towers, &c.

Those bricks are about 17 inches of our measure in length, and about 11 inches broad, and 2 inches and a half thick; agreeing very well with the notion of the Roman foot, which the learned antiquary Greaves has left us; viz. of its being about half an inch less than ours; they seem to have shrunk in the baking more in the breadth than in the length; which is but reasonable, because of their easier yielding that way; and so for the same reason, more in thickness, if we suppose them to have been designed in the mould for 3 Roman inches.

This demonstrates Pliny's measures to be right, and not those of Vitruvius, as they are extant; which makes me much wonder at the confidence of Daniel Barbarus affirming the bricks now to be found, are all according to Vitruvius and not Pliny's measures; for all that I have yet seen in England are of Pliny's measures; as at Leister in the Roman ruin there, called the Jews Wall; at St. Albans, as I remember, and here with us at York. And to go no farther for arguments than this very chapter of Vitruvius, the diplinthii parietes in Rome were against law, and the single brick wall was only allowed as standard, viz. a foot and a half thick wall, or one Roman brick a length, as was above noted.

Pliny lived sometime after Vitruvius, and being a professed transcriber, and as it appears from this very place, having taken the whole business of brick almost verbatim out of him, and not differing in any one thing in the whole chapter, but in this, or the measure of the didoron; and the bricks demonstrating the truth of that difference, it is but reasonable we should make Vitruvius's longum pede latum semipede, a foot long and half a foot broad, a fault of Vitruvius's copiers.

I shall conclude this discourse with this remark, that proportion, and a plain uniformity, even in the minutest parts of building, is to be observed, as this miserable ruin of Roman workmanship shows. In our Gothic buildings there is a total neglect of the measure and proportion of the courses, as if that was not much material to the beauty of the whole, whereas indeed in nature's works it is from the symmetry of the very grain that much of the beauty arises.

On the Colour and Distribution of the Chyle. By Martin Lister, Esq. N° 149, p. 242.

These thoughts contribute nothing to the elucidation of the subjects of digestion and chylification, and are therefore omitted.

On certain Conjunctions of the Planets Jupiter and Saturn. By J. F. [John Flamsteed], Astron. Reg. et R.S.S. N° 149, p. 244.

Whilst common observers have wondered to see the two superior planets Saturn and Jupiter continue so near each other this whole year, and our astrologers have affrighted them with fearful predictions of direful events to succeed this appearance, the more judicious are desirous to know how often and at what time their conjunctions happen, that by comparing their tables of these planets' motions with the observed appearance, they may be the better able to correct them, and render them more agreeable to the heavens. Examining our ancient ephemerides, I do not find that three conjunctions of Saturn and Jupiter have ever happened in one year's space, since they were first in use, till this present. Those of Moletius, calculated from the Alphonsine tables, indeed make three in the space of 8 months, between August 1563, and April 1564 inclusive. But the ephemerides of Stadius, calculated from the Prutenic, make only one, on the 26th of August, of which Junctinus gives us the following observation, in the preface to his astronomical tables. Anno 1563, Aug. 24, 14h. 30m. P. M. Jupiter on the north-side as it were covered Saturn, which was on the south-side, and both these planets were at the end of 28° of Cancer. Riccioli hence concludes that the planet Jupiter covered some part of Saturn at this time. But without reason, for the words quasi cooperiebat intimate not that the one did corporally cover the other, but rather that there was some small interval between them. The Caroline tables make the visible latitude of Saturn now 11′ 45″, of Jupiter 20′ 10″, both north, the conjunction being some few days past: but because their latitudes alter slowly, we may hence conclude the difference 8′ 25″ to have been nearly their distance at that time, these tables being grounded on the Tychonic observations made within less than 40 years after; and showing the latitudes of the planets well at this time, near 100 years later, we may conclude to have answered them as well then; and, if we consider how small a space the distance of 8½ minutes appear to the naked eye in the heavens, especially between two such bright planets as Saturn and Jupiter, that the Caroline distance agrees very well with the words of Junctinus, and that Riccioli was mistaken.

Their next conjunction, according to Maginus's ephemerides, founded on the Prutenic numbers, was April 29, 1583, in 21° of ♓, the sun being then in 17° of ♉; so that the planets rising before him, in signs of short ascension and with south latitude, this congress could not be observed by Tycho, who was mindful of it, as appears by this note in page 55 of his Historia Cœlestis. May 30, A. M. as soon as we saw Saturn after the conjunction, the following distances between Jupiter and Saturn were taken with a radius, viz. at 1h. 47m. 3° 24′; and at 1h. 50m. 3° 24′.

The same ephemerides show the next conjunction of Saturn and Jupiter 1603, Dec. 14, at noon, in 9° 36′ of ♐; but the ingenious Kepler and our Sir Christopher Heydon found it by observation 7 days sooner, or the 7th day of the same month in the morning, in near 8° of ♐; the planets being then but newly emerged from the sun's rays.

The ephemerides of the learned Kepler, calculated from his own Rudolphine tables, makes the next conjunction 1623, between the 7th and 8th of July, in 6° 46′ of ♌; the planet Saturn being then only 4′ to the north of Jupiter: but this first conjunction in the fiery Trigon, happening under the sun's beams, was not observable.

By the same tables and ephemerides of Eichstade, calculated from them, these planets met again in the 25th degree of ♓, between the 15th and 16th of Feb. 1643, with a degree difference of latitude.

By the joint consent of Eichstade's and our Wing's ephemerides, the same planets were in conjunction again 1663, on the 10th of October at noon, in 13° 30′ of ♐, with 1° difference of latitude. This conjunction was observable after sun-set in our latitude, but I hear not that any one observed it.

In each of these years there happened only one conjunction of the two superiors: nor is it possible that there can be more, except the Heliocentric conjunction fall near the opposition of the sun; for then there may be three, two direct, and one retrograde, as has been within the space of 7 months, between October and May last inclusive, of which the true times are determined from the following observations, of the distances between the two planets, in October 1682:

5^d	17^h	51^m	.... between their centres	°	34′	54″
		54	.... the same repeated		34	48
12	13	49	.... between the centres...............		16	2
		54	.... repeated		16	4
	14	3	.... between the near limbs....		15	22
17	14	10	.... between the centres...............		20	9
		17	.... repeated.........		20	12
		21	.... again		20	14
		25	.... between the near limbs...........		19	44
		33	.... between the remote limbs...... ...		20	37
	15	9	.... Saturn from Castor's heel	48	32	25
		14	.... Jupiter from the same.............	48	45	5
		17	.... repeated	48	45	20
		20	.... Saturn from the same	48	32	20
		50	.... between the centres again.........		20	30
19	15	41	.... between the further limbs		26	2
		45	.... their centres....................		25	37
		47	.... their near limbs		25	11
22	18	25	.... their centres....................		33	19
		29	.... repeated........................		33	26

The distances between the planets were measured with the micrometer and a 16-foot glass; from the fixed stars with the sextant: those of the 12th day by my assistant, myself being then very ill of the stone; the rest by myself.

On the 22d day the planet Jupiter was in consequence of Saturn, something less distant from him then he had been observed on the 5th day, near the same hour. Hence the middle time between these observations is pointed out for the time of their true conjunction. But to determine it more accurately, I shall examine the observations made with the sextant on the 17th day, which being nearest the time are most proper for this purpose.

The correct longitude of the heel of Castor is now ♋ 0° 50′ 42″, its latitude 51′ 40″ south. The latitude of Saturn by the Caroline tables 56′ 20″, of Jupiter 41′ 30″, both north.

By the assumed latitude of Saturn 56′ 20″, and his distance from the heel of Castor, observed and corrected, 48° 32′ 30″, I find their difference of longitude 48° 30′ 37″; therefore Saturn in Leo 19° 21′ 19″.

By the latitude of Jupiter assumed 41′ 30″, and his distance from the star 48°

45′ 20″; their difference of longitude is 48° 43′ 56″, and Jupiter's place in Leo, 19° 34′ 39″.

Hence Jupiter's place in consequence of Saturn's 13′ 20″, with which, and the distance of their centres, observed the same night, 20′ 12″, I find the true difference of their latitudes 15′ 20″; only half a minute different from what I assumed it on the authority of the tables.

The apparent motion of Jupiter from the 14th to the 18th day of October, by an ephemeris exactly calculated, and made agreeable to these observations, is 29′ 16″, of Saturn 15′ 01″, both direct; hence the motion of Jupiter from Saturn in 4 days, is 14′ 15″. I say therefore, as 4 days motion, or 14′ 15″, is to 4 days or 96 hours, so is 13′ 20″, which Jupiter is past the conjunction of Saturn, to 90 hours, or 3 days 18 hours, the time interlapsed since the conjunction; which taken from the 17th day 15 hours, the time of my observation, gives the true time of the conjunction of the two planets on the 13th day 21 hours after noon, or according to the common account, the 14th day at 9 o'clock in the morning. At which time Saturn is with Jupiter in 19° 0′ 7$\frac{1}{3}$ of Leo, with 15$\frac{1}{3}$ more northern latitude.

The Acta Eruditorum Lipsiensia, page 366, make this conjunction to have happened the same day in the same longitude with the 11th star of Leo; whose place they state in Leo 19° 4′, latitude 0° 16′ north, with 14′ difference of latitudes between the two planets. But their observation seems to have been made only by the judgment of the bare eye, without an instrument, which considered, I wonder not that it differs at all, but rather that the difference is so small from this determination.

An. 1683, Jan. 19, viewing the planets, then both retrograde, with the 16-foot glass, I found them approached within a measurable distance of each other; when I began to take the following measures:

1683	h.	m.		°	′	″
Jan. 19,	6	41	.. between their centres		33′	28″
		45	.. repeated		33	24
		49	.. between the remote limbs		33	52
			Jan. 26, both planets being in ☍ to the sun.			
26,	6	3	.. between their centres		15	8
		7	.. repeated		15	6
	7	0	.. between their remote limbs		15	31
		12	.. between their centres		15	5
		14	.. repeated		15	2
		17	.. between their next limbs		14	29
		20	.. repeated		14	21

	h	21m	.. again	°	14′	26″
	9	24	.. Jupiter from Castor's heel	46	18	10
		26	.. repeated	46	18	5
		28	.. Saturn from the said heel	46	8	50
		30	.. repeated	46	8	55
		37	.. Jup. from the bright star of the Lion's head E.	8	42	5
		39	.. repeated	8	42	5
		40½	.. Saturn from the same star	8	29	35
		42½	. repeated	8	29	40
		48	.. Jupiter from the Lion's heart	8	18	0
		50	.. repeated	8	17	55
		52	.. Saturn from the same	8	29	35
		54	.. repeated	8	29	35
		59	.. the Lion's heart from E in the head	12	58	50
	10	3	.. the heel of Castor from the Lion's heart	54	34	20
		8½	.. the heel of Castor from E ♌	46	24	45
Jan. 30,	5	28	.. between their centres		11	36
		30	.. repeated		11	33
		34	.. between their remote limbs		11	58
		36	.. repeated		12	1
		38	.. between their next limbs		11	1
	5	41	.. repeated		11	0
Feb. 7,	7	37	.. between their centres		28	35
		40	.. repeated		28	34

From observations formerly made, I have determined the true places and latitudes to this present time, viz. of

The heel of Castor	♋	0°	51′	10″	.. Lat. 0°	51′	40″	south.
Bright * in the Lion's head, E.	Leo	16	15	27	9	41	7	north.
Lion's heart	Leo	25	24	45	0	26	20	north.

And from the above recited measures, the true distances of the planets from these stars, Jan. 26, at 9h. 40m. P. M. as follows:

Saturn from the heel of Castor	46°	9′	0″
Jupiter from the same	46	18	10
Saturn from the Lion's heart	8	29	40
Jupiter from the same	8	18	0
Saturn from the bright * in the Lion's head, E	8	29	40
Jupiter from the same	8	42	10

Whence I collect the true places at this time, viz.

Of Saturn, Leo	16°	57′	10″;	latitude	1°	13′	10″
Of Jupiter, Leo	17	7	10;	latitude	1	1	30
Differences of longitude,		10	0;	of lat.		11	40

The retrograde motion of Jupiter from Saturn in 4 days, between the 26th and 30th of this month, by my correct ephemeris, is 12′ 15″. I say therefore, as 12′ 15″ is to 4 days or 96 hours; so is 10′ 0″, the difference of the planets' present longitudes, to 78 hours, or 3 days 6 hours, which therefore added to the time of that observation, Jan. 26d 9h $\frac{2}{3}$, gives the true time of the conjunction, Jan. 29, 16 hours after noon, or according to the common account Jan. 30, at 4 o'clock in the morning. At which time both the planets are in ♌ 16° 41$\frac{1}{5}$′, with 11$\frac{1}{2}$ min. difference of latitude or distance from each other. Which is further confirmed by the measured distances of the planets on the 30th at night before recited.

On the 26th day, at 9h. 40m., the sun's true place was by my tables in ♒ 17° 21$\frac{1}{3}$′; so that he was now about $\frac{1}{3}$ of a degree past their opposition.

Towards the latter end of the following April, the planet Jupiter began to approach Saturn again, both being now direct; the 28th at night, with the 16-foot glass and micrometer, I measured the distances as below:

April 28,	10h	21m	. . . . between their centres	°	32′	35″
		23	. . . . repeated		32	33
		24	. . . . between their next limbs		32	4
		26	. . . . repeated		32	2
May 7,	8	59	. . . . Jupiter from the Lion's heart	10	59	0
	9	1	. . . . repeated	10	59	0
		3$\frac{1}{2}$	. . . . Saturn from the Lion's heart	10	58	50
		5	. . . . repeated	10	58	50
		11	. . . . Jupiter from E in the Lion's head	8	55	35
		15	. . . . repeated	8	55	40
		17	. . . . Saturn from the same star	8	39	40
		18	. . . . repeated	8	39	40
			With the Micrometer.			
		30	. . . . between their centres		15	38
		33	. . . . repeated		15	37
		35	. . . . between their next limbs		15	3
		36	. . . . repeated		15	0
		40	. . . . between their remote limbs		16	2
		42	. . . . repeated		15	58

With the Sextant again.

May 9,	10h 20m	.... Jupiter from B in ♍	38	11	45
	23	... repeated	38	11	4
	26	.... Saturn from the same star	38	10	55
	28	.... repeated	38	10	45
11,	9 28	.... between their centres		20	2
	31	.... repeated		20	2
16,	9 22	.... between their centres—dubious		34	4

From these observations I state the distances of the planets from the fixed stars May 7, at 9h. 5 m. P. M. as follows:

Saturn from the Lion's heart	10	58	50
Jupiter from the same	10	59	0
Saturn from E in the Lion's head	8	39	40
Jupiter from the same	8	55	35

Hence the true longitude of Saturn ♌ .... 14° 27′ 41″; lat. 1° 12′ 46″ north
of Jupiter ♌ .... 14 26 37 ; lat. 0 56 43 north
Difference of longitude .... 1 4 ; lat. 0 16 3

The difference of latitudes something exceeds the distance measured with the micrometer, by reason that the wind then shaking the sextant permitted us not to be so exact as usual, but the difference, being less than half a minute, I esteem inconsiderable.

The diurnal motion of Jupiter from Saturn was now 3′ 15″. Therefore as 3′ 15″, one day's motion, is to one day or 24 hours, so 1′ 4″ the distance of Jupiter from the ☌ with Saturn, to 8 hours; the interval between the observation and following conjunction, which was therefore 17 h. after noon, or according to the vulgar reckoning, May the 8th, at five o'clock in the morning. At which time the true place of the planets is ♌ 14° 28¾′, the difference of their latitudes 15′ 40″, Saturn being so much more northerly than Jupiter.

In all the best esteemed astronomical tables extant, the mean motions of the planet Saturn are too swift, of Jupiter too slow, considerably; hence it came to pass that they made the direct conjunctions some days later, the retrograde earlier, than they were found by observation.

Traité de l'Organe de l'Ouie, par Mons. Du Verney, 8vo. A Paris, 1683. N° 149, p. 259.

This work is divided into 3 parts, the first of which contains an anatomical description of the organ of hearing. The 2nd the use of all the parts of that organ, grounded on the mechanism of the whole. The 3d and last, the diseases incident to this organ, with a full account of their causes and remedies. The whole is illustrated by 16 plates, in which the several parts of the ear are (for the clearer understanding thereof) represented larger than nature.

An Account of two Letters of M. Perrault and M. Mariotte, concerning Vision. Printed at Paris, 1682. N° 149, p. 265.

The occasion of these 2 letters was an observation of Mr. Mariotte's, that any object is not seen when the species light upon the basis of the optic nerve. The experiment on which it is grounded is this: take a piece of white paper of 6 inches diameter, and fasten it on a dark-coloured wall, that it may be level with the eye; take another small piece of paper, and place it towards the left hand, at 2 feet distance from the former, but about 2 inches higher on the wall; if you then remove to the distance of 8 or 9 feet, and close the left eye, fixing the right on the smaller piece of paper, the larger paper will then quite disappear.

It is not at all doubted but the image which should appear falls just on the base of the optic nerve; it is also certain that the retina is to be found in that place, but the choroid not; which gives a very fair suspicion to M. Mariotte, that the choroid is the seat of vision, and not the retina.

The novelty of this opinion has found many opposers, and among the rest M. Perrault, whose arguments in the first letters are in short reduced to 3 heads. 1. If the choroid were the seat of vision, its function would be hindered by the branches of blood-vessels lying in the retina. 2. The choroid should not be rugged and unequal, nor hard and thick, nor have a slime or dirtiness on it, to hinder the impression of light, nor want a communication with the optic nerve. 3. If the want of vision in the foregoing experiment may be solved by any of the two probable reasons here offered, then there is no need of discharging the retina.

To the first of these M. Mariotte answers: that there are defects in vision, caused by the blood-vessels in the retina; but these defects are not sensible when we look with both eyes, for there are no vessels that lie so near the optic axis as to hinder a direct view, and in an oblique, one eye helps the other, it being difficult for the rays to fall on a like plane in each eye. Again, these vessels that are nearest the optic axis are no thicker than a hair, or the 240th part of an inch, and being in the surface of the retina, are at some distance from the choroid, so as to let rays enough pass under, for the distinguishing of objects not very small. The vessels also that carry the blood are clear and pellucid, causing a refraction that is helpful to vision. Here also may enter some general considerations: as, that the impression of a luminous object remains some time in the organ; that some fibres being strongly moved, others near them are also in motion; that the eyes are always in motion, and very hard to be fixed in one place, though it were desired.

To the 2nd head he answers, that the concavity of the choroid cannot be very rugged; for, on dissecting an eye and removing the retina, the surface of the choroid has reflected an object as distinctly as a concave speculum. That there appears no soil or dirtiness till the outward cuticle is broke, and then the organ is disordered. As for the thickness of it, he says, he finds it in a man but as a sheet of paper, or the pia mater in the brain. That the blood-vessels are woven together with the nerves, on which account there may be as true a sense of light in them as there is of pain in the hand when it is pricked with the point of a needle; and perhaps the presence of veins and arteries in a member is absolutely necessary to sensibility.

To the 3d head, where M. Perrault gives reasons why there is no vision on the base of the optic nerve, as first supposing that vision is to be made on a smooth surface, the optic nerve, which is a bundle of fibres, is not smoothed at its first entering the retina, but afterwards when the fibres are dissolved, and spread into a coat, as when rags are made into paper. Here M. Mariotte, if I rightly comprehend him, denies that the retina consists of fibres, affirming it to have nothing but a mucousness, with some veins and arteries. But in an experiment of Dr. Briggs's, a retina put into a glass of fair water, and drawn about under water, both for the expanding and magnifying it, appeared plainly to have a fibrous texture, like that of a piece of very fine lawn. In the 2nd place, M. Perrault supposes that the choroid being pierced by the optic nerve, there may come a light through the parts of the eye, the back way, into the optic nerve, which would spoil the sense of another light coming through the pupil. But this M. Mariotte will by no means agree to.

Historia Naturalis Helvetiæ Curiosa. Authore Joh. Jacobo Wagnero, M. D. Tiguri. N° 149, p. 268.

The author professes that he undertook to write the natural history of Switzerland on the invitation of my lord Bacon, and with an intention thereby to promote a true experimental philosophy. The work is divided into 7 sections, treating of the limits and nature of the soil; of the Alps, the height, the ice, snow, waters, &c.; of the lakes, rivers, cataracts, &c.; of the people, beasts, birds, fishes, insects, serpents, trees, plants, fossils, metals, &c.

Johannis Jacobi Zimmermanni Cometo-scopia; or, three Astronomical Relations concerning the Comets that have appeared in the Years 1680, 1681, 1682. *Printed at Stutgard in 4to. Anno* 1682. N° 149, p. 272.

The author of these descriptions divides every relation into 3 parts: first an historical account, when and how the comets appeared, and in what manner he observed them; secondly, an astronomical calculation of their places and motions; and thirdly, an astro-theological prognostic of their effects. The prognostic which he gives, is grounded on the vulgar supposition, that comets are signs of such mischiefs and miseries as happen to men after a dissolute and irregular life. As for the natural cause of the first comet, he thinks, that in the same manner as the great conjunction of planets in Sagittarius produced a comet in 1663; so, by a new conjunction of Sol, Venus, Mercury, and Luna, in the same sign, and in opposition to Jupiter, the like effect might be taken notice of. But about the substance and original cause of comets, he has no mind to say any thing, being doubtful what they truly are, the dispute of their parallax not being yet fully decided, and so of their matter, whether ethereal or elementary, the question not resolved. But to show the agreement of this comet with others that have been observed, he has composed a convenient table, containing a list of all comets, that ever have been described by historians and astronomers; setting down first, the year before or after Christ when they have appeared. 2. The place or country where they have been seen. 3. The authors who have mentioned them. 4. The month or time of the year when first seen. 5. The time of the day, whether in the evening or morning. 6. The name and shape of the comets. 7. The situation, or to which part of the horizon their motion proceeded. 8. The whole arch or quantity of degrees which they ran through. 9. The number or quantity of days which they were seen. 10. The degrees of their swiftest motion. 11. The degrees of the length of their tail. 12. The direction of their tails to any part of the heaven. And 13. The effects, mischiefs, and strange accidents, that have happened after their appearance. Among these some have been observed in the shape of the sun, some of the moon and Venus, sometimes there have been 3 or 4 comets together, as in the year 843 and 1529, &c. So that the number of all in the space of 4000 years amounts to 370 comets. To which now must be joined another, or the third comet that was seen in the year 1682, in the month of August.

A further Account of some Rock-plants growing in the Lead Mines of Mendip Hills, mentioned in the Philosophical Transactions, N° 129. *By Mr. John Beaumont, Jun. of Stony Easton in Somersetshire.* N° 150, p. 276.

The particulars set forth in these figures, more than what have been observed by others, in reference to those rock-plants, are as follows:

1. A curious radix, somewhat more entire than elsewhere to be found, on which those rock-plants sometimes grow, though it be manifest that they often grow also from plain roots.

2. Several diversified tops of other radixes.

3. The manifest tapering of those rock-plants.

4. Bores of those rock-plants, with 4, 6, and 7, inlets in them; with other differences in their rays and jointings.

The particular explanation of the figures is below, though it be but short, and not so full as the descriptions I have given of those rock-plants in my former letters, to which I refer the curious reader.*

The Explanation of the Figures, plate 21.

Fig. 1, an entire radix, aa foreseams in 2 of the feet wanting in Mr. Lister's figures. 2. The same radix inverted. 3. A broken piece of a radix with rays on the top. 4. The inside of the same. 5. A plate of a radix with the impression of an oval joint on the top. 6. The top of a radix with rays, and the plates curiously wrought. 7. The inside of the same. 8, 9, 10, 11, 12. Other tops of radixes. 13, 14, 15, 16, 17, 18, 19. Several sorts of plates that compose the radixes. 20. A rock-plant growing from a plain root, branched several ways and tapering. 21, 22. Two trunks of rock-plants tapering, and with marks of branches torn off. 23. A rock-plant with oval joints growing twisting. 24. Another plant with oval joints growing on a scabrous root. 25. A trunk of a rock-plant growing without joint, like coral. 26. Sprouts of mineral coral growing in a heap together. 27. An intorted heap of rock-plants growing on a piece of limestone rock, somewhat resembling the others, being thick set with edges, but having no joints. 28, 29. Two pieces of the summitates or fastigia, mentioned by Mr. Lister in the Philosophical Transactions, N° 100. 30, 31. Two single joints of rock-plants with 7 inlets in the bore. 32, 33. Two single joints with 6 inlets in the bore. 34, 35, 36. Three joints with 5 inlets in the bore. 37, 38. Two single joints with 4 inlets in the bore. 39. A single

* The petrifactions described in this paper are of a similar nature with those described in a former paper by Dr. Lister; they all belong to the coral tribe, and the reader is referred to the above-mentioned paper for an explanatory note.

oval joint, the oval in the upper part of it, standing clear contrary to the oval in the lower part. 40, 41, 42, 43, 44, 45, 46, 47, 48, 49, 50, 51, 52, 53, 54, 55, round and oval single joints, differing in their rays and other ways of jointing.

Extract of two Letters from Mr. Sampson Birch, of Stafford, concerning an Extraordinary Birth in Staffordshire, with Reflections by Edw. Tyson, M. D. F. R. S. N° 150, p. 281.

The wife of one Taylor, in Heywood, Staffordshire, about 24 or 25 years of age, being married about a year, in January last fell in labour, and not being able to be delivered, after 5 or 6 days, the child being dead, the surgeon brought it away by strength with instruments, and afterwards the after-birth; but then perceived that there was still remaining something besides, which lay separate from the child and after-birth, but very firmly adhering to the womb, so that in separating this, it was much more difficult and painful to her than bringing away the dead child, and occasioned a large flux of blood.

The thing itself was sent by Mr. Birch to Dr. Plot at Oxford, and by him to the Royal Society; so that having an opportunity of observing it, I shall here give not only a figure of this strange body, but add to the accounts of it in the letters what I think material. But must premise that it was mentioned in them, that the child was perfectly formed; that the mother was since recovered and walks abroad; that before marriage she was never troubled with any remarkable distempers; and that this body I am going to describe was not observed to be included in any cystis, the secundine being all brought away before it.

The size and shape of this preternatural body will be easily conceived by the figure (pl. 18, fig. 7) which is made as exact and large as the thing itself. In the upper part was a round protuberant bone, 3½ inches in compass, covered with a thick fleshy skin, beset with short hairs. On the top of this bone in a circle were placed 8 dentes molares, or those double teeth we call grinders. These so exactly resembled teeth as to their shape, whiteness, hardness, and in all other circumstances, that they can certainly be nothing else. A little below this, in another bone, which was fastened to the former, were placed 5 other teeth, or dentes molares; these were not so in a cluster together as the former, but 4 of them made almost a straight line, with some distance in the middle, and the 5th a little out of rank, being placed below the 2 uppermost.

The remaining part of this monstrous body, composed a large cystis or bag, filled with a liquid slimy matter, but not fœtid. This cystis on the outside was smooth, appeared somewhat red, and was about the thickness of the scrotum.

But farther to increase the wonder of this so extraordinary a birth, we observed a little below the bone first mentioned, a large lock of hair of a bright brown-colour, having its end intricated and entangled in a large quantity of hair of a more faded and yellowish colour, which was fastened to the end of the cystis opposite to the teeth. But that this lock of hair was of a considerable length, we easily guessed by the several small curls observed in this yellowish hair, which were of the same bright colour with the former lock.

In the middle of the circle of the 8 teeth I observed a small hole, but which did not lead far; and though these teeth, hair, and a strong imagination might create a fancy of some monstrous head here designed to be formed, yet considering how many and far more considerable parts were wanting, I cannot but rather choose with the sceptic ἀπέχειν, and suspend my belief thereof, or with Pliny to say, hoc nobis miraculum, sibi ludibrium ingeniosa finxit natura.

What most I can parallel it with are those instances I have formerly given in the Philosophical Collections, N° 2, of teeth, bones, and hair, I have met with in the ovariums of women; as in one a dens caninus, or eye-tooth, on the outside of the cystis, and within, a large quantity of a fine white long hair, as in fig. 8, pl. 18. In a second woman 3 dentes molares, or grinders, perfectly formed, and a 4th budding forth, all set in their distinct sockets in a bone, as in fig. 9, and in this cystis a great deal of yellowish hair. In a third woman, dissected by Dr. Sampson, in each testicle (ovarium) was a large tumor or cystis, which inclosed a very large quantity of hair, and there was fastened to the sides of them 2 bones, represented in fig. 10 and 11, which though not teeth, yet as to their substance, though not shape, approached something near them; and a like instance, Dr. Needham has informed me, he has met with, of teeth and hair in the same part.*

But our present instance differs from the former, in that this was in the womb, and firmly adhering to it, the others in the ovarium. In this the hair was on the outside the cystis, and rooted in its tunicle; in the others it was contained within it. But as the child, which was perfectly formed, and with much difficulty at last was brought from this woman, I doubt not at first being included in the egg, descended from the ovarium: so likewise this subventaneous egg, I question not, might be transmitted from the same place; and nature, who is never idle, being disappointed of forming in this a perfect fœtus, made the best of what the matter would afford, and might produce the teeth, bones, and hair, which may be reckoned as animal vegetables.

* A case of this sort is also recorded in the 79th vol. of the Transactions by Dr. Baillie, whose reflections hereupon are well deserving attention.

How liable these parts in women are to tumors, there is none who have been conversant in morbid dissections but must be sensible, insomuch that those frequent instances I have met with have fully persuaded me that there is no part in the body so often the seat and causes of dropsies in women as the ovarium; a too luxuriant afflux of humours easily extending these minute eggs into large and sometimes most prodigious cystises, that sometimes I have taken some gallons of water out of them, where I have found them entire, though most times the vast quantity of waters bursts the membrane, and so it empties itself into the cavity of the abdomen.

Extract of a Letter from M. de S. Maurice, M.D. to M. de la Closure, a Physician of Aubeterre, April 26, 1682, concerning the Formation of a Fœtus in the Testicle (Ovarium); taken from the Journal de Medicine of Jan. 1683, by M. l'Abbe de la Roque, at Paris. N° 150, p. 285.

I think, sir, that after what lately happened to Madam de St. Mere, we ought no longer to doubt of the formation of the fœtus in the testicles (ovaria) of women, and consequently of the existence of eggs. This lady had been safely brought to bed 8 times, and after having continued 5 years without being with child, about 3 months since she suspected herself to be fallen into that condition again. But on the 22d instant, after she was up in the morning in very good health, she fell into faintings, which made her lose absolutely her pulse from that moment, without depriving her of her understanding or speech. In the evening I found her cold and wholly without any pulse, her countenance deadish, and covered with a clammy and cold sweat, having still an entire understanding and her speech strong. She complained of colicky pains, which were quickly followed by all the forerunners of an imminent travail; she called her surgeon and died in his arms.

By desire of Mr. de St. Mere the body was opened. In the epigastric region all the entrails were found floating in blood. I caused 2lb to be taken out with a spoon, to avoid changing the situation of the parts, after which, seeing that there remained in the right flank a prodigious quantity which was coagulated, I tried myself to take it out with my hand, but was surprised, when among the first clots which I drew forth, I found a little fœtus about the size of a thumb, and a third less in length, all very distinctly formed, and in which was manifestly discovered the sex of a boy, but naked and without covering. Two fingers from the same place I found the right cornu of the womb; but my amazement was doubled, when I found the testicle (ovarium) torn longwise and through the middle on the side, that it did not touch the tube, and all its cavity full of clotted blood. I no longer doubted that this was the place where this infant was formed, and I conceived that having acquired in this place a growth too great to be able to fall in time; and having continued to grow there, without being able to come forth, it had at length broken its prison by stretching it.

I was confirmed in my opinion when comparing this testicle (ovarium) with the left, I found it at least four times larger, its size approaching that of a hen's egg; and the left being not greater than a little chesnut; it was all red without and within, besides the clotted blood that it contained; whereas the left was pale and full of little grains of the colour and consistency of yellow tallow.

I examined the tube on the right side; and I could not find that this infant had ever entered there; it was in all things like the left tube.

Lastly, I examined the body of the womb with all the care and exactness that might be. It appeared to me every where without any rent, and in a state purely natural: I only observed, that it was a little larger and softer than it is found in women who die without being with child: it was all as Dr. Harvey has described it, in the first month of pregnancy. I caused a probe to be put into its cavity by the vagina, and then to be cut open, and I found not the least sign of conception. Indeed the vessels of the interior membrane appeared to me full of blood.

Authors speak of certain fœtuses found in the tubes; and of others that have been found in the cavity of the belly, neither the womb, nor the tubes being any way torn: but I do not think that any person hitherto has been able to show, that the conception is made in the testicle or ovarium, as it seems to me that the fact, which I have now related, manifestly demonstrates.

An Account of some Experiments made at several Meetings of the Royal Society. By Fred. Slare, M. D. F. R. S. and one of the Coll. of Physicians, with some short Applications of them to physical Matters. N° 150, p. 289.

Expt. I. *A Parallel betwixt Lightning and Phosphorus.*

In order to keep the solid phosphorus from wasting, I usually placed it at the bottom of a glass of water. Having several of these glasses disposed on a table in view, whilst I lay on my bed, I could observe several flashes of light that successively passed through the water, and made such bright and vigorous coruscations in the air, as would surprize and affrighten one not used to the phenomenon. This fiery meteor passes somewhat contracted through the incumbent water, but expands itself as soon as it gets above it. To make these experiments to advantage, the glass ought to be deep and cylindrical, and not above 3-4ths filled with water.

If we compare these appearances with lightning, we may observe that lightning, which comes at intervals, passes uninterrupted the most condensed clouds, and is not extinguished or obstructed by the greatest storms or cataracts of water; but like the beams of the sun, or any other fire, freely passes through glass and water.

This phosphorus in the abovementioned state emits those flashes of light only in warm weather, a certain temper of the air being necessary to produce the effect, for in the winter or cold weather I never observed it.—The warm season of the summer is most productive of lightning.

The abovementioned flash of light is not apt to kindle or burn any combustible matter, as I found by holding my finger in it unmolested; but not trusting to that, I held in the flame, paper, flax, and such materials as are apt to take fire, which it did no more than when we projected the light of the moon by a concave glass on the like bodies.—Such an inoffensive flame that of lightning is generally observed to be. But

The matter of the phosphorus, whilst in a more condensed body, will easily be accended by the warmth of the air, or by the immediate beams of the sun, and then will burn very furiously, with such a penetrating fire as will not easily be extinguished.—So lightning, when condensed or contracted, and wrapt up in a vehicle of air, so that it does not so easily diffuse itself through the yielding æther, will then set fire to houses, trees, &c. and do great mischief.

The phosphorus, whilst burning, acts the part of corrosive; and when it goes out it resolves into a menstruum that dissolves gold, iron, and other metals.—Thus lightning melts down gold, iron, lead, and other metals.*

EXPT. II. *By mixing two Liquors, actually cold, to produce such sparkling and fiery Bodies, as are not only visible in the Dark, but at Noon-Day, in the enlightened Air.*

This experiment was thus made. We took between 10 and 20 grains of the solid phosphorus, and caused it to melt in as much water as would just cover it, which was about a drachm: after it was actually cold we poured it into 2 oz. of oil of vitriol; the mixture being well shaken together, it first heated, and then threw up such fiery balls, that like so many stars adhered to the sides of the glass, and continued to burn for some time.

EXPT. III. *By the Addition of an Oil to the foregoing Mixture, to produce a Flame.*

This is done by only adding a small quantity of oil of turpentine. Without shaking the vessel, the mixture takes fire, and burns very furiously. This

* Modern discoveries have furnished a more satisfactory phenomenon to compare with lightning, namely, that of electricity.

experiment ought to be made in an open vessel, where the air has free access. This succeeded with oil of petroleum, and de lateribus: but sallad oil and spirit of wine could not be made to flame.

The ingredients that compose this burning mixture, are apart cold to the touch, and some of them in their operations: thus, water and oil of vitriol are cooling in their nature; but these in conjunction cause a great heat, which soon excite the agile particles of the phosphorus to an actual fire; and this meeting with an inflammable ingredient, such as oil of turpentine, or the like, produces as considerable a flame as boiling oil is wont to do.

EXPT. IV. *Being a Refutation of Borrichius's Experiment that pretends to Accension.*

We took, according to direction, 4 oz. of fresh drawn spirit of Venice turpentine, to 6 oz. of aquafortis, newly drawn and very strong. We mixed them together in a glass vessel, and placed it in the sun beams. After half an hour's patience, the liquors began to ferment furiously, insomuch that a very great smoke was raised by this means, which was directed to be kept down by a cork that stopped the vessel. This condensed red fume represented flame, by reason of the beams of the sun, that were permitted to shine upon it: but I was assured that this was a great fallacy. I was willing to give the experiment every advantage, which made me perform it where the beams of the sun were admitted; but this very circumstance giving ground to the mistake, I desired leave to make the experiment in a dark room, where we should better discern any real productions of light; being assured, that the action of the liquors would as certainly succeed in the darkened room as in a light one. The experiment was repeated, and the action of the liquors was no less vigorous than in the former instance. And flax, being considered as a very combustible matter, was suspended in the fume: but none of the persons present could discover the least spark of fire or glimpse of light; so that the flax remained untouched, and the fermenting liquors gave no light, fire, or flame. Only take this caution; keep your candles at a distance, for the fume will soon take fire at any actual flame, and set the liquors a burning, and so it may impose upon the careless.*

* Borrichius's assertion respecting the inflammation of oil of turpentine by aquafortis, (nitrous acid) is not disproved by this failure. Supposing Dr. Slare to have employed a well rectified oil of turpentine, and a concentrated, smoking nitrous acid; yet his manner of mixing the oil and acid together might not have been proper. It is not stated that the acid was poured upon the oil at different intervals, and not all at once; though this circumstance is requisite to the success of the experiment. Certain it is that oil of turpentine, like other essential oils, may readily be inflamed by

Expt. V. *Being a new Experiment with Ebullition and Incalescence.*

Among the various mixtures, wherein great heats and effervescences with much ebullition were produced, none were so considerable as this, which was also shown at the same meeting. On 1 oz. of spirit of nitre, if you pour 2 or 3 drachms, gradually, of the highest rectified spirit of wine, the heat and ebullition will be exceedingly great. And whereas in the former experiment you must wait some time for the effervescence, here it is performed in an instant. So that I had more reason to expect, from the mighty action of these liquors on each other, a production of actual fire or flame, than from Borrichius's experiment. Possibly some may be ready to imitate this experiment, which may fail them, unless they observe some little directions. It must be the red spirit of nitre, and a very high rectified spirit of wine. In the next place, you must first pour into the glass the spirit of nitre, which is the heavier liquor, and then the spirit of wine after; for if you invert the order, there will be no ebullition.

Expt. VI. *Of Cold produced without Ebullition, giving some Account of Hysterical Paroxysms.*

In about a pint of water we dissolved a quarter of a pound of sal ammoniac; when the solution was found to be so very cold to the touch, that we needed no weather glass (thermometer) to convince us of the effect.*

Expt. VII. *Of Cold, produced by a very great Ebullition; wherein the cold and hot Fits of an Ague are resembled by a mixture of Liquors.*

If we use in this experiment any acid, whether of vinegar, verjuice, wood-sorrel, oranges, lemons, or perhaps yet milder ones; by casting into these juices a volatile salt of human blood, I always observed a notable ebullition would ensue, which I never could find would heat as such boiling liquors are apt to do; but on the contrary, affected a good weather glass (thermometer) so as to make the liquor descend, which was a manifest token of cold. Here I found that the higher the acid was, the greater the ebullition and the cold

commixtion with a concentrated nitrous acid. This effect, however, may be greatly promoted, by adding to the nitrous acid a small quantity of strong vitriolic acid.

* In a manner by no means satisfactory to philosophers or physicians, the author endeavours to account for the paroxysms of hysteria, upon the supposition that the salts of the blood being diluted by a super-abundance of lymph, a cold effect is produced, as in the abovementioned solution of sal ammoniac in water.

would be, which is very remarkable. For this very reason I made use of very strong vinegar, dephlegmed by freezing; by this mixture we came much nearer the freezing point. But since it proves troublesome to prepare this vinegar, and because it can be done only at certain times, I have most commonly made the experiment with spirit of venus, or verdigrise, which is the highest vinegar in the world: with this the cold will be most sensible to the touch, and most conspicuous on the weather glass (thermometer.) For by this mixture I have in summer made a weather glass (thermometer) to descend below the temperament of cold fountain water, six inches at least, which brought it so near the freezing mark, that it scarcely wanted half an inch.* But at the same time the liquor swells and takes up more room than before, and will not be contained in shallow vessels. In this experiment we have some things very rare, that a great and violent motion of two dissenting liquors should be so far from producing heat as to produce a notable degree of cold, and that too with a considerable expansion of their parts. Here we might instance in an apposite and as unexpected an experiment, where an effect contrary to our common observation happens; and that is thus, if you mix with oil of vitriol a quantity of water, a great heat will follow, without an expansion of these liquors.

The Anatomy of Plants: with an Idea of a Philosophical History of Plants: and several other Lectures, read before the Royal Society. By Nehemiah Grew, M. D. F. R. S. and of the Coll. of Physicians, 1682. N° 150, p. 303.

This work, which was begun about 19 years ago, has been carried on by the special appointment of the Royal Society; and by their order is now made public.

The idea prefixed to the anatomy gives us an account of the materials and methods necessary for making a complete history of plants; which are copiously proposed under these five general heads of inquiry, viz. 1. Of those things which are of more external consideration about plants; as their figures, &c. 2. Of their compounding parts; as vessels, &c. 3. Of their liquors and other contents. 4. Of their principles; as salts, &c. 5. Of their aliment; as water and other means of growth. Aiming chiefly at the discovery of the reason of vegetation, and of the virtues of plants. The anatomy is divided into 4 books. In the first, all the observations, except one or two, are made with the naked eye: in order that a proof might first be given, how far it is possible for us to go without the help of glasses, which many ingenious men want; and more, the patience to manage them. It begins with the seed sown, and so proceeds

* The author's attempt to apply this experiment to the explanation of the cold and hot fit of an ague, is in the highest degree unphilosophical, and is for that reason omitted in this abridgment.

to the root, trunk, branch, leaf, flower, fruit, and lastly to the seed also to be sown again; for each, allowing one whole chapter. In every chapter, the anatomy of the part being first given, the uses are subjoined. And there being two chapters for the seed; in the first is shown the manner of its growth; and in the last, the manner of its generation.

The 2d book is divided into two parts. The first contains the anatomy of roots: which is here prosecuted both with the naked eye, and with the microscope. It begins with some special remarks on the motions, and some other properties of roots. Next proceeds to the anatomy of the skin, bark, wood, and pith: giving the description of several sorts of sap-vessels; as also of the air vessels or lungs; and of the bladders and fibres of the pith and all the parenchymous parts of a plant; and showing the elegant and accurate work of nature in all. Particularly, that the pith is a rete mirabile; herein more curious, than that in the brain, as its fibres are not only exquisitely small, but very regularly composed together.

The 2d part, gives a large account of the vegetation of roots: showing how the ground is prepared for it; how the sap is imbibed, and distributed to the several parts; how the several parts are nourished; how they are formed; and how they come to be situated or disposed; how roots obtain their different size and shape; how their different motion, and different age: how the contents of the several parts are made; and how their odours, colours, and tastes. And what is here said, is also applicable to the same parts in the trunk, &c.

The 3d book, is also divided into two parts. The first, contains the anatomy of trunks and branches, being here prosecuted in like manner as the former of roots. Further showing the admirable artifice of nature, in the structure and composition of all the parts of a plant.

The second part, insists on the further clearing of the following particulars, viz. The motion and course of the sap; the motion and course of the air; the structure of the parts; the generation of liquors; the figuration of trunks; the motion of trunks; and their nature, as fitted for mechanical use.

The 4th book, is divided into 4 parts. The first treats of leaves. In which are described, first, their protections and folds: then, those things which appear on their surface; the apparent position of their fibres. Next the apertures, parts, and curious texture of leaves: and the time and manner in which they are formed. Where the author designs an ascent to the highest step in the business of generation.

The 2d part, treats on flowers. In which are described the three general parts of the flower, viz. the empalement; the foliature; and the attire, both the seminiform, and the florid, in which there is a great deal of curious and unexpected variety; which nature has bestowed upon these parts, next to the seed itself, of greatest use. For every plant being ἀῤῥενόθηλυς,* the attire answers to the genitals of male and female both together; and the powder which they disburse, let fall on the uterus, is the sperm of plants. The time also, in which the flower is formed, is observed, not to be the same year in which it appears, as hath hitherto been thought, but the year before. Hereto is subjoined an appendix; being a method proposed for the ready finding, by the leaf and flower, to what sort any plant appertains.

The 3d part, of fruits. In which are described these following, viz. an apple, lemon, cucumber, pear, plum, grape, gooseberry, and some others; which are so many several sorts of the fleshy uterus. Next, of the seed-case or membranous uterus. And lastly, the use of the several parts, both to the fruit and seed, is set down. Particularly, the manner of the ejaculation of the seed in noli-me-tangere.

The last part, of seeds. Wherein we have first a description of the various and elegant figures of seeds. Next, an account of their number, and several motions, and for what purpose they are made. As for instance, in the seed of hart's tongue, and all that tribe, which are shot off with a spring contrived for it. The annual product of these seeds from one plant being about a million; of which, ten thousand are not so large as a white pepper corn. After this, the description and use of the covers of the seed, and of the vitellum; of the several parts of the fœtus or true seed; and lastly, a further account of the rare contrivance of the stone in fruits, and of the three membranes over the seed, in order to the generation and growth of the same. Thus far the anatomy.

The following Lectures are these:—

I. Of the nature, causes, and power of mixture. In which a foundation is laid for these axioms, viz. That the whole business of the material world is nothing else but mixture. That natural and artificial mixture are the same, as also the causes of both. And that therefore, so far as we can govern mixture, we may do what nature does. As in rendering all bodies sociable or miscible, in making artificial bodies, in imitation of those of nature's own production, &c. —II. Experiments in consort, of the luctation arising from the mixture of several menstruums with all sorts of bodies. Being a specimen of a natural history of the materia medica.—III. An essay on the various proportions, wherein the lixivial salt is found in plants.—IV. A discourse concerning the essential and marine or muriatic salts of plants.—V. A discourse on the colours of plants.—VI. A discourse on the diversities and causes of tastes, chiefly in plants.—VII. Experiments in concert on the solution of salts in water.

* The male and female organs are not always within one corolla, or upon one plant.

Excerpta ex Literis Ill. et Clariss. Virorum ad Nob. Ampliss. et Consultiss. D. Joh. Hevelium Cons. Gedanensem perscriptis, Judicia de Rebus Astronomicis, ejusdemque Scriptis, exhibentia, Studio ac Operâ Johannis Erici Olhoffii Secretarii Gedani, Anno 1683, *4to.* N° 150, p. 308.

A work chiefly in praise of the astronomical labours and merits of Hevelius.

Tractatus de Podagra et Hydrope. A Tho. Sydenham, M. D. Londini, 1683. N° 150, p. 309.

The 1st of these treatises contains an admirable description (accompanied with pathological and practical remarks) of the gout, a disease with which the author himself was afflicted. The other exhibits the author's mode of treating dropsies, in which he employed the most powerful evacuants ανω και κατω, interposing opiates, and completing the cure by bitters, chalybeates and aromatics.

Account of the Earthquake at Oxford, &c. Sept. 17, 1683. *By Tho. Pigot, Esq. F. R. S.* N° 151, p. 311.

The season when this earthquake happened is a time in which such effects are most commonly experienced, if we may credit Aristotle, who tells us that they are most frequent in Spring and Autumn; when there is generally a greater abundance of vapours, and a larger quantity of nitre exhaled; all which ingredients may conspire to the producing of an earthquake. For if we consider how capable they are of a large expansion, how forcible they are when rarefied in vessels, closed and placed over the fire; in Æolypiles, from which they break out, with forcible blasts, or in winds, which frequently proceed from the rarefaction of such principles, we may suppose that those vapours, which produce such great commotions in the air, may cause a considerable disturbance in the earth, when pent and locked up.

As to the weather at the time, the latter part of the first week in September was so rainy, that most people were apprehensive of a flood; and on the 9th there fell some very considerable showers in the afternoon; but from that time it cleared up, and to the end of the next week continued very warm and pleasant weather. The 16th, in the evening, was inclineable to frost; and the next morning it was very hard frost for the season, and then about 7 o'clock, the day being very clear and calm, the earthquake happened. The like observations of cold preceding, are in Dr. Wallis's account of an earthquake, N° 10 of the Philosophical Transactions; as also in that of Mr. Boyle, N° 11. The height of the quicksilver in the barometer was as considerable as at any time these three years; which, together with a remarkable calmness of the air, a matter generally considered as one of the circumstances which accompany earthquakes, and by many reckoned among the signs which precede them, may be sufficient to show how free the air was from vapours at that time; and surely the fewer there were above, the more may be supposed below. Ignes fatui were frequently seen a few days before the earthquake; which may be a probable argument, at least, to show how full the earth was then of damps and exhalations; since a stench, that tainted well water after an unusual manner, has on the same account been generally reckoned amongst the signs of an earthquake, and by which it may be predicted: for by this it was that Pherecydes is said to have presaged the earthquake of Lacedemon; and Helmont mentions another who pretended to the same foresight, by tasting the water of a very deep well in the castle of Lovain.

The motion of this earthquake was not of that sort which are termed pulses or successions; such as strike the ground at right angles with a violent shock, or intermittent knocking, so as to raise the earth to a considerable height, or force their way by a breach; but it appears rather to be such a trembling motion, as vibrates and shakes, without altering the position of the earth, and leaves all things in the same posture in which it found them. For it shook the earth with a tremulous and vibrating motion, whose reciprocations were repeated with great quickness. The pulses were a little discontinued, and yet they came so thick that I could not count them, though the whole earthquake continued here scarcely more than 6 seconds of time; and when that ended, the restoring motion, or settling of the building in which I was, seemed to be with a crash.

Now as tremulous and vibrating motions are proper to produce sounds, so was this earthquake accompanied with a hollow murmuring sound, like distant thunder; which sound kept time so exactly with the motion, and was so conformable to it in all respects, that it plainly appears there was the same cause for both. To those that were within doors, it appeared to be more considerable, and as it were in the air above, occasioned chiefly by the shaking of the building. But those that were abroad in the fields and open air, perceived, with a gentle shaking, a hollow murmur towards the surface of the earth, not unfitly compared to the groaning of some planks of elm, ash, or fir, when the application of fire to the wood causes both a trembling and sound.

That there is considerable heat within the earth, is manifest, from the experience of miners working in the deeper grooves, from those hot springs which break out thence, and from fermentations occasioned by mineral spirits, &c. Nor is it less commonly observed, that such heats and fermentations within the earth are augmented by frosty weather; when the steams being more pent up, work more forcibly on each other. And that sounds and tremors may be produced by such heat, though it work only on air, watery vapours, or nitre, included in pores and cavities, appears by several experiments; as that of filling glass bubbles half full with water and nitre, which being set to the heat of the fire, will tremble with a sort of humming sound, and after that break with a great noise and violence.

The extent of this earthquake was but 70 miles, or thereabouts; its largest distance was from S. E. to N. W. the least from N. to S.; a very inconsiderable space, if compared with that which others have extended, as for instance the earthquake which happened in the south parts of Norway, April 24, Anno 1657, 160 miles in length, and as much in breadth; and Kircher mentions one of 200 miles in length.

Another earthquake, far more considerable than the above, on all accounts, happened Oct. 9, about 11 at night. It was noticed in Oxfordshire northwards very much, and it spread over all the midland counties, and extended into Derbyshire, in which, as in the coal counties, it was very violent.

An Account of the Eclipses or Ingresses of Jupiter's Satellites into his Shadow, and such Emersions of them from it, as will be visible at the Observatory at Greenwich in the last three Months of this Year 1683. *By J. F. Astron. Reg.* N° 151, p. 322.

I here send you an account of the eclipses or ingresses of Jupiter's satellites into the shadow, and such emersions of them from it as will be visible at the observatory in the last three months of this year; they are calculated from new tables which I made this last summer, and I hope so exactly that the ingresses of the first will seldom differ above 5 minutes from the time stated, the 3d but little more: the eclipses of the 4th and 2d I dare not promise you shall agree so well, for I find their motions evidently entangled with inequalities, which it will require a long time and many more accurate observations to determine and limit, than I have yet obtained: these eclipses have been esteemed, and certainly are, a much better expedient for the discovery of the longitude than any yet known, by reason that they happen frequently, and are easily observable with a telescope of 12 feet, or for need with one of 8. These ingresses (and emersions also if visible) from the time till the opposition of the ☉ and ♃ happen truly on the right hand, or in antecedence of the planet, but if they shall be observed with telescopes of only two convex glasses, which invert the object, they appear on the contrary or left side of him. If these find acceptance with our ingenious friends, the eclipses of the next year shall be imparted early enough to be published in the December Transactions. I am, &c.*

Observatory, Sept. 22, 1683.

An Account of the cutting out the Cæcum of a Bitch. By William Musgrave,† *LL. B. Student in Physic, and Fellow of New-College, Oxon.* N° 151, p. 324.

The use of the cæcum being still a desideratum in anatomy, I thought it worth while to try, what light the cutting out of that part might afford us, in a matter so obscure. In my first experiment of this kind (1683) my hopes were soon defeated, by the death of the dog, two days after the operation; but I was more successful in a second attempt. April 1683, I took a bitch of about a year old, and opened the abdomen on the right side, in the regio iliaca, passing my knife through the musculus obliquus ascendens, and by the side of the musculus rectus; having found the cæcum, I immediately put up the other guts again into the abdomen, after which I separated the cæcum from the ileum, cutting the membrane which binds part of the former to the latter; then having made a ligature on the artery which comes to the cæcum, I made three or four prick-seams through the sides of the cæcum, at the farther end of it, where it is continued to the rectum, and by thus sewing the sides together stopped the passage of the fæces that way; after this I cut off the cæcum about $\frac{1}{4}$ of an inch from the stitches, and sewed together the new-made lips, entering my needle always on the inside and passing it through the outer membrane, that so the lips might the better touch edgewise, and grow together. The wounds being sewed up, and the bitch tied away, milk was set before her, of which she lapped a small quantity the next morning, and by degrees recovered, so that in 3 weeks she seemed as well as ever; in a little time she grew fat and proud, and the last summer brought a litter of whelps: in 4 months observation I could not perceive any such alteration in her, as might reasonably be imputed to the loss of the cæcum.

Sept. 19, 1683, I caused her to be hanged, and opened her a second time in presence of Dr. Pitt, our professor of anatomy. We found a great part of the

* The calculations are omitted, as quite useless now.

† William Musgrave, a learned physician and antiquary, was a native of Charlton, in Somersetshire, and was educated at Oxford. He was one of the secretaries of the Royal Society, and superintended the publication of the Philosophical Transactions, from N° 167 to N° 178 inclusive. He afterwards removed to Exeter, where he continued to practise as a physician until the time of his death, which happened in 1721, when he was about 64 years of age. He wrote two medical tracts de Arthritide, and several other learned works, chiefly on antiquities; besides various papers inserted in the Philosophical Transactions.

omentum lying in a heap on the right side; it had not recovered its natural posture, since it was put up with the guts at the first opening; the edges of the wound were well grown together; in short we did not find any thing that seemed to intimate the least want, or to supply the place of the cæcum.

A short Account of three Conjunctions of the Planets Saturn and Jupiter, observed at Dantzic, from the end of the Year 1682 *to May* 28, *N. S. of* 1683. *By M. Hevelius. Translated and abridged from the Latin.* N° 151, p. 325.

On Oct. 26, N. S. 1682, at 1 h. 40 m. in the morning, taking the situation of Saturn and Jupiter with my tube and micrometer, when a remarkable fixed star was near them, being, I think, that in the right shoulder of ♌; three of Jupiter's satellites, if not the fourth also, being present. The distances taken were, between Saturn and Jupiter 16′ 44″, between Saturn and the star 38′ 1″, and between Jupiter and the star 27′ 55″; the star being then, according to our catalogues, in 19° 2′ 9″ of ♌, with 20′ 45″ north latitude. On Oct. 30, at 5h. in the morning, the distance of Saturn and Jupiter was found 25′ 5″. Whence it may be concluded, that the conjunction happened several days sooner than Nov. 3, when it is given by the ephemeris and the calculation. This will be still farther evinced by the following observations. For if the conjunction were near, the distance of Saturn and Jupiter would daily decrease, whereas it continually increased. Thus, on Nov. 2, I observed their distance 35′ 21″; Nov. 3, at 1 h. in the morning, it was 39′ 9″; and Nov. 4, it was still greater.

As to the other conjunction, which, according to the almanacs, ought to happen by the retrogressions of those planets, on Jan. 26, this year 1683, the following are some of the principal observations.

An. 1683, Feb.			Dist. ♄ & ♃.		
d.	h.	m.	pts.		
1	6	40	3300 =	25′	5″
2	9	30	2900 =	22	3
3	9	0	2500 =	19	0
4	10	0	2300 =	17	29
5	8	30	2100 =	15	59
6	7	51	1850 =	14	6
7	8	17⅓	1700 =	12	55
8	6	10	1600 =	12	10

Feb. 9, at 9h. receiving the planets in my tube I pursued them with my eye, and found that the conjunction fell in the night before, or between the 8th and 9th of Feb. for now the distance was rather greater than on the 8th, as also on the 11th, at 9h. when the distance was 2000 parts, = 15′ 12″, whereas it was only 12′ 10″ on the 8th day. That the planets were now past the conjunction, manifestly appeared also from the change in their position with regard to the near stars. And the same will farther appear by the continual increase of distance observed on the following days, as in the annexed tablet.

d.	h.	m.	pts.		
12	9	0	2200 =	17′	6″
13	7	15	2550 =	19	24
14	9	0	2900 =	22	3
17	6	0	3750 =	28	30
20	9	0	5250 =	30	12

The following are also some of the principal observations made of the third conjunction in the month of May following, the distances taken in parts of the micrometer as before. From which it is manifest that, as the observed distance continually decreased till May 18, and after that continually increased, the conjunction of the planets took place on that day, and indeed, as appears from the observations of the 15th and 20th days, at the hour of 10 in the morning; which, according to the compilers of the almanacs, ought not to have happened till the 26th, being 8 days later. And the same result was derived from many other observations continued on the following days, both of the distances and positions of these planets with certain stars; the distances being sometimes taken with the micrometer, and other times with the sextant.

May, 1683.			Dist. ♄ & ♃.		
d.	h.	m.	pts.		
8	9	6	......4300 =	32′	41″
10	9	14	......3750 =	28	30
11	9	10	......3450 =	26	13
12	8	45	......3050 =	23	10
13	9	15	......2800 =	21	17
14	9	45	......2550 =	19	23
15	9	30	......2400 =	18	15
16	9	30	......2250 =	17	6
17	9	40	......2150 =	16	10
18	10	0	......2100 =	15	58
20	11	45	......2450 =	18	37
21	9	15	......2650 =	20	9
22	9	20	......2900 =	22	3
23	9	5	......3250 =	24	43
24	10	6	......3600 =	27	22
25	9	30	.....4000 =	30	25
26	11	0	......4453 =	33	50
27	9	25	......4900 =	37	15
28	9	56	......5325 =	40	29

Occultations of some Fixed Stars by the Moon in 1683, observed at Dantzic by M. Hevelius. Abridged and translated from the Latin. N° 151, p. 331.

Anno 1683, Feb. 11. N. S. at 9h, when first the moon came in my eye, the star Regulus was at some distance on the east. So that their conjunction, as near as I could make by a rude estimate, took place about the rising of the moon, at 5 or 6 o'clock. But whether the star had been really covered I could not be quite certain.

Anno 1683, April 2, N. S. I observed 2 occultations and a conjunction of certain stars by the moon, as here follows:

Time by the watch.					
9^h	53^m	30^s	.... Beginning of the occultation of the small star A.		
10	8	30	.... Conjunction of the moon and the little star C, distant from the ☾'s lower horn 4′.		
10	29	36	.... Beginning of the occultation of the small star B.		
10	52	50	.... End of the occultation of the star A.		
11	45	30	.... The altitude of Lyra	31°	44′
11	46	30	.... The same again	31	55
11	47	50	.... Repeated again	32	6

The small star A is not in Tycho's catalogue; but in my new one it is called the following under the bull's horn, of the 5th magnitude. At present its place is 19° 11′ 35″ of ♊, and 4° 43′ 44″ south latitude. The other star B is 19° 17′ of ♊, and in 4° 47′ south latitude. And the star C, which is scarcely to be seen with the naked eye, is now in 19° 9′ of ♊, and 5° 2′ south latitude.

Historical Observations relating to Constantinople. By the Rev. Tho. Smith, D.D. and F.R.S.* N° 152, p. 335.

An Abstract of a Letter from Mr. Anthony Leuwenhoeck, concerning Generation by an Animalcule of the Male Seed. Also on Animals in the Seed of a Frog; with some other Observables in the Parts of a Frog. And on Digestion and the Motion of the Blood in a Fever.† N° 152, p. 347.

Having been solicitous to examine the generation of frogs,‡ on account of their young being like a worm, with a round thick body and a short tail; I was surprised to find that the male was not joined to the female in copulation, but that he only sat upon her, and that he had no membrum masculum; that at the same time when the female cast her eggs or spawn, the male also dropped his seed, which is to be spread under the eggs, in like manner as the seed of fishes that want the membrum masculum is cast under the eggs of the female, that the animalia in semine may conveniently impregnate the eggs. For I hold it necessary that some one of the animals in semine should get into a certain point§ of the yolk of the egg, which point is only fit to receive it, and give it the first nourishment, till such time as the egg comes to be sat on. But if no one animal should find this point, then the egg is unfruitful; and this may be a reason why there are so many thousand more animals in semine masculo than eggs in the female.

In several of my observations I had not found the animals taken out of the testicles and vasa deferentia of frogs to be alive. But on the 1st of April, when frogs were ready to spawn, I took some of the males sitting upon the females,

* So many accounts of this city have at different times been published by travellers and antiquaries of our own and other nations, that it is deemed unnecessary to reprint this paper. Among the latest and most accurate topographers of the capital of the Turkish dominions may be mentioned Mr. Dallaway, formerly chaplain and physician of the British embassy to the Porte, and author of a Description of Constantinople, ancient and modern, published in 1797.

† The observations on digestion and on the motion of the blood in a fever are omitted, as being irreconcilable with subsequent physiological discoveries.

‡ On this subject, the generation of frogs, three writers have distinguished themselves, viz. Swammerdam, Roesel, and Spallanzani.

§ By this point or speck he means the cicatricula.—Orig.

and squeezed their hinder parts, that I might get the seed out of the vasa deferentia; but the animalcules I then found moved but little, because the matter they were in was full of salt particles, which made me judge it to be urine. I then cut open the testicles, and there I found an innumerable company of animalcules, swimming among a sort of ill-shaped particles; these continued alive till the next day, though there was but a small quantity of liquor to contain them. I judge the bodies of the animalcules to have been of the thickness of $\frac{1}{1000}$ part of a hair of my head; they are represented to the best of my skill in fig. 12, pl. 18, where ABC is an animalcule as it lay in the watery matter, and moved itself therein; sometimes the head appeared to be thicker than at other times, and often I could see the body only from A to B, by reason of the thinness of the tail, BC; when the animal moved itself strongly, though the progress were but little, the motion towards the head was like that of a snake, and the tail was cast into 3 or 4 bows. Fig. D is an animalcule lying dead, and stretched out at length; but in this posture I saw but few, for many that were dead lay with the fore part of their body bent in, as in fig. E; others made as it were a half circle, and others had the fore part of their body bent, and moved their hinder parts; these last I took to be ready to die. The number of animalcules in all the seed was so great, that I judge there might be 10,000 of them to one of the female's eggs. The same computation I formerly made of the melt of a codfish; but it must not be thought that all the animals in the melt of the codfish live together, but only such of them as are nearest the passage they are to be cast out of, and which have more moisture about them; the rest of them being more remote in the body, and being encompassed with a thicker matter, are not alive; for though some fishes, as the bream and trout, cast their melt and spawn in 2 days time, yet codfishes are about a month in doing it; in all which time the seed is successively ripened and perfected. So also are frogs by what I have experienced, for the first animals I sought were dead, and though I afterwards found live ones, yet those were dead that lay deepest in the testicle.

It is well known, that when a cock has trodden a hen but once, many eggs are made fruitful; the reason that I give for it is, that many of the eggs in the ovarium have each of them received an animalcule out of the male seed. This animalcule, while the egg is sat upon, does not presently take the figure of a chick, but grows into a disorderly bulk, wherein the heart is first plainly to be discerned. Other fœtuses have a different way of growth; the louse has all its parts, and is a breeder, while it is yet in the egg: the flea shows like globules swimming in a watery substance, it afterwards becomes a worm, then a nympha: the frog is a thick worm till it be of a considerable size: the human fœtus, though no larger than a green pea, yet is furnished with all its parts. I have often endeavoured to discover the animal coming out of the male seed in the egg of the hen, but have been unsuccessful, though some of the globules of the egg were magnified to the size of common apples. This disappointment has put me on the eggs of insects, as the flea and louse, which being very small, may be so much the fitter for this discovery.

The first frog which I anatomised lay on the ground in my way, and seemed so weak through cold that though I kicked it with my foot it would not leap away; this proved a female, in the guts of which I found worms like those in children, of about the thickness of a hair of my head. The blood consisted of flat oval particles, swimming in a clear liquor; these had no colour as they lay singly, but when two of them lay upon each other, as here fig. 13, their colour was stronger. A is an oval of blood partly covered with B a second oval of blood; C is a third oval of blood, covering a part of ACB as at D, and casting a deeper colour, because 3 plates lie over each other; but there was another small oval hard by, represented by E, which showed of a higher red than the 3 plates together. Many of these oval particles were very pleasant to look upon, especially when the moisture wherein they swam, having also globules on the surface as large as $\frac{1}{6}$ of a blood globule, was evaporated; for some had in the middle a faint oval shade; others appeared as if they were made of several ovals of unequal sizes; others seemed to be set round with small globules; others had no globules in the circumference, but several in the middle; these globules, I believe, were at first swimming in the watery liquor under the ovals, though now they cleave to them by accident.

On the plate whereon I laid the frog that I anatomized, I found several animals moving in a watery blood; they were about half as long and half as broad as the oval particles, and about 50 of them might lie in the space of a sand. These I had never seen in the pure blood, nor could I perceive them in the water that came out on reaping the skin from the flesh, or on opening the belly, or squeezing the head of the frog to make it lie quiet on the plate. At length in the month of June I met with some frogs, whose excrement was full of an innumerable company of living creatures, of different sorts and sizes, the largest kind were shaped like fig. F, and of these I judged that 40 might be in the space of a sand. The second sort had the shape of fig. G, which were but few in number. The third sort was like our river eels, as fig. H, and these were more in number than the first, but the whole excrement besides was so full of living things, that it seemed all to move, and I guessed there was not less than 1000 of the third sort in the space of a sand. From hence I concluded that the animals found among the blood might come from my cutting a gut. By the way I observed something of the damage that frogs may do to fish-ponds, for I took out of some of their stomachs 8, 10 or more young fishes.

I examined one of the muscles of the hinder leg of a frog, which consisted of filaments, and those again of a great number of lesser filaments, which had more rings in them than I had formerly seen in the muscular threads of an ox, fly, gnat, flea, or louse. Such numerous rings I have since met with in the filaments of the muscle of a lamb, taken from the rim of the belly, near the hinder leg. From the indentings of these filaments I can not only satisfy myself how the limbs come to stand bent, when the muscles are at rest, but also why we can walk a longer time than stand still; and why our arms when we walk do not hang down at their full length stretched out by our sides, but move backwards and forwards; for if the arms should still hang straight down, then would one muscle be stretched too much, and another bent or drawn up too close; both these things disagreeing with the natural constitution of the muscle.

An Account of a Book entituled Relatione de Ritrovamento dell' Uova di Chiocciole, &c. N° 152, p. 356.

This anonymous author dedicates his treatise to Signor Malpighi. He first takes notice of the opinion of the schools which taught two sorts of generation, either by the seed of the animal, or spontaneous from corruption; and says, the moderns reduce those two to one; and having observed the uniformity of nature in most of her works, have gone yet farther, and ventured to prove that the general universal productive principle of all species is ex ovo. And this he endeavours to pursue in the works of the ancients as well as the moderns, and out of scripture as well as philosophy; but one passage is remarkable out of Hippocrates, in his book de Natura Pueri; where, relating the case of a young woman, who in dancing miscarried of a fœtus of 6 days old, Hippocrates says, it was like a raw egg the shell being first taken off; and therefore in the end concludes that the nature of the oviparous differs not from that of man.

But still there seemed to be much difficulty in the production of insects, till Signor Redi, by many incontestible experiments, proved them to be generated out of eggs; and by that convicted of error the natural histories of Aristotle and Ælian, showing the necessity of a new and more accurate one, which he owns is with much glory pursued by the Royal Society of London, as well as that of Paris and Florence; and particularly makes very honourable mention of Mr. Lister, for his history of spiders and snails; and of Mr. Ray, for being the first discoverer of their being all hermaphrodites, or androgynous, as he terms it.

After this, he says, the thing seemed out of dispute, when a book came out called the Recreation of the Eye and Mind in Observations upon Snails, by Father Philippo Bonani, a Jesuit; it being a pretty large volume containing the natural history of all the snail kind, where he not only doubts of their production by eggs, but also endeavours to overthrow most of Signor Redi's experiments that tended to establish the principle of an univocous generation, to which he prefers the old principle of corruption for the insects.

While he was reading this book, he says, he chanced in taking up some flowers, about the 10th of July, to observe by a border* a cluster of little eggs, which had nothing in them but the white, like the white of an egg; but on a more diligent search he found another cluster, as large as would fill the palm of his hand, out of which came young snails, some having but just broke the shell, others being half out, and others quite out with the testa of the egg fastened to the tail of the animal. He says the eggs were no larger than pepper corns, and mostly white, but those which were ready to break, tending to yellow; they were fastened in a lump by a kind of glutinous water.

Tajaçu, seu Aper Mexicanus Moschiferus, or the Anatomy of the Mexican Musk-Hog,† *&c. By Dr. Edward Tyson, F. R. S.* N° 153, p. 359.

The shape of this animal is such that it may be easily reduced to the swine kind; though it was much less than our usual hogs; for, from the end of the body, where the tail should be, to the top of the head between the ears, was 2 feet 2 inches; from thence to the end of the nose 11 inches; the compass of the body 2 feet; the compass of the neck 16 inches; of the head in the largest place 18 inches; and of the snout 12 inches.

* The common or garden snail is said to deposit its eggs during the summer months, almost as often as once a fortnight: they are laid in shady places, and generally a little below the surface of the mould. From these the young are hatched and completely formed, and with the shells on their backs.

† The animal here described is the *Sus Tajaçu* of Linnæus, or Pecari. It is the only species of hog found in the New World, and is considerably smaller than the common hog, and of a more compact form: it is covered on the upper parts with very strong dark brown or blackish bristles, each marked by several yellowish white rings, so that the colour of the whole appears mottled, and round the neck is commonly a white collar. The head is large; the snout long; the ears short and upright; the belly nearly naked: there is no tail, and on the lower part of the back is a glandular orifice, surrounded by strong bristles in a somewhat radiated direction: from the orifice exsudes a strong-scented fluid, and this part has been vulgarly supposed to be the navel of the animal: the tusks in this species of hog are not very large.

Dr. Tyson in his anatomical description falls, according to Buffon, into a very material error, viz in affirming that the animal has three stomachs, whereas it has in reality but one stomach, parted a little, like that of the tapir, by two strictures or contractions.

What is most particular in this hog, and makes the greatest difference in it from any other animal I know of in the world, is the teat or navel, or foramen rather, on the hindpart of the back. All who mention this animal, consider this as a thing so extraordinary and uncommon, that their amazement has so far clouded their reason, as to betray them into most extravagant conjectures and opinions concerning it. Not any one that I have met with affording the least glimmering of a probable truth.

These animals are found in Panama, and New Spain; in Nicaragua, in Terra Firma, and in Brasil. They are usually met with in the mountains and woods; and go in herds together. They feed on roots, acorns, and fruits; but, as the greatest delicacy, they hunt for all sorts of poisonous serpents and toads; and having caught them, holding them with their fore feet, with a great deal of dexterity, with their teeth they strip off their skin from the head to the tail, then greedily devour them. And it is said they eat the root or bark of a certain tree, as an antidote against the poison; by which means they are well fed, and grow large. When they are tamed, they will feed on any thing. But naturally they are very fierce.

Oviedus remarks that the swine the Spaniards left on the islands of St. Domingo, St. Joannes, and Jamaica, multiplied, and increased. But those in Terra Firma durst never go in the woods; but were destroyed by the lions, tigers, and lupi cervarii. Yet in these woods, there are great herds of these tajaçus, that can make their party good against the fiercest beasts. If any of them be wounded, he presently gets to his assistance a great number of his kind; and never desists till he has revenged the injury or is slain. They are always at enmity with the tigers. And there is often found the body of a tiger, and many of these tajaçus slain together. If they spy a man, they will fiercely set on him; and his best escape is to get up a tree, which they will most furiously assault with their teeth; nor will they easily leave him till forced by hunger, or slain by him, by clubs, darts, or a gun. It is said, it is usual to take them, viz. by a man's showing himself to them, whom it is known they will presently pursue. If they are hunted, the dogs are often torn in pieces by them. Their flesh is esteemed very good, and much desired by the inhabitants, though they have but little fat.

To come now to the anatomy; having divided the muscles of the belly, what was most remarkable was the structure of the stomachs, of which it had three. Into the middlemost was inserted the œsophagus or gullet; which we may therefore call the first ventricle or stomach. From this, on one side was a large passage into the second; which pouching out had its two ends winding like a horn; and on the other side of the first or middle stomach, was a free open passage into the third, which emptied itself into the duodenum.

The first stomach was lined within with a white thick hard membrane, almost like the inward pellicle of the gizzard of fowls; with which none of the other stomachs were endowed; for the inward surface of the second was smooth and soft, its membranes thin, and more inclining to the common make of that of carnivorous animals. The third somewhat like this, but thicker, and rimpled within, with large plicæ or folds.

Dr. Grew observes, that in the common hog, against the pylorus stands a round caruncle, as large as a small filberd kernel, like a stopple to the pylorus; a part he thinks peculiar to this animal. This in our Mexico hog I did not observe. His conjecture of the use of it is probable enough: it being so voracious an animal, for the preventing a too sudden and copious irruption of the aliment; which is sufficiently provided for in our subject by the great straitening of the pylorus here, and the great ascent it must make before it can go out: which may be the reason too of nature's making these several cells, or partitions, for the better digestion and maceration of the food; for this animal being frugivorous, graminivorus, and carnivorous too, the stomachs are so contrived, as that the first, by its inward pellicle somewhat resembles that of birds, which are carpophagous; so the others, those of quadrupeds.

The small guts, which in other animals being fastened to a large mesentery, usually hang down lower, were here closer gathered, by the shortness of this membrane, to the spine; and the colon, which in others is more suspended, here by its peculiar structure lies loose, and falls down. For the duodenum arising from the pylorus with a short turn, that and the other small intestines made abundance of convolutions and windings; and although the mesentery was but very short from the spine, and its circumference seemingly but very small, yet in this compass it contained 27 feet of these intestines, for so much they measured from the pylorus to the colon. The colon was not fastened to the periphery or rim of the mesentery as usual; but, arising from the centre or middle, made a spiral line, its end hanging loose, and its turnings were closely united to each other by membranes. This colon was very large in comparison of the other guts, and measured 9 feet in length. It had a short cæcum, but pretty wide, and filled with fæces. What Dr. Grew observes, viz. that it is peculiar to the cæcum of a hog, and that of a horse, to have the same structure with the colon, is true here also. And it may be reckoned as an appendix of the colon. In a hog, Dr. Grew makes 7 intestines. The same differences I might perhaps have met with here; but I was prevented by the little leisure I had of being so nice in this, as some other parts; and it being kept so long before I had it for dissection, it was rendered less fit for such inquiries. Falcoburgius says the length of all the guts were 34 geometrical feet; ours measured more. The structure of the colon here seems extraordinary. Some such gut I find in a goat, making several spiral windings in the middle of the mesentery; but then taking a compass round, near the verge, to which are fastened the lesser intestines, at last it passes into the rectum. So in a woodcock there is such a spiral gut. But in our tajaçu not only the stomach, gut, and mesentery were extraordinary, but the mesaraic vessels too; for in men and dogs, &c. making the segment of a circle near the middle, they then send out several large branches towards the intestines; which as they approach them by their mutual inosculation, forming several small arches, from whence issue numerous lesser branches to the guts themselves; but here, in our hog, we observed a large vein and artery, running a small and equal distance from the intestines; and from them arising an infinite number of lesser, but straight vessels; which going to the guts so regularly, and in so great numbers, afforded a very pleasant sight.

The spleen was about 10 inches long; almost of the same breadth throughout; and in the middle was one inch and half broad; it was of a lead colour, a little speckled or marbled. The liver consisted of 4 large lobes; and was of a dark red colour. It appeared plainly glandulous, and had no vesica fellea; which is the more remarkable, since our common swine have a large cystis fellea. But it had a ductus bilarius, which went from the liver to the duodenum as usual. The pancreas was about 5 or 6 inches long, and consisted of several glands.

The testes were 2 inches long; larger at the upper end, then the lower, and in the middle about an inch broad; they were placed in the scrotum; their colour white; their structure close; so that the vessels which composed them, did not so plainly appear as in an ordinary boar: yet no doubt their whole compages was vascular, though here closer wrought together and united. Vauclius Dathirius Bonglarus* discovered this vascular structure of the testis of a boar, as also of a man about 10 years before Reg. de Graaf published his book, De Organis Virorum Generat. inservientib. and has given good figures of the same. Though the latter has given a much larger and further account of this subject since. Their use is no doubt to prepare the semen; which is conveyed thence by the vasa deferentia to the vesiculæ seminales. These deferentia arise near the lower part of the testes; and are so inserted that they might almost equally empty themselves, either into the vesicæ seminales or urethra.

* Vid. Philosophical Transactions, N° 42.

The vesiculæ seminales were $1\frac{1}{2}$ inch long; in some places $\frac{1}{4}$, in others half an inch broad. Though called vesiculæ, yet here they appeared more glandulous; nor was their cavity very large. The common orifices to them and the vasa deferentia made a rising in the inside of the urethra, which de Graaf calls caput gallinaginis; in men and other animals there is a better resemblance and show for the name. In those too, at this place, is seated that glandulous body called the prostatæ. But the vesiculæ here being so glandulous, possibly they may perform their office; unless we should ascribe their use to those two glands, which lay on each side the urethra, and emptied themselves with two orifices, near the root of the penis. These glands were cylindrical, of a whitish yellow colour, an inch and half long, and $\frac{3}{8}$ of an inch in diameter. Their substance was close, like that of the testes, and no perceptible cavity within; and they lay along the outside of the urethra, reaching from the musculi erectores penis to the glandulous vesiculæ before described. The penis in our tajaçu was a long slender body, composed of several muscles, whereof two were very long. The vesica urinaria, or bladder of urine, was rounder than in some other animals; where usually it is more oblong. The ureters were inserted at the neck of the bladder.

In the thorax, the structure of the aorta was uncommon. For as it descends along the spine in all other animals, its trunk almost of an equal breadth, only a little tapering downwards; here, between the heart and its branchings into the iliac arteries, we found three large aneurisms or tumours. The largest was that nearest the heart, which, after a small contraction, emptied itself into the second; and this, though less than the first, was yet much larger than the third, which was near the division of the aorta into the rami iliaci. Two of these swellings I opened; and found within several unequal cells or hollows; and the membranes here were altogether as thick as where the artery was not extended.

These extensions of an artery are called aneurismata; as those of a vein, varices; and are reputed to happen, when the inward coat of the artery is burst, and so gives way for the extension of the outward; and commonly they have been occasioned by pricking an artery, instead of a vein. But what should be the cause of it, in our subject, is most difficult to assign. For, it being the only one of the kind I have dissected; I know not how far it may be preternatural, or to be met with again. If preternatural, it is the more remarkable here, because this is the strongest and thickest artery in the whole body. If natural, there is nothing I can at present better parallel it with, than those protuberant swellings in the aorta of silkworms, and other such insects, which Malpighi takes for so many several hearts. Which must be allowed him, unless we deny them to have any heart at all. For in a leech, there are two large arteries, without any of these swellings; so we must either confess them to be two hearts, or not to have any; for there is no part I have yet observed in them, that I can give that name to, besides; nor to these without some allowance.

We come now to what seems most peculiar to this animal, to be met with in none besides, viz. the glandulous body seated on the ridge of the back, just over the hinder legs, but so covered by the long bristles there, that it was not to be observed but by opening them with the hand, and then appears a space almost bare, only beset with a few shorter and finer hairs; and in the middle of it is the protuberant orifice of the gland, by which it discharges the liquor which is separated by it within. This orifice has its lips a little reflected, and protuberant above the surface of the skin. It would easily admit of a large probe, which could be turned into several parts of the gland. On a gentle pressure with the finger, there was observed a small quantity of a white yellowish juice, and some part of it of a little darker colour, which yielded a very pleasant and agreeable scent, much like that of musk or civet. The gland itself was seated between the skin and some part of the panniculus carnosus. For in the middle of that part or surface which regarded the back, it was bare, and not covered with that muscle, only the edges inclosed within it; so that in taking off the skin, the gland too could easily come away with it; however, this muscle may be assisting to it, by its contractions, in pressing out its liquor, as the sphincter muscle is to those scent bags placed at the extremity of the rectum of other animals. The gland was conglomerated, or made up of several minute and small white glandules. It had no considerable cystis or cavity within; but like the pancreas, or salival glands, it had abundance of excretory ducts; which terminating at last in one, discharged its separated juice by the common orifice. This orifice having something of a resemblance to a navel, has imposed upon almost all who have described the animal, as to believe it an umbilicus; and others, who have deviated from this sentiment, have delivered quite as absurd and extravagant conjectures about it.

But my opinion concerning it is, there is nothing I can compare this gland with more than those scent-bags or glands in other animals. For though the whole body may be perspirable, and so diffuse a smell, yet that peculiar fœtor, which is observed in all strong-scented animals, I have hitherto constantly found more remarkably collected into one part; the particles, which cause it, being separated from the mass of blood by peculiar glands, which either quickly discharge it wholly out of the body, as in some, or transmit the separated juice into bags, or bladders, where it remains some longer time, as in many other animals.

This I first noticed in polecats, viz. that just at the extremity of the rectum are placed two bags, filled with a thick and whitish liquor, of a very strong and offensive scent. The same I have observed in abundance of other animals; as in all the polecat-kind, in our common cats, in a lion, in dogs, in a fox, &c. Those bags in the civet-cat, or hyæna odorifera, are nothing but the same. As are likewise those of a musk-quash, mentioned by Josselin in his History of the Rarities of New-England; for they are not the testicles of that animal; for having seen the skins here in town, and those musk cods, I find them to be only the scent-bags. So the castoreum we have in the shops is not the stones of a beaver, as formerly reputed, but altogether of the same nature with our scent-bags. And in most species of animals there may be observed something the same or analogous to it, which give them their peculiar fœtors or smells. Thus I have observed in reptiles, as the rattle-snake, in vipers, in the common snake, &c. two long bags in the tail, which discharges their fœtid liquor near the verge of the rectum. But in all animals these bags or glands are not seated here; but in some, in different parts of the body. In fowls and birds in the rumps are two glands, which have their pipes or excretory ducts arising on the top of it, above the surface of the skin, which discharge a fœtid liquor. These glands are largest in geese and the duck kind, which use the water. In Turkeys the rump is less glandulous, but they have a larger cystis within. In the ostrich the gland lies higher on the back, where it makes two bunches; and under the skin is a cystis filled with a concreted yellowish juice, which is near the place where the gland in the Mexican hog is seated. Again, the musk deer has its musk-bag on the belly, near the navel.

To give a short account of the skeleton; the cranium seemed entire, without sutures. From the nose, to the end of the pole, 8½ inches. Here the cranium grew very narrow; and then spread itself again triangularwise, and made a large hollow behind; and where were inserted strong muscles, and the ligament from the back; by which means the head is kept so straight up, that when alive, he seemed to have but a very short (if any) neck at all. The porus auditorius, or passage to the ear, was something remarkable, being placed near the pole. In the upper jaw in the front were four teeth, or incisores. A little farther was placed a large flat tusk, sharp-edged, and standing outwards; and beyond that, on each side, 6 double teeth, or molares. The lower jaw was 6½ inches long; 1½ broad at the first double tooth, of which there were 6 on each side. The bone of the lower jaw here, from the dentes molares to the incisores, seemed spongy and carious; and the tusks in this jaw were rotted out; as were one or two of the incisores, which in all were about four.

There were seven vertebræ of the neck; which measured in length 4½ inches. The first, or atlas, had two broad transverse processes, but no spine. The second had a broad large spine. The third, fourth, fifth, had no spines; the sixth and seventh had large acute ones. There were 19 vertebræ of the back; the spines of the first, second, and third, were about 3 inches long; but they gradually decreased, as they approached the tail. The first vertebra of the os coccygis was 2 inches long; and though it seemed to be several, yet it was but one bone. There were but 6 vertebræ more, which ran no farther than the extent of the os ischii. There were 14 ribs on each side. The os sterni jetted out about an inch beyond the setting on of the first ribs. The scapula was 5 inches long; the os femoris of the fore foot 5$\frac{1}{9}$; the os tibiæ of the fore foot about the same length in the whole: but from the juncture with the os femoris to the os metatarsi it was but 4 inches; for from the juncture with the thigh-bone it jetted out further. The bones of the tarsus were 5: of the metatarsus 3, about 2 inches long. The bones of the digiti 9; there being 3 to each claw, and 3 claws on each fore foot. The os femoris of the hinder foot was almost 6 inches long; and near its juncture with the os tibiæ it had a small bone, like the patella in the knee of a man. In the leg were two bones, the focile majus and minus, 5½ inches long. But this part in the fore leg was only a single bone; though in a dog, a monkey, and some other animals, there are two bones in the fore-leg likewise. The os calcis was almost 2 inches long; and there were 4 other bones of the tarsus or instep. The metatarsus or foot was composed of 4 bones, but the two inward much the largest; being 2¼ long; there were four digiti, in each three bones; whereof the last was covered with a nail.

The Explanation of the Figures.

Plate XXII. Fig. 1, represents the natural shape of this Mexican hog, and the line, a, points to the scent gland, on the hinder part of the back.

Fig. 2, gives a view of the skeleton. a the fore teeth or incisores; b the tusk; cc the grinders, or molares; d the lower jaw; e that part of the lower jaw which was carious; f the cranium; g the orbit of the eye; h the porus auditorius, or passage to the ear; i the triangular expansion of the cranium backwards; k the vertebræ of the neck; ll the vertebræ of the back and loins; m the vertebræ of the os coccygis; n n the ribs; o the protuberant bone of the sternum; p the scapula, or shoulder blade; q the os ischii; r r the os femoris, or thigh bones; s the patella of the hinder legs; t the tibia of the fore leg; v a large protuberancy of the tibia; w the tibia or focile majus of the hinder leg; x the fibula or focile minus of the hinder leg; y y the tarsus, or instep on both legs; z the calx, or heel in the hinder leg; ααα the bones of the metatarsus or foot; ϐϐϐ the digiti or toes; γγγ the nails.

Fig. 3, shows the orifice of the scent gland, as it naturally appeared on the outside of the skin of the back; a little space round this orifice was almost bare of bristles.

Fig. 4, the scent gland itself; which was conglomerated, or made up of abundance of lesser glandules.

Fig. 5, most of the viscera in the belly; a the œsophagus, or gullet; b the first ventricle, or stomach; c the second ventricle, or stomach; d d the cornua, or horns of the second stomach; e the third stomach; f the pylorus; g g g the intestina tenuia, or small guts; h h h the colon; i the cæcum; k the rectum; l the mesentery; m m the meseraic vessels; n the pancreas; o the spleen; p the liver; q the duct of the gall from the liver to the duodenum.

Fig. 6, represents the stomach opened; a the œsophagus or gullet; b the entrance of the gullet into the first stomach; c c the inside of the first stomach, which was invested with a strong thick white pellicle or membrane; d d the second stomach; e e the third stomach, in which were remarkable several plicæ or folds; f the pylorus.

Fig. 7, represents the outside of the three stomachs, more in their natural situation; a the gullet; b the first stomach; c the second stomach; d the third stomach; e the pylorus; f f f the blood vessels.

Fig. 8, represents the genital parts, and the bladder; a the bladder of urine; b the neck of the bladder; c c the ureters; d d the testes; e e the vasa deferentia; f f the vesiculæ seminales, which here were glandulous; g the caput gallinaginis, where the vesiculæ seminales, and vasa deferentia, empty themselves into the urethra; h h two glandulous bodies, which possibly may be reckoned the prostatæ; i the orifices by which these glandulous bodies empty themselves into the urethra; k the urethra opened; l the penis; m m two muscles belonging to the penis; n n other muscles assisting to the same.

Fig. 9, shows the heart, and the aneurismata of the arteria aorta, or great artery; a the heart; b b the ascending branches of the great artery; c the descending trunk of the great artery; d the first aneurisma, or distinction of the great artery opened to show its several cells within; e a straitening of the artery again; f the second aneurisma opened likewise; g the third or smallest aneurisma; h h the iliac branches of the great artery.

Recherches Curieuses, &c. i. e. Curious Researches of Antiquity, contained in divers Dissertations concerning Medals, Bas-reliefs, Statues, Mosaic Works, and Inscriptions of the Ancients: enriched with a great Number of Brass Cuts. By Mons. Spon, Dr. of Physic, at Lyons, 1683, *in* 4*to.* N° 153, p. 386.

The subjects described in this work, consisting of various pieces of antiquities, as bucklers, shields, monuments, statues, rings, medals, coins, pictures, urns, inscriptions, &c. are too numerous to be particularized.

The Antiquity of the Indian Numeral Characters. By Dr. Wallis. N° 154, p. 399.

Sir, I send you a drawing of an ancient mantle-tree, which was lately showed me, at Helmdon, in Northamptonshire, belonging to the parlour chimney of the parsonage house. What makes it very remarkable, is the date, expressing I suppose the time when it was first made, described partly in numerals. A° Do^{i} M° 133. But both the letters and the figures are of an antique form, agreeing well enough with that age.

That the numeral figures now in frequent use, 1, 2, 3, 4, 5, 6, 7, 8, 9, 0, with the manner of computation by them, and the names of algorism appropriated to that way of computation, came to us from the Arabs, but somewhat altered as to the shape of the figures in succeeding ages, as are also the Latin letters, which were originally derived from those of the Greek, and to them from the Indians, who are supposed to have been the first inventors of such figures, and the way of computation by them, is generally agreed I think by all who have made these affairs their study. But it is not so generally agreed, of what antiquity the use of them has been in Europe.

Vossius, in his Treatise De Scientis Mathematicis, says, they have not been of use in Europe for much more than 350 years; and thinks they were not introduced till about the year of our Lord 1300, or at the farthest later than the year 1250.

And P. Mabillion, in his Treatise lately printed De Re Diplomatica, tells us, that he has not found them any where used sooner than the 14th century: which is yet somewhat later than the time assigned by Vossius.

What time is assigned by others I cannot say. But for my part, I think their use in these parts to have been much older. As old at least as the year 1050; if not so frequently in ordinary affairs, yet at least in mathematical things, and especially in astronomical tables.

They came to us, I suppose, first from the moors in Spain, from whom we had our Arabic learning, especially the astronomical. And it must needs be as ancient as any Latin translations are out of Arabic, of astronomical tables and other astronomical treatises. Which could neither be well understood in Arabic, nor translated into Latin, without the use of such figures; which occur frequently in those authors. But I do not remember, that I have any where seen any monument of them more ancient than the mantle-tree here described.

The sides of the chimney, by which the mantle-tree is supported, are of stone: but the mantle-tree itself is of wood, oak, or some other hard wood, which by being kept always dry and smoked, is become as durable. And it may yet (to all appearance) continue for some hundred years more. It is all over as black as ink, by age and smoke. The length of it, AB, (fig. 10, pl. 22,) is 5 feet 9 inches: its breadth or depth, at the ends, as AC, BD, is one foot, or rather 11½ inches; but at the middle, as EF, somewhat less; being somewhat hollowed, archwise. It is carved, from end to end. The lower part of it is abated, as in the mouldings of other chimnies. On the front of the upper part, is in the one half, the sculpture of a dragon's head, and wings. In the other half, there is here only shown in the fig. on three squares parted from each other by a deep furrow or channel, the date as it is here expressed; and, on a fourth, a flower; on a fifth, the two letters W. R. with an escutcheon, representing I suppose the name of him to whom it then belonged. And then, in two lesser squares as the space would permit, one over the other, flowers, as before. The letters and figures, on their several squares, are not engraved or cut in, but prominent, by way of bas-relief, the wood being abated round about them. The o, over the A, is a round o; but that over the M, is a square ▫; and part of this ▫ has been lately pared off with a knife, by somebody it seems who had a mind to see of what colour the wood is underneath; and it appears there not so black as the rest, but fuscous of a dusky smoke colour. And this, as I remember, is all the defacing that appears in the whole mantle-tree.

Hence it appears, that the use of such figures here in England, not only in astronomical tables, and other like pieces of learning, but even on ordinary occasions, is at least as old as the year 1133; which was the 33d year of king Henry the first. And I judge it to have been yet somewhat older, because the shape of the figures, though not come just to the shape which we now use, was even then considerably varied from the shape of the Arabic figures; which argues they had then been for some time in use; such change of shape in figures and letters coming on gradually with time.

The foot of the figure 3 being turned backward, makes it more resemble the

Arabic figure, which is much the same with this, save what here stands upright, there lies flat; and I find it so constantly in many of the ancient manuscripts, before the use of printing.

Nor need it move any scruple at all, that part of the number is expressed by the numeral letter M, or the word millesimo, of which M° is but a contraction, while the rest is expressed in numeral figures. For the like often occurs in old manuscripts; and sometimes even at this day. And it rather favours the simplicity of that age, not very nice in such things, especially among mechanics, than any design of imposture.*

On the Eclipses of Jupiter's Satellites for the Year following, 1684, *with a Catalogue of them, and Informations concerning its Use. By Mr. Flamsteed.* N° 154, p. 404.

It has been my custom for years past to make quarterly a small ephemeris of the eclipses of Jupiter's satellites visible with us, that none of them might escape me unobserved when the weather permitted; having by this means obtained a good stock of observations of them of my own, besides what I had collected from the works of Gallileo, Hodierna, Borelli, the papers of Mr. Rooke, late professor of Geometry, at Gresham College, and the communications of my friends Mons. Cassini and Mr. Townley, I found myself well furnished for the restitution of their motions, which I attempted last summer, and accomplished with such success, that having seen only 2 of the predicted eclipses of the first satellites, I find neither of them differ above 2 minutes from my calculations. I have also observed one of the third, not above 3 minutes faulty, and another of the second erring but 2; which makes me hope the inequality I suspected in this last, will not be found so great as I feared

* This subject is treated at greater length by the doctor in his Algebra, cap. 4, p. 17; as also by several other writers; some of them agreeing in Dr. Wallis's opinion, while others oppose it. Thus, among others, we shall find in the Philosophical Transactions, No. 439, An. 1735, a dissertation by Dr. John Ward, against the antiquity of the Arabian numerals, and particularly against the above date on the Helmdon mantle-piece; which he conjectures was intended for 1233, rather than 1133: and in the Archæologia, vol. 13, is a learned paper, (Art. 10,) by the Rev. Samuel Denne, in which he contends for a much later introduction of those numerals, at least in common use as well as in books of science. But, on the other hand, in the Gent. Mag. an. 1800, vol. 70, a writer, (Mr. R. Churton) has given a fac-simile of the above inscription, as large as the letters themselves, taken from the carving itself by a kind of impression on paper; showing that the fig. 1, for the hundred in Dr. Wallis's number, is the true and exact shape of the character; and that it has no approach to the form of the 2, as conjectured by Dr. Ward. On the whole, the truth seems to be, that those Indian numerals may have been used so nearly in this country, as above, on some particular occasions, though not brought into common use till a century or two later.

it might be: after I had finished the tables of their mean motions, I set myself to calculate others for finding the true times of their return to the heliocentric conjunction of ♃ in all places of his orbit, with some other which I foresaw requisite for the easy calculation of their eclipses: having this in readiness, and being encouraged by the late success of my endeavours, I resolved to calculate all the eclipses of the following year 1684, and to impart them to the public in the tracts, for the use of foreigners as well as ourselves; for, if these eclipses be observed, it will certainly show the difference of meridians between them and us. And I must confess it is some part of my design, to make our more knowing seamen ashamed of that refuge of ignorance, their idle and impudent assertion that the longitude is not to be found, by offering them an expedient that will assuredly afford it. Such of them as pretend to a greater talent of skill than others, will acknowledge that it might be attained by observations of the moon, if we had tables that would answer her motions exactly; but after 2000 years experience, we find the best tables extant erring sometimes 12 minutes or more in her apparent place, which would cause a fault of half an hour, or 7½ degrees in the longitude, deduced by comparing her place in the heavens with that given by the tables. I undervalue not this method, for I have made it my business, and have succeeded in it, to get a large stock of good lunar observations, for the correction of her theory, and as a ground work for better tables; but the examination will be a work of a long time; and if we should afterwards attain what we seek, the calculation will be so perplexed and tedious, that it will be found much more inconvenient and difficult than that I propose by observing the eclipses of Jupiter's satellites, which however at present I must prefer.

For I am persuaded that the eclipses of the first will scarcely be found above 4 minutes differing from my calculation in the catalogue, nor those of the third above twice as much; now an error of 4 minutes cannot cause a fault of more than 1 degree in the longitude, collected by comparing an observed ingress of the first satellite into ♃'s shadow, or emersion from it, with the time given in the catalogue; and I hope it will scarcely ever be found to err so much. But if the same eclipse may be observed in 2 distant places at the same time, or compared with an observation of the same satellite made within a week elsewhere, the difference of meridians will be had something better than by comparing 2 observations of the same phasis of a lunar eclipse made in distant places.

For whereas it is somewhat difficult, by reason of the penumbra, to determine the true time of the application of either of the moon's limbs to the shadow, the satellites eclipses, especially those of the first, are almost momentary. And as there can seldom be 4 eclipses of the moon visible the same year, those of the satellites happen so frequently, that there are more of them visible in 1 year than there are days, though the planet ♃ lie hid under the sun's rays every year a whole month together.

I know our navigators will object to this method, that it is difficult to practise at sea, because long telescopes are required, which the motion of the ship will not permit them to manage aboard; that it is hard to distinguish one satellite from another, and that tables or other contrivances for showing their mutual positions are here wanting: to which I answer; That if it be not practicable at sea, they cannot deny but it is at land; that the true longitude of remote coasts from us is the first thing desired for the correction of their charts; let them attempt these first, and I doubt not but the success will encourage them so much, that they will readily find means to put it in practice at sea.* That the French have used this method successfully both in Denmark and their own country; that a telescope of 14 feet long at most, or for need one of 8 feet, with broad eye-glasses, will be sufficient for this purpose; that the difficulty cannot be known till it be tried, and that use renders many things easy which our first thoughts conceived impracticable.

If it be required to know whether any one of those eclipses invisible to us be visible in any other given place, convert the difference of meridians between it and London into time. And if the place lie to the east of London, add it to, if to the west, subtract it from the time of the appearance at London, the sum or difference accordingly will be the true time of the eclipse under that meridian, at which, if ♃ be above the horizon, and the sun beneath it, the eclipse is there visible, otherwise not. Or by the help of the ephemerides of the planet's places, and a terrestrial globe, the space on it in which any of these eclipses will be visible may be found thus: first seek the true places of the sun and Jupiter, with his latitude, in the ephemerides, whereby you may find their declinations and right ascensions, either by the common tables or the globe itself exactly enough for this method. Bring London on the globe to the meridian, and detaining it there, note what degree of the equator is cut by it; from this subtract the time of the eclipse after noon, converted into degrees and minutes, the remainder shows the longitude of that meridian on the earth, where it is then noon when the satellite is eclipsed, which I therefore call the meridional longitude of the eclipse. Bring this meridional longitude under the meridian, and elevate the nearer pole to the sun as much as is his declination, keep the globe in this position, and if ♃ be in consequence of the sun, draw a line on the globe along the eastern horizon, it passes over all those places where the sun is setting at that time, but if ♃ be in antecedence of the sun,

* This, however, has never yet been done.

draw the said line on the globe by the western edge of the horizon, it passes over all those places where the sun is then rising.

Jupiter being in consequence of the sun, add the difference of his and the sun's right ascensions to the said meridional longitude, bring the degree of the equator answering to their sum under the meridian; raise the pole next Jupiter equal to his declination, and detaining the globe in this position, draw a line again by the eastern horizon, the space intercepted between this and the line of the sun's settings, before described on the globe, comprehends all those places on the earth from sun setting till ♃ is set. But if ♃ were in antecedence of the sun, subtract the difference of his and the sun's right ascensions from the meridional longitude, set the degree of the equator answering to the remainder under the meridian, and elevate the pole next Jupiter equal to his declination; keeping the globe in this position, draw a line by the western edge of the horizon, then the space included between this and the line of the sun's risings, contains all those places on the earth where this eclipse is visible between ♃'s rising and sunrise.

When any eclipse of these is observed, the difference between the noted time and that in the catalogue, will be the difference of meridians between the place of the observation and London, which lies so near the meridian of the observatory, that the distance need not be accounted for. And this determination may be relied on, if the first or third satellite were observed; but I dare not be so confident of the second and fourth for the reasons formerly given. However, I shall make it my business to observe all such eclipses of as many of them as shall be visible with us, that by comparing my observations with such as shall be made abroad, the error, if any, may be discovered and corrected.

When ♃ is in quartile of the sun, the distance of the first satellite from his next limb, when it falls into his shadow and is eclipsed, is one semidiameter of ♃; of the second, 2, or a whole diameter nearly; of the third, 3; of the fourth, 5 of his semidiameters, or something better when the parallax of the orb is greatest. But these quantities diminish gradually as he approaches the ☌ or ☍ of the sun, somewhat nearly, but not exactly, in the proportion of the signs.

As the sun removes from the ☌ of ♃, the ingresses of the satellites into his shadow become observable. When he is about 30° from it, the emersions of the 4th, and at 60°, of the 3d, begin to be seen between the shadow and body; continuing so till the sun be arrived within 60° of the ☍ of ♃, when the emersions of the 3d fall behind his body, but the emersions of the 4th continue visible till he be less than 30° distant from the ☍, at which time they also are hid behind him, all the appearances being made really to the right hand or in antecedence of ♃, though with inverting telescopes they appear the contrary, to the left.

After the opposition of the ☉ and ♃, we begin to see the emersions of all the satellites from the shadow, now on the left hand or in consequence of ♃, but through inverting glasses on the right, when the ☉ is near 30° from the opposition of the ingresses of the 4th, when 60° from it of the 3d, begin to be observable between the body and shadow, continuing so till the sun arrive at the same, or rather within a wider distance from the ☌ of ♃.

[The catalogue of calculated eclipses is omitted, as of no use at this time.]

A short Account of the Comet of the Year 1683. *By M. Hevelius.* N° 154, p. 416.

After noting the daily observations and appearances of the comet, tracing its course among the fixed stars, M. Hevelius from these deduces the following table of its motions and observed places.

M.	D.	Longitude.	Latitude.	Diur. Mot.	Declination.	Right Ascen.
July	30	7° 0′ ♋	29° 15′ N	° ′	51° 30′ N	100° 0′
	31	6 25	29 0	0 42		
August	1	5 45	28 45	0 44		
	2	5 0	28 30	0 46		
	3	4 10	28 15	0 48		
	4	3 20	28 0	0 50	51 40	96 0
	5	2 20	27 45	0 52		
	6	1 20	27 30	0 54		
	7	0 20	27 15	0 56		
	8	29 20 ♊	27 0	0 58		
	9	28 20	26 40	1 0		
	10	27 20	26 20	1 2		
	11	26 20	25 55	1 4		
	12	25 20	25 30	1 6		
	13	24 20	25 0	1 8		
	14	23 20	24 30	1 10		
	15	22 20	24 0	1 12		
	16	21 10	23 20	1 14	46 0	77 0
	17	19 20	22 30	1 19		
	18	17 40	21 30	1 25	44 0	73 30
	19	16 0	20 30	1 35		
	20	14 20	19 15	1 45	41 0	69 30
	21	12 20	18 0	1 55		
	22	10 20	16 45	2 5		
	23	8 20	15 30	2 15		
	24	6 20	14 15	2 25	35 0	60 40
	25	3 50	12 45	2 40		
	26	1 5	11 0	2 55		
	27	28 15 ♉	9 0	3 10		
	28	25 15	6 30	3 25	24 30	51 0
	29	22 15	4 0	3 40	21 30	48 30
	30	18 55	1 30	3 55	18 0	45 40
	31	16 25	1 0 S	4 5		
Sept.	1	12 55	3 30	4 20		
	2	9 55	6 0	4 40	10 30	40 0
	3	6 25	8 40	5 5		
	4	2 35	11 20	5 30	1 30	34 0

Hence it appears that this comet moved retrograde, or contrary to the order of the signs, its motion in the ecliptic, during the interval in the table, being 63° 55′, but 74° 35′ in its own orbit, which was inclined to the ecliptic in an angle of 39°, but to the equinoctial 56°. Its latitude at first was 29° 15′ N. and at last 11° 20′ S.; so that it varied its latitude almost 41°. As to the head of this comet, its diameter at first was much less than afterwards; but, on the other hand, it was much brighter at first than towards the end. It exhibited no distinct and fulgent nucleus, as I have seen in most comets, but appeared a confused mass of matter, which became much thinner towards the conclusion. As it was seen mostly without a tail, it may be numbered among the hairy comets, or those that have beards like goats; for it extended upwards its very short and diluted bristles only till Aug. 18; which afterwards quite disappeared.

Account of the Book, De Urinis, Pulsibus; de Missione Sanguinis; de Febribus; de Morbis Capitis; et Pectoris: Opus Laurentii Bellini: Bononiæ, 1683. N° 154, p. 425.

With this work all our medical readers are so well acquainted, that to retain the account of it, inserted in the Transactions soon after its first publication, cannot now be desirable.

Account of Mr. Boyle's Memoirs for the Natural History of Human Blood, especially the Spirit of that Liquor. Lond. 1683. N° 154, p. 428.

This book is divided by its honourable and learned author into 4 parts; in the 1st of which he lays down some rules, concerning the way of compiling the natural history of any particular subject; where he proposes that whatever occurs concerning the subject to be treated of, may be ranked under several orders or classes, agreeing in the general names of titles: the first or primary order, the author would have to consist of such titles as shall comprehend the whole matter of the subject, as it appears at first view: but when the matter of any primary title encreases on hand, it will be proper that that also should be divided, according to its own nature, into several subordinate branches, or secondary titles.

And further he says it will be useful that a mantissa should be subjoined to the first edition of the titles, consisting of paralipomena and addenda: pursuant to which method, the author gives the titles of the first order, for the natural history of human blood of healthy men; and has also marked out a way for the natural history of urine.

The 2d part contains miscellaneous experiments and observations, about human blood, referable to some one of the titles of the first order: thus he treats of the heat of human blood freshly emitted; of the inflammability; of the specific gravity; of the volatile salt of human blood, and its figure; of the two oils; of the fixed salt, of the terra damnata; of the proportion of differing substances chemically obtained from human blood.

The 3d part contains promiscuous experiments and observations, about the serum of healthy men's blood.

The 4th part contains the history of the spirit of human blood begun; and is a summary of the history of volatile salts in general: under these (secondary) titles. 1. Whether human blood may be so ordered by fermentation or putrefaction, as that in distillation, a spirit, either urinous, or vinous, may ascend before the phlegm? 2. Whether spirit of human blood be really any thing, but the volatile salt and phlegm well commixed? 3. Of the species of saline bodies to which the spirit of human blood is to be referred. 4. Whether spirit of human blood be different from spirit of urine, and other spirits that are called volatile alkalies. 5. Of the quantity of spirit contained in human blood, whether accompanied with its serum, or dried. 6. Of the consistence and specific gravity. 7. Of the odour, taste, colour, and transparency. 8. Of the dissolutive power of spirit of human blood. 9. Of the tinctures that may be drawn with spirit of human blood. 10. Of the coagulating power. 11. Of the precipitating power of spirit of human blood. 12. Of the affinity between spirit of human blood and some chemical oils, and vinous spirits. 13. Of the relation between spirit of human blood and the air. 14. Of the hostility of the spirit of human blood to acids, whether they be in the form of liquors or fumes. 15. Of the medicinal virtues of the spirit of human blood applied outwardly, and 16 inwardly.

After which follows the appendix; where, among other things, under the title of the several ways of distilling human blood, the author gives an account of his success in distilling three portions of dried blood, each with a different additament, as with quicklime, calcined tartar, and oil of vitriol.*

END OF VOLUME THIRTEENTH OF THE ORIGINAL.

* The knowledge which philosophers possessed at this period of time, respecting the nature of the blood circulating in the vessels of man and other respiring animals was, in many particulars, very obscure and incomplete. Modern chemists have shown, and particularly the French chemists, that it is resolvable into a great variety of component parts, some of which it has in common with the other animal fluids, and even with some of the solids; while others of its component parts are, as a fluid, peculiar to itself. Besides the three distinct animal matters, albumen, gelatine, and fibrine, (diffused in a large proportion of water) it contains a considerable number of salts. To one of these (consisting of phosphoric acid and iron) its red colour is attributable. It also contains a portion of sulphur, combined with hydrogen and ammonia. This will account for some of the properties ascribed by Mr. Boyle, to what he terms the "spirit of blood."

END OF VOLUME SECOND.

treats of the heat of human blood freshly emitted; of [illegible] of the specific gravity; of the volatile salt of human blood, and its figures; of the two oils; of the fixed salt, of the terra damnata; of the proportion of [illegible] substances chemically obtained from human blood.

The 3d part contains [illegible] experiments and observations [illegible] the serum of healthy men's blood.

The 4th part contains the history of the spirit of human blood [illegible] a summary of the history of volatile salts in general; [illegible] tables. 1. Whether human blood may be [illegible] by [illegible] [illegible] a spirit, either [illegible] or [illegible] before the phlegm? 2. Whether spirit of human blood be simply [illegible] the volatile salt and phlegm well commixed? 3. Of the [illegible] to which the spirit of human blood is to be referred. 4. [illegible] man's blood be different from spirit of urine, and other [illegible] volatile alkalies. 5. Of the quantity of spirit obtained in human blood [illegible] accompanied with its serum, or dried. 6. Of the [illegible] [illegible] parts. 7. Of its odour, taste, colour, and transparency. 8. Of the [illegible] power of spirit of human blood. 9. Of the mixtures that are [illegible] with spirit of human blood. 10. Of the coagulating [illegible]. 11. Of the [illegible] coagulating power of spirit of human blood. 12. [illegible] human blood and [illegible] oil, and [illegible]. 13. Of the [illegible] between spirit of human blood and the air. 14. Of the [illegible] human blood [illegible], whether they be in the form of [illegible]. 15. the medicinal virtues of the spirit of human blood applied [illegible] internally [illegible].

After which follows the appendix; which, among other things, under the title of the several ways of distilling human blood, [illegible] which [illegible] [illegible] of dried blood, [illegible] calcined [illegible] [illegible], [illegible] [illegible], [illegible] [illegible].

[illegible]

* The knowledge which [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] in the name of [illegible] [illegible] [illegible] [illegible] [illegible] and [illegible]. [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible]

[illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] a large proportion [illegible] [illegible] [illegible] of phosphate of [illegible] [illegible] [illegible] combined with [illegible] [illegible] [illegible] [illegible] [illegible]

CORRECTIONS AND ADDITIONS TO VOLUME II.

PAGE 76, in the note, for *killed* read *skilled.*

126, l. 23, for "*notion* of pearls" read "*origin* of pearls."

282, for "Αρχιμήδυς" read "Αρχιμήδȣς."

356, in the note, for "*calculus* concretion" read "*calculous* concretion."

In the running title from p. 203 to p. 231, for vol. ix. read vol. x.

To the note on alum, p. 458, add: Kelp ashes are an impure saline product. Besides other saline matters, they contain both soda (mineral alkali) and potash (vegetable alkali). It would appear that it is the last-mentioned alkali which renders the kelp-ashes useful in the preparation of alum.—When, together with the incinerated ashes of vegetables, putrid urine is also employed, the obtained alum is a quadruple salt. There is much room for improvement in the fabrication of this compound salt, so useful in the processes of dying and other arts.

ADVERTISEMENT TO THE SECOND VOLUME.

BESIDES the remaining Part of Volume Seven, this Second Volume contains Abridgments of Volumes 8, 9, 10, 11, 12 *and* 13 *of the Original Transactions; together with the Seven Numbers (abridged) of the Philosophical Collections.*

CONTENTS OF VOLUME SECOND.

THE CONTENTS CLASSED UNDER GENERAL HEADS.

Natural Philosophy—Acoustics, Astronomy, Hydraulics, Hydrostatics, Hydrology, Magnetics, Meteorology, Optics, Pneumatics.

Anatomy, Chemistry, Medicine, Pharmacy, Physiology, Surgery, &c.

Natural History—Botany, Mineralogy, Zoology, &c.

Chronology, Geography, Mathematics, Mechanics, Navigation, &c.

Miscellanies—Agriculture, Antiquities, Architecture, Grammar, History, Music, Painting, Perspective, Sculpture, Travels, Voyages, &c.

Books, of which an Account is given in this Volume.

Biographical Notices of the following Authors in this Volume.

REFERENCES TO THE PLATES IN VOLUME II.

REFERENCES TO THE PLATES IN VOLUME I.

Fig. 1.

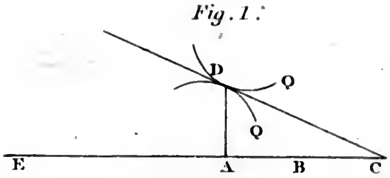


Fig. 2.

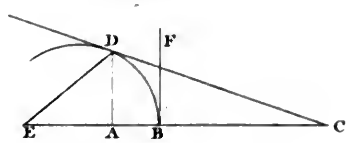
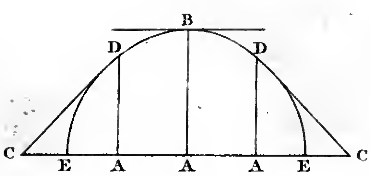


Fig. 3.



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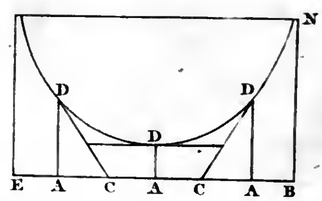

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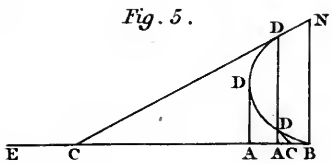

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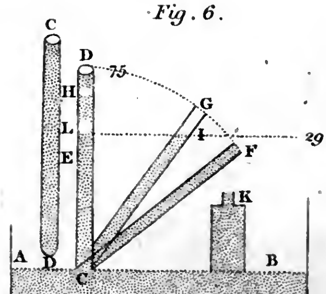
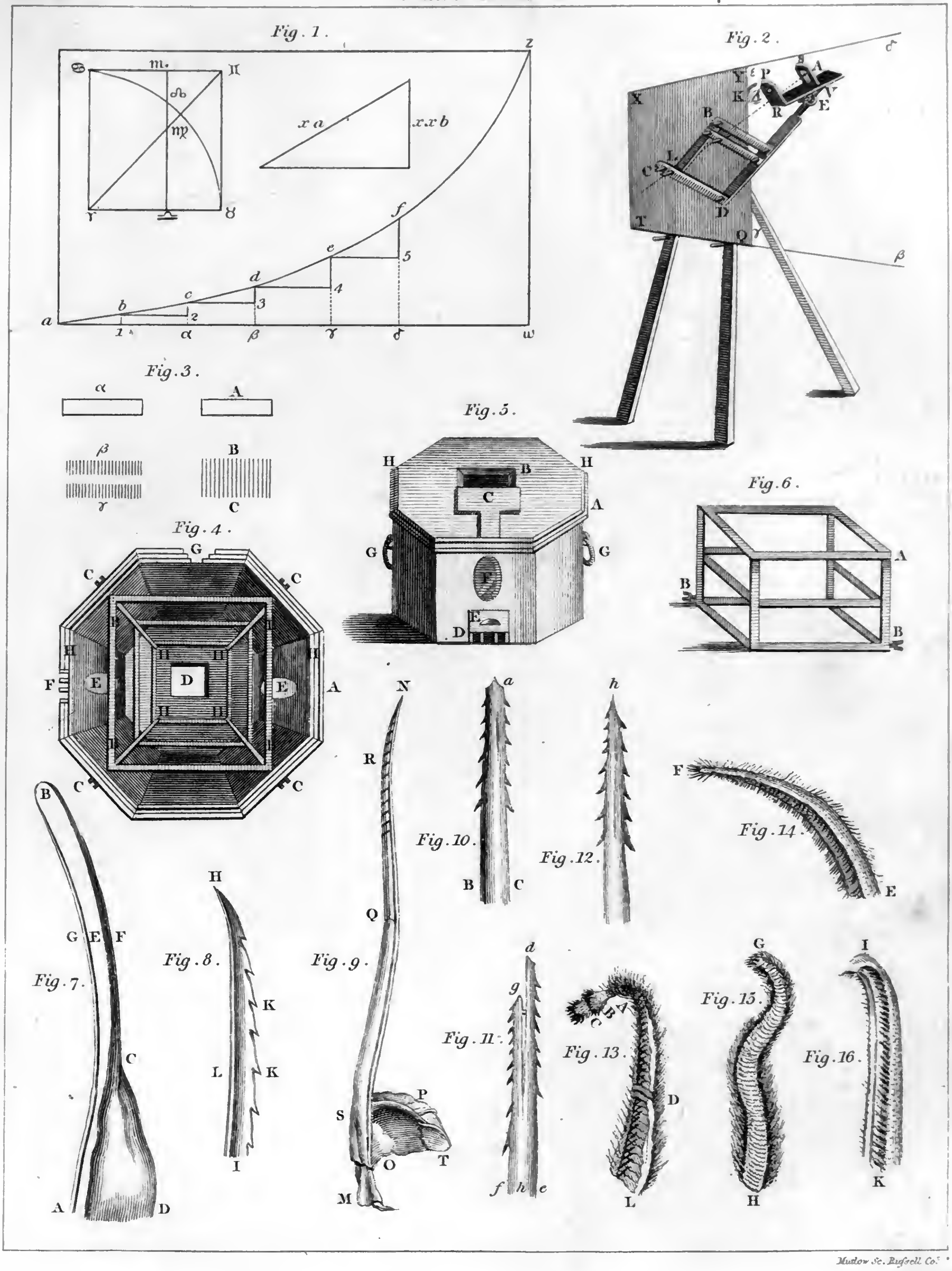


Mutlow Sc. Russell Co.

1803. Published by C & R. Baldwin, of New Bridge Street, London.



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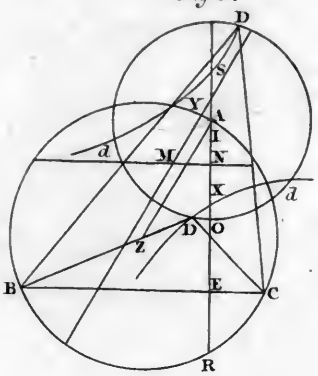

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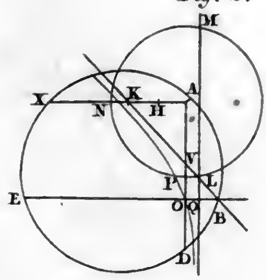

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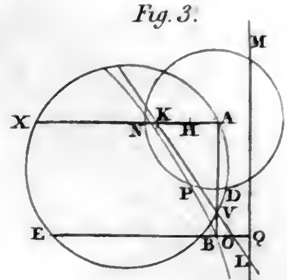

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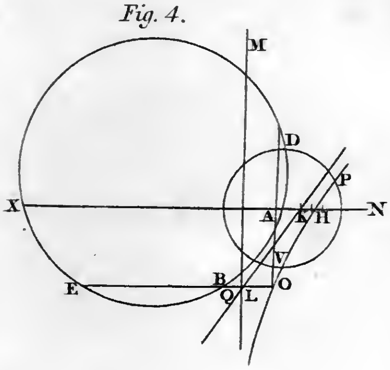

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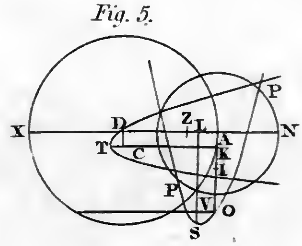

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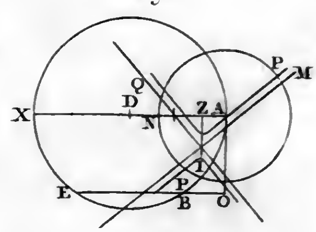

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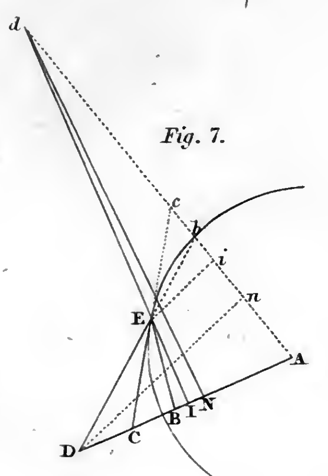

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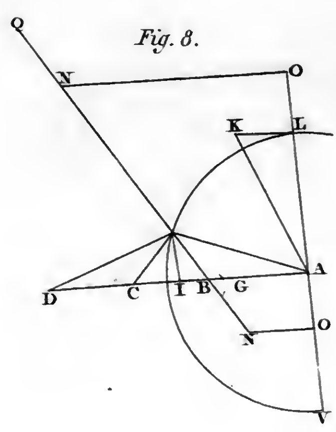

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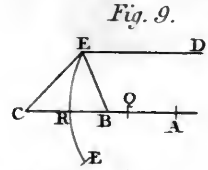

Mutlow Sc. Russell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London 1803.

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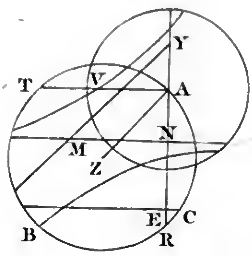

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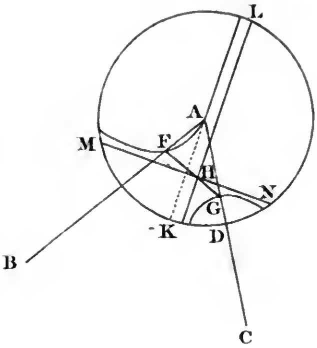

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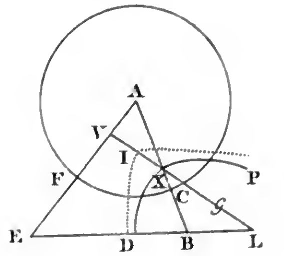

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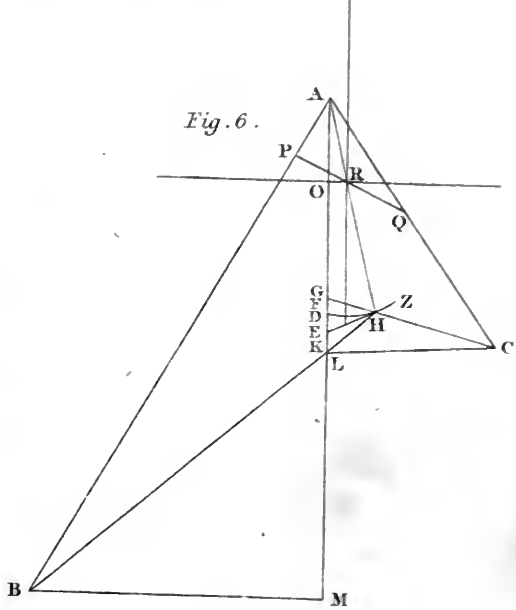

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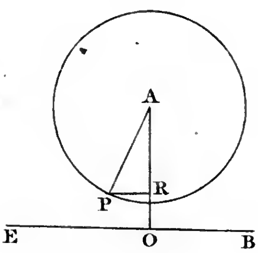

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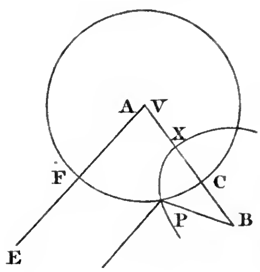

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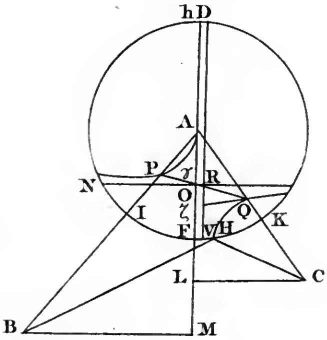
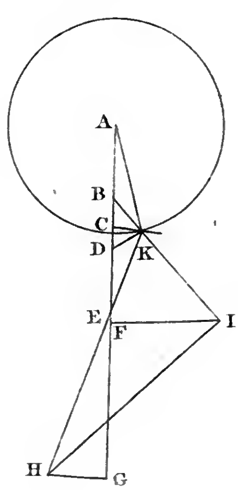


Fig. 8.



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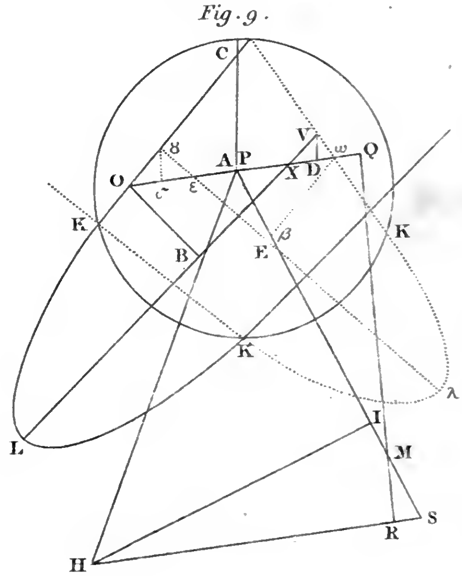

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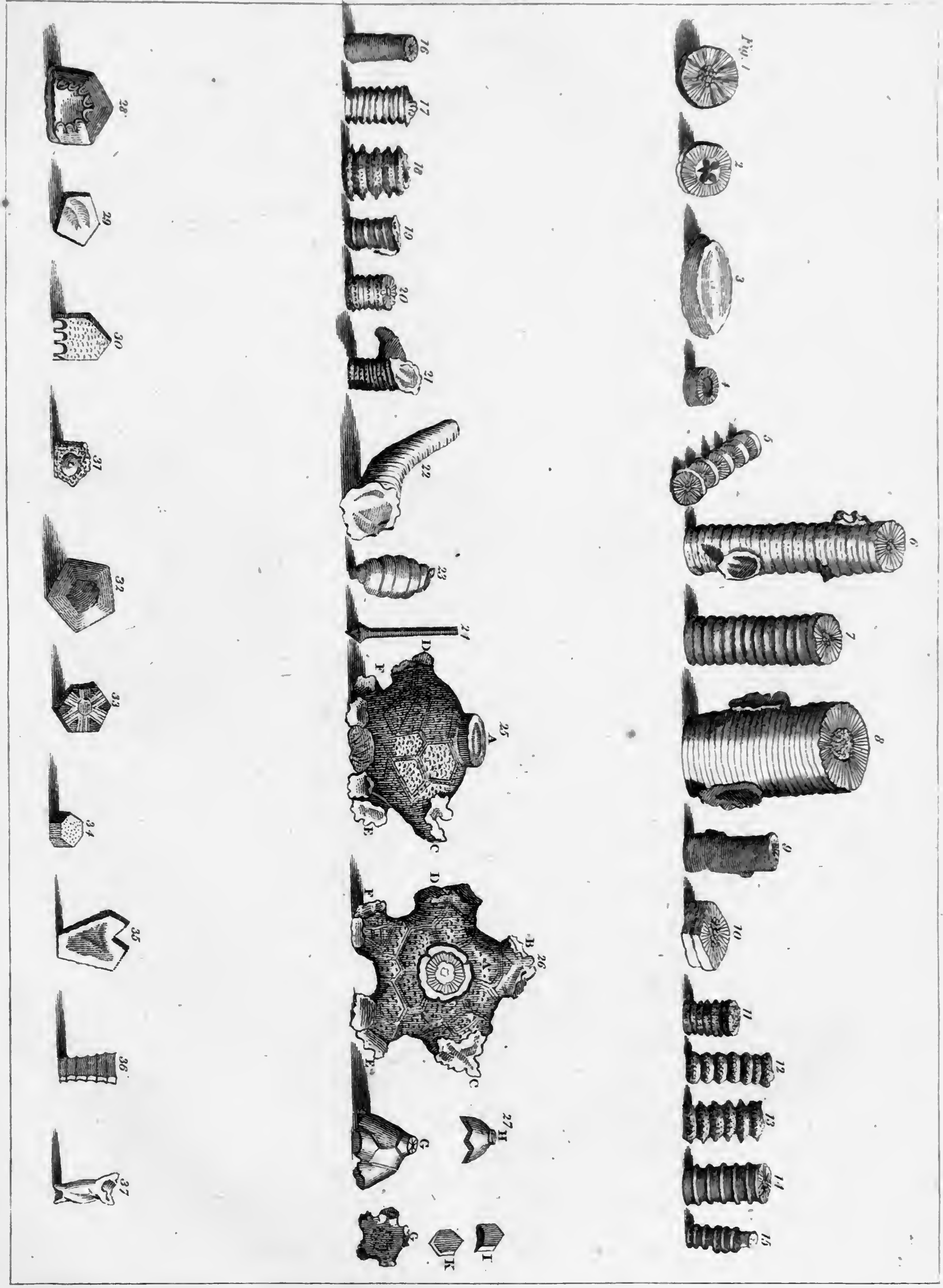

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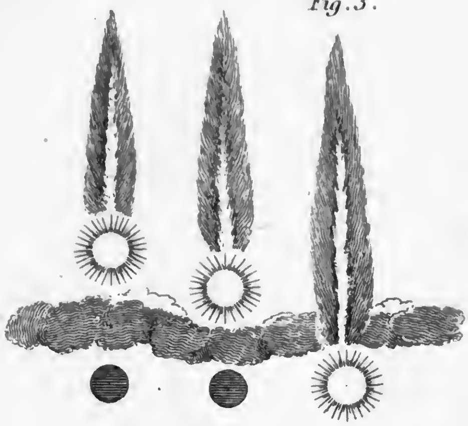
Fig. 1. Fig. 2. Fig. 3.



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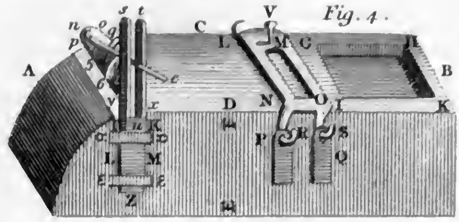

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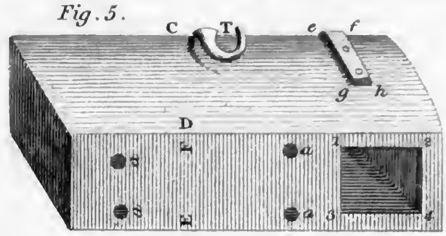

Fig. 6.

The following figures refer to the orders of the Snails in Mr. Lister's paper, in No. 105.

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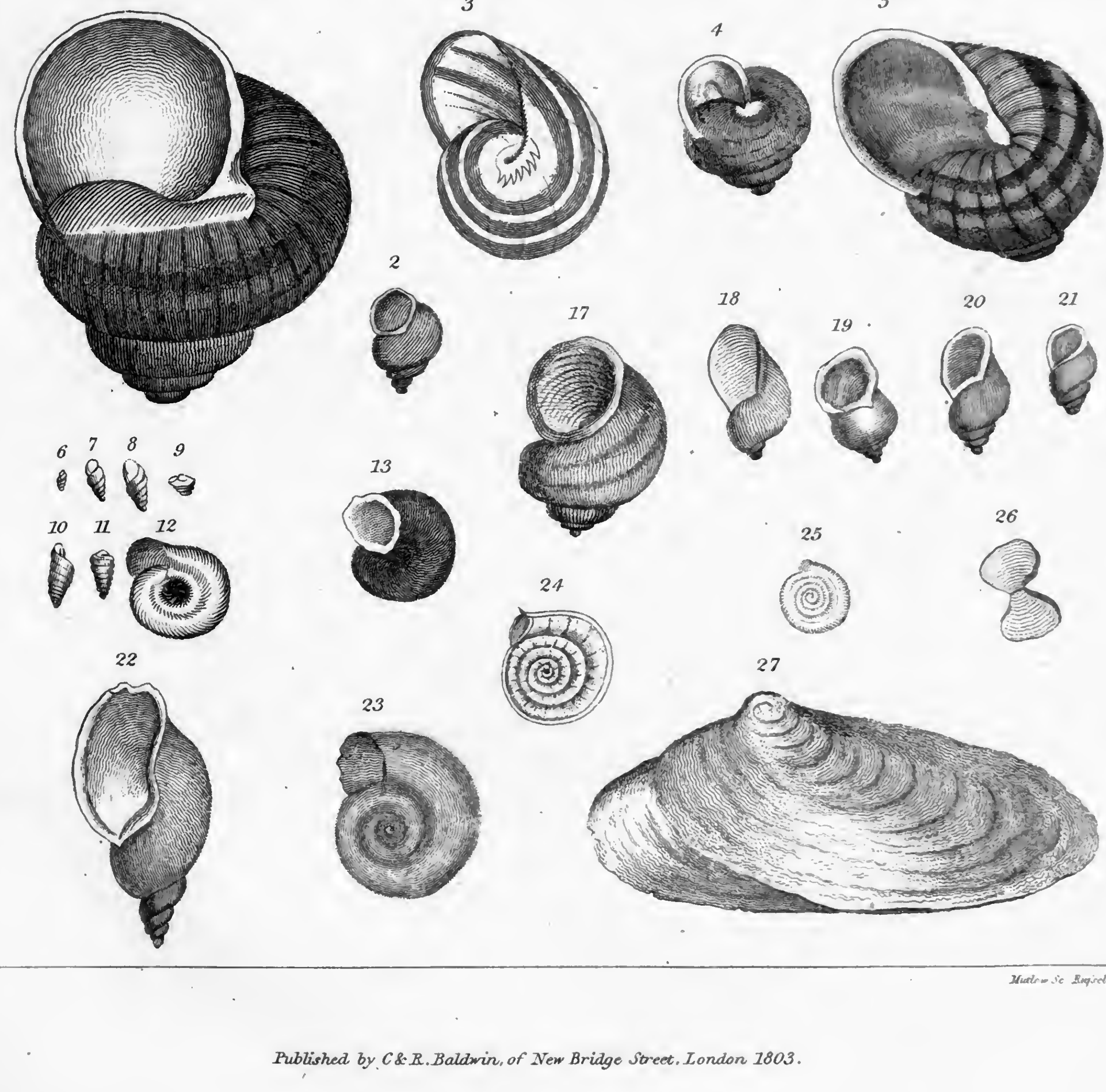

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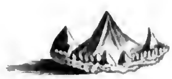

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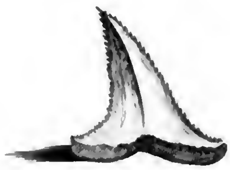

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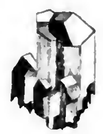

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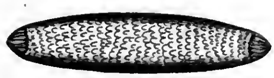

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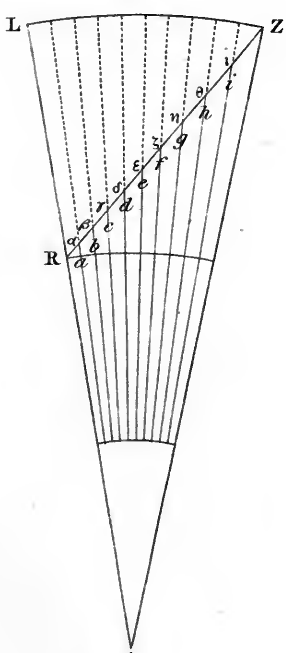

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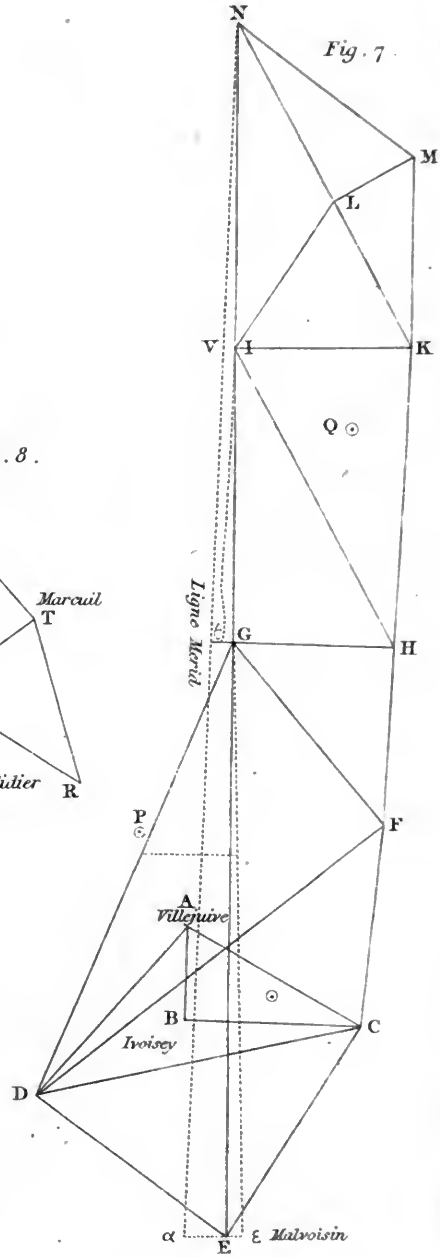

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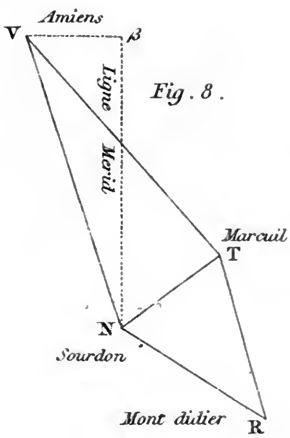

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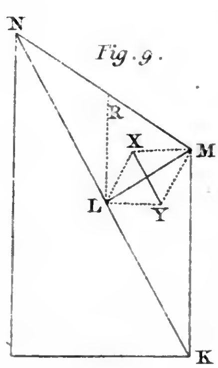

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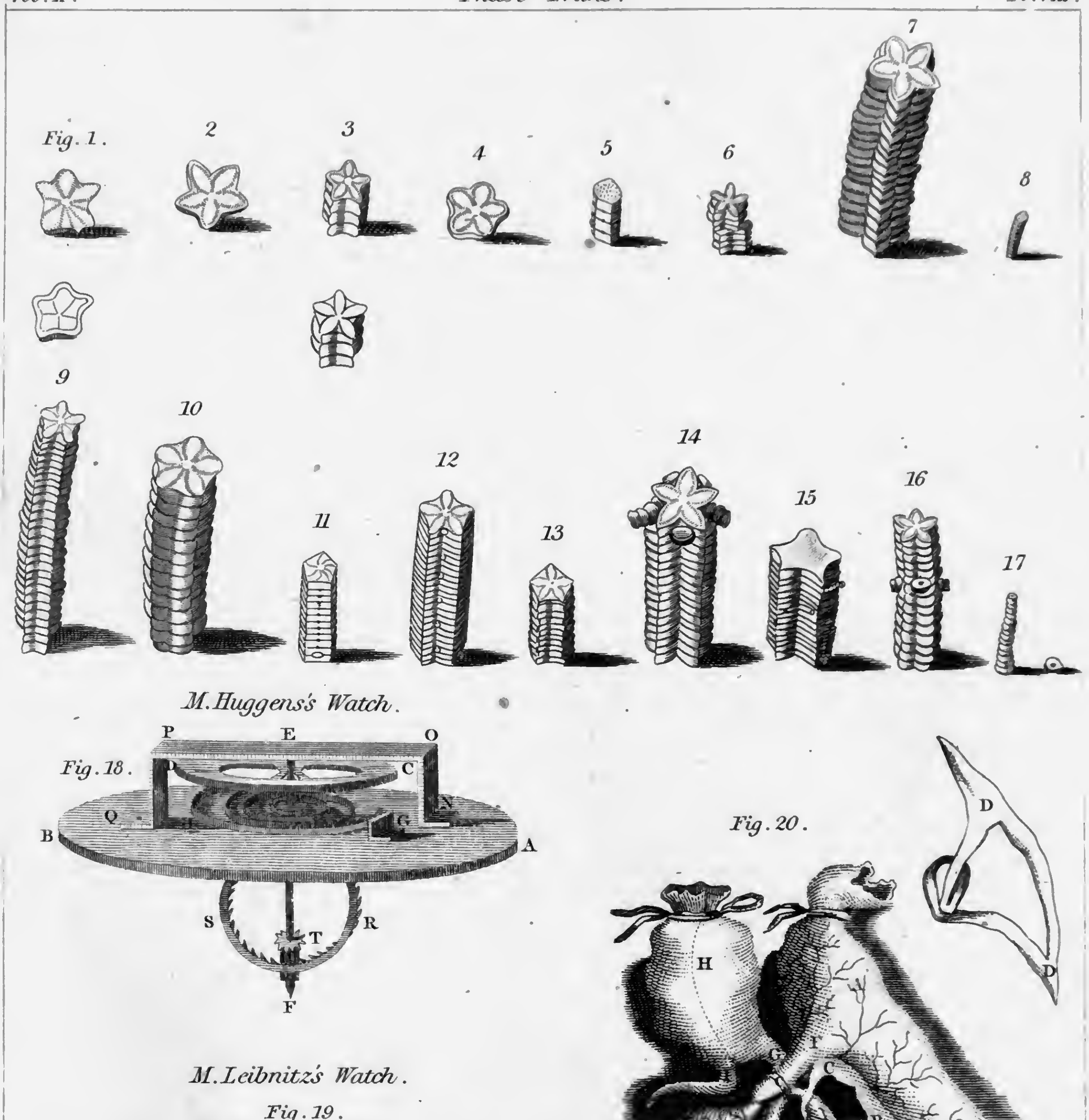

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Fig. 1.

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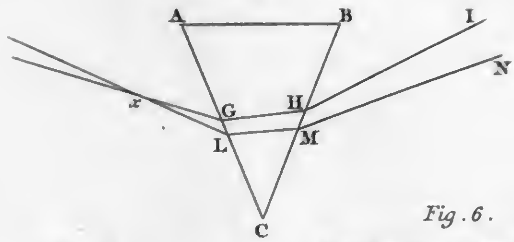

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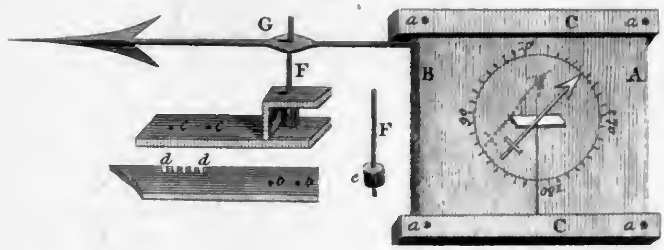

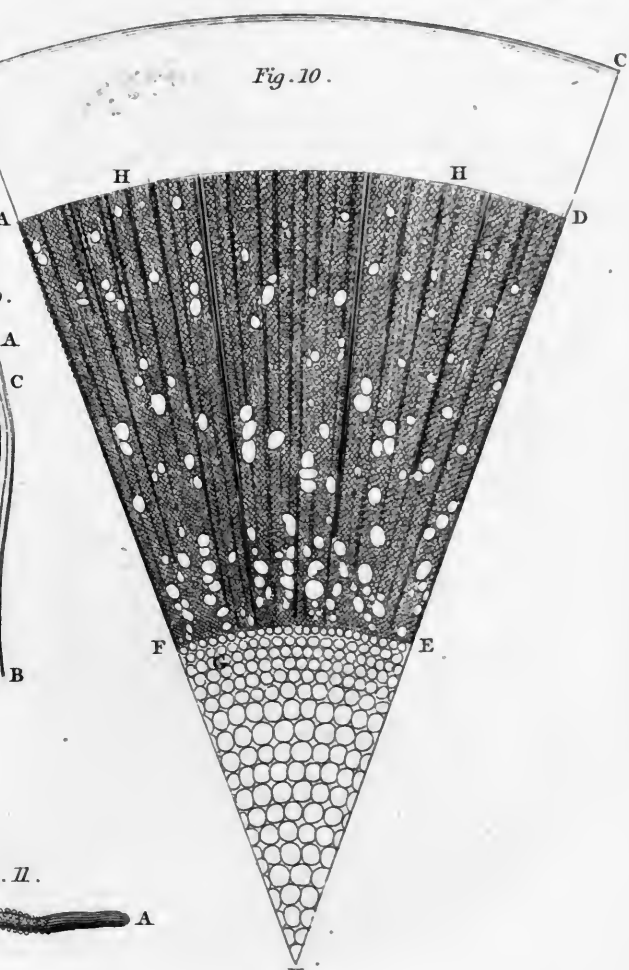

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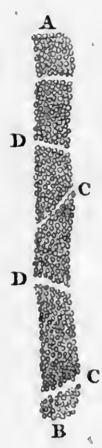

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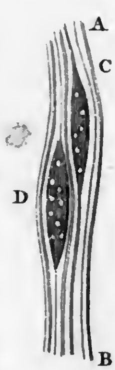

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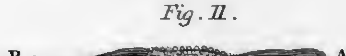

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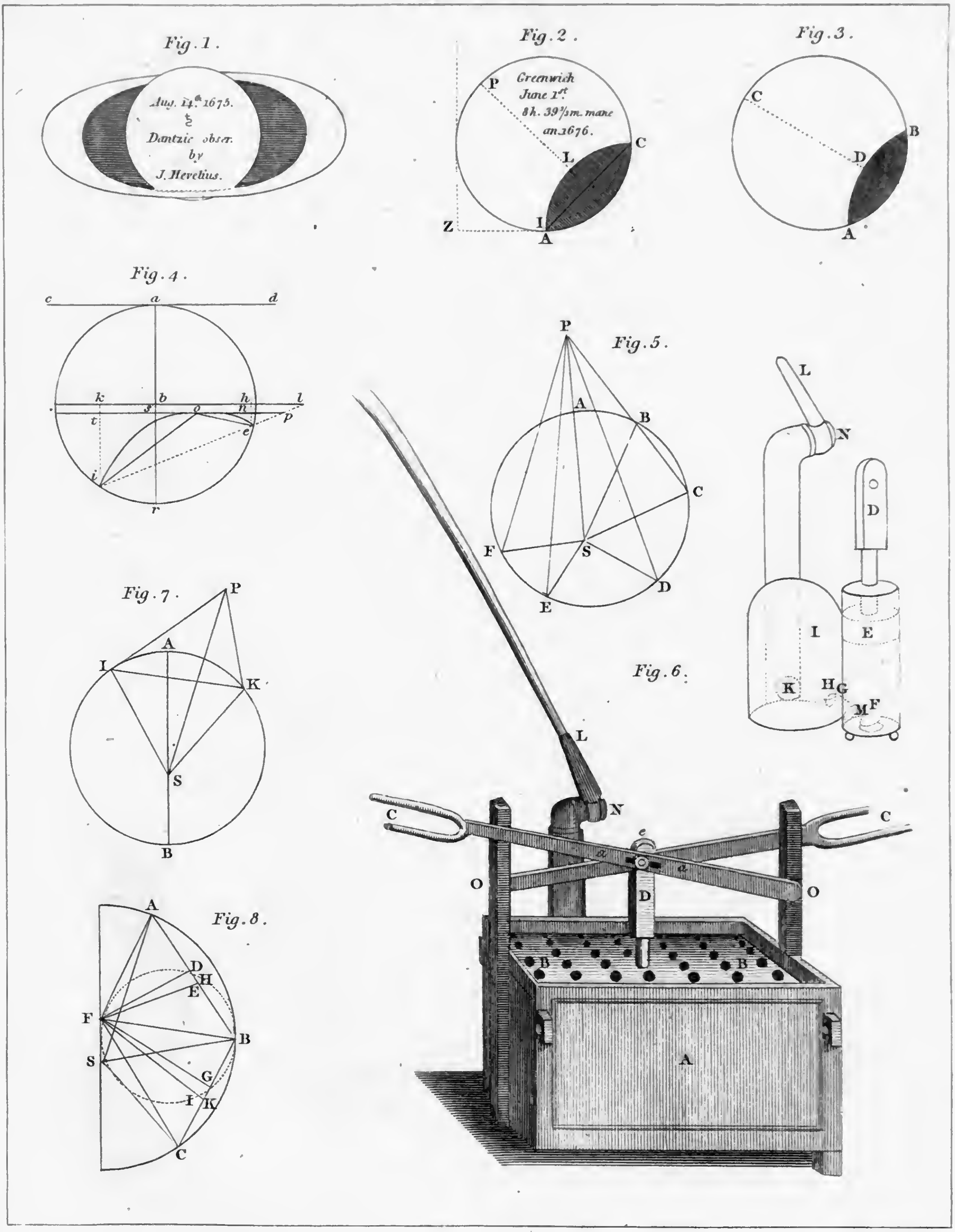

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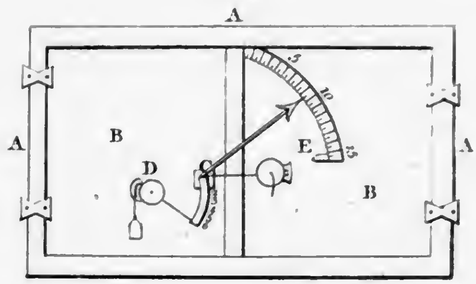
Fig. 1.

Fig. 4.



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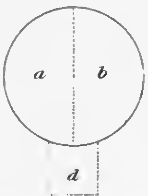

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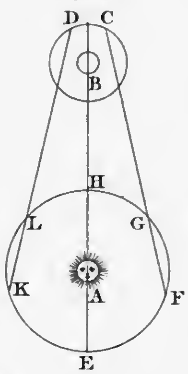

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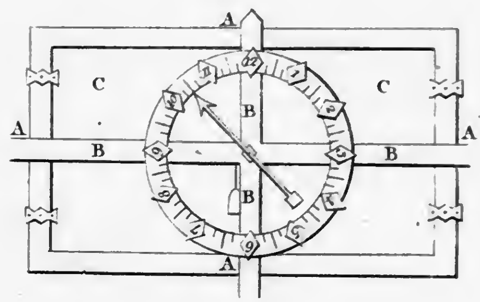

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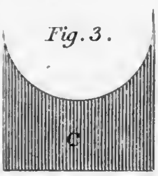

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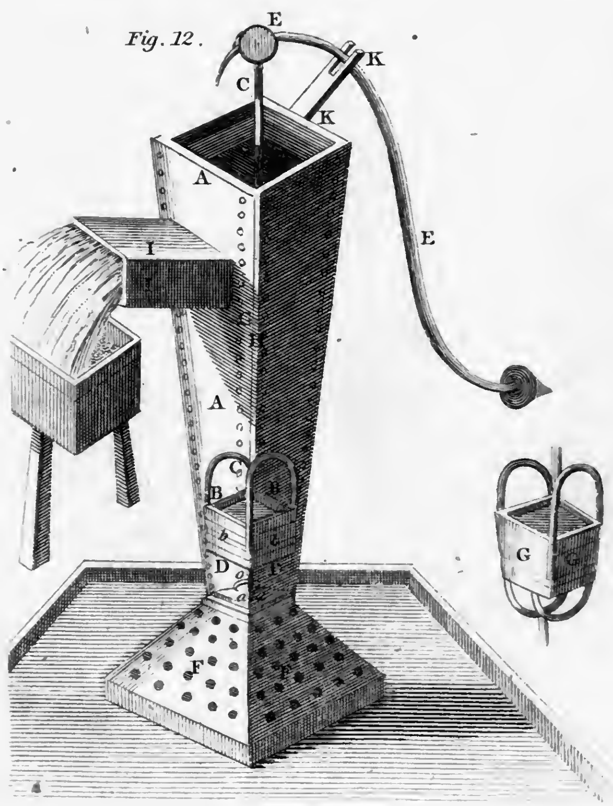

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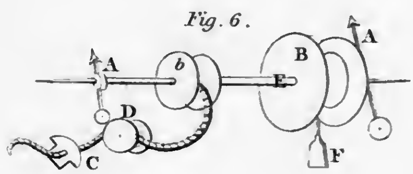

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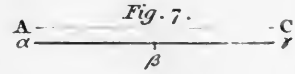

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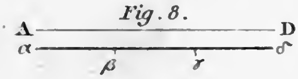

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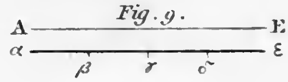

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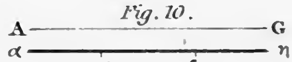

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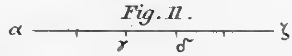

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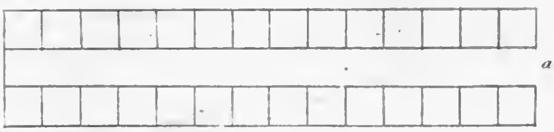

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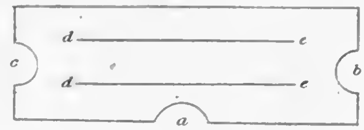

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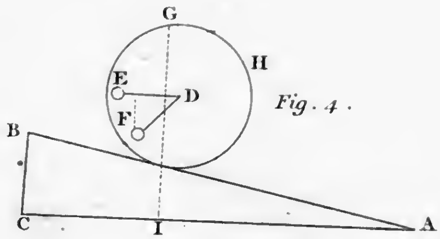

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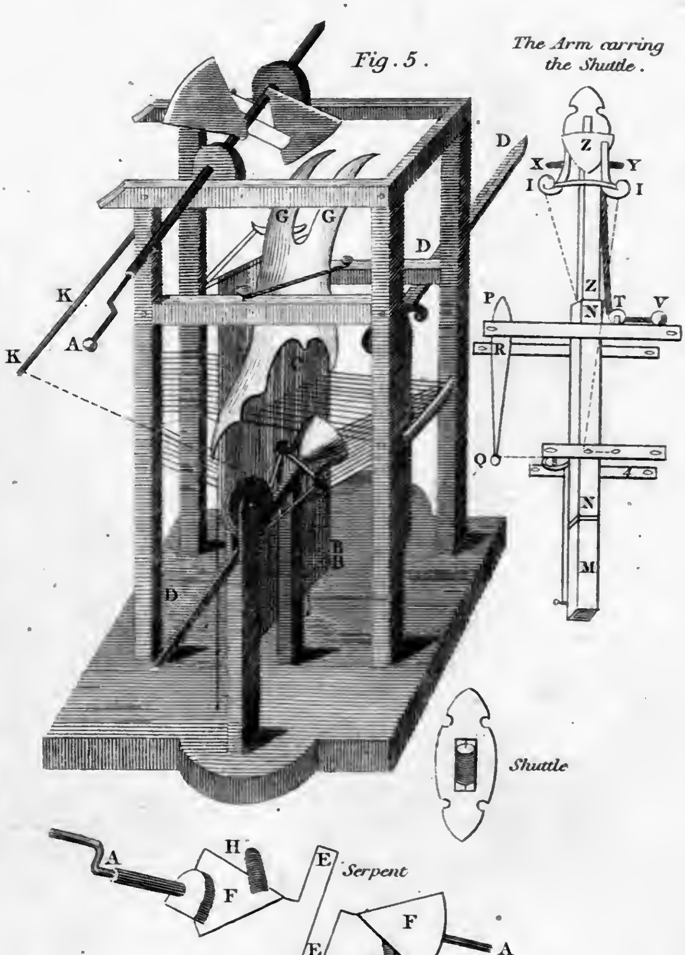

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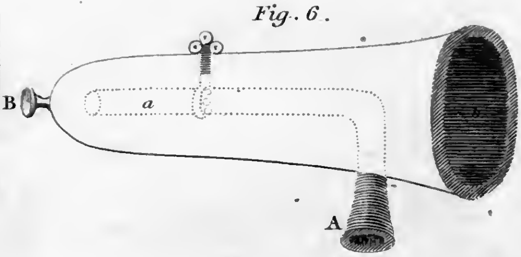

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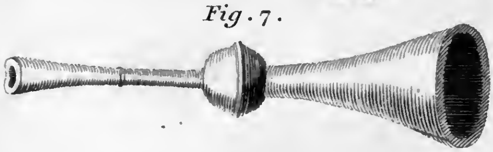

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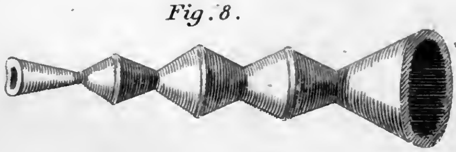

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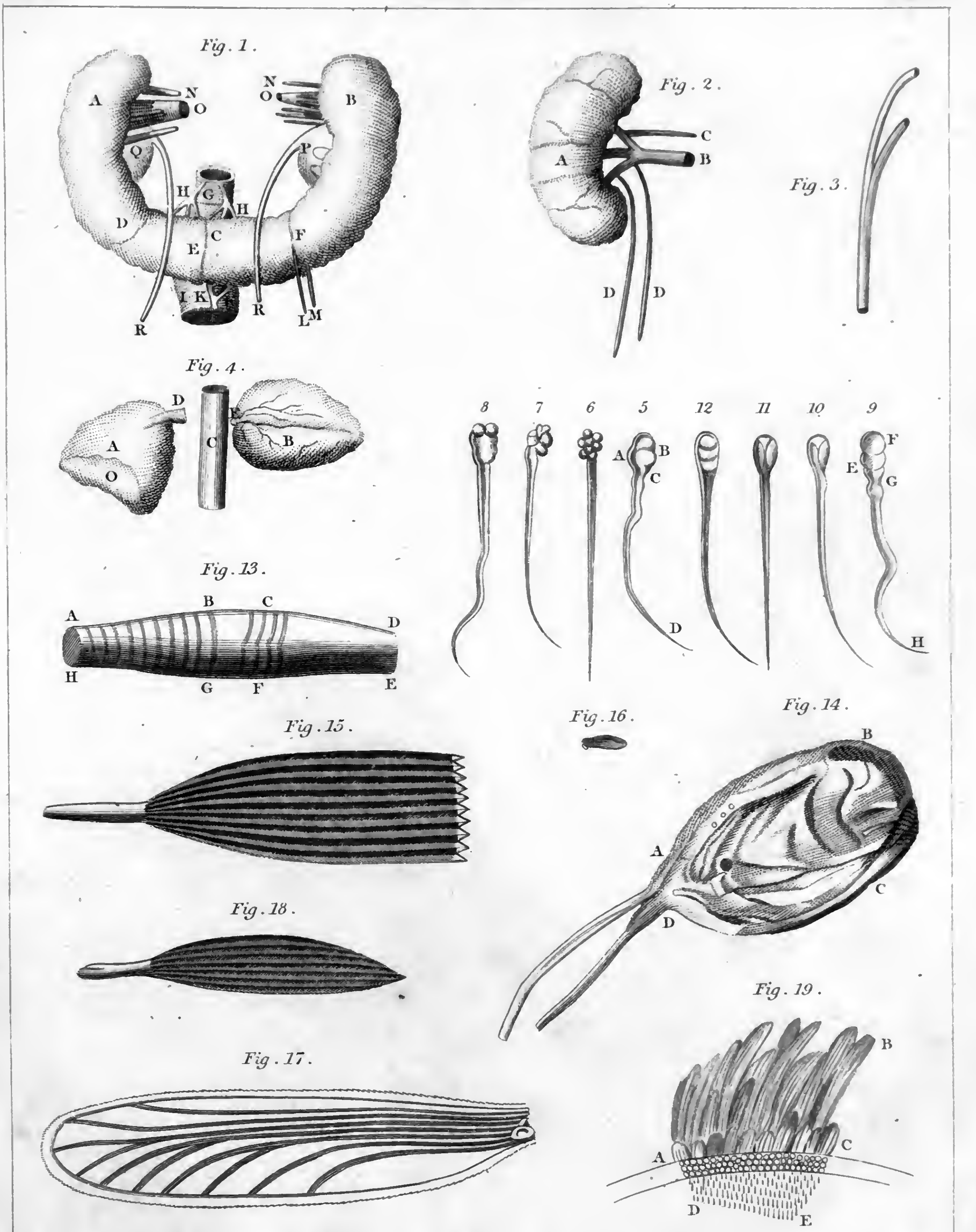

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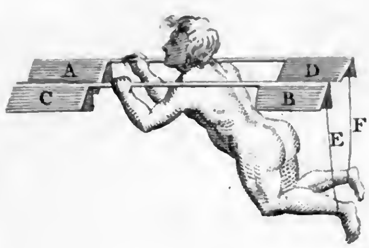

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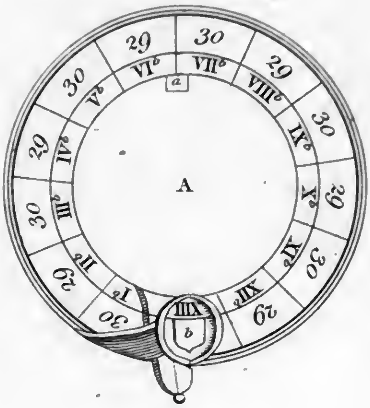

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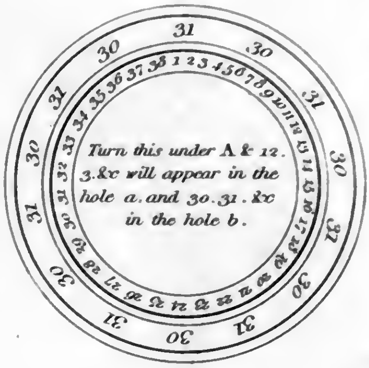

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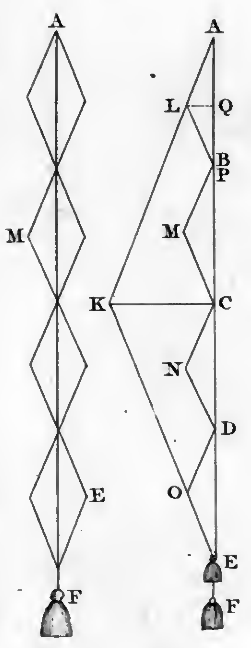

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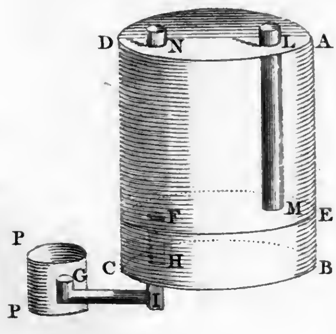

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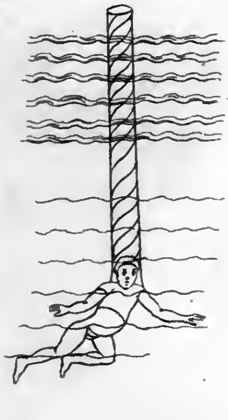

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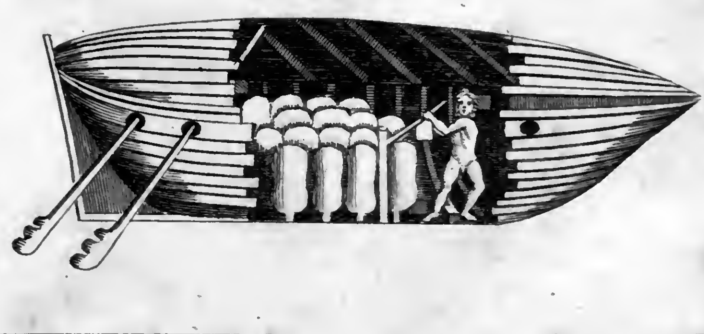

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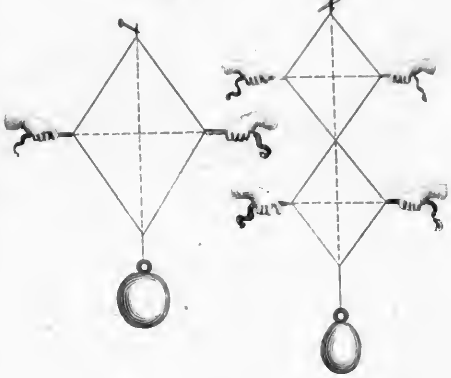

Fig. 1.

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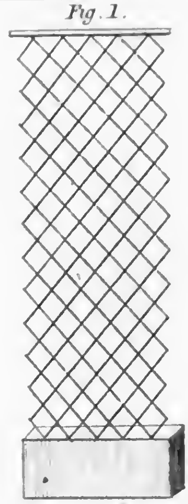

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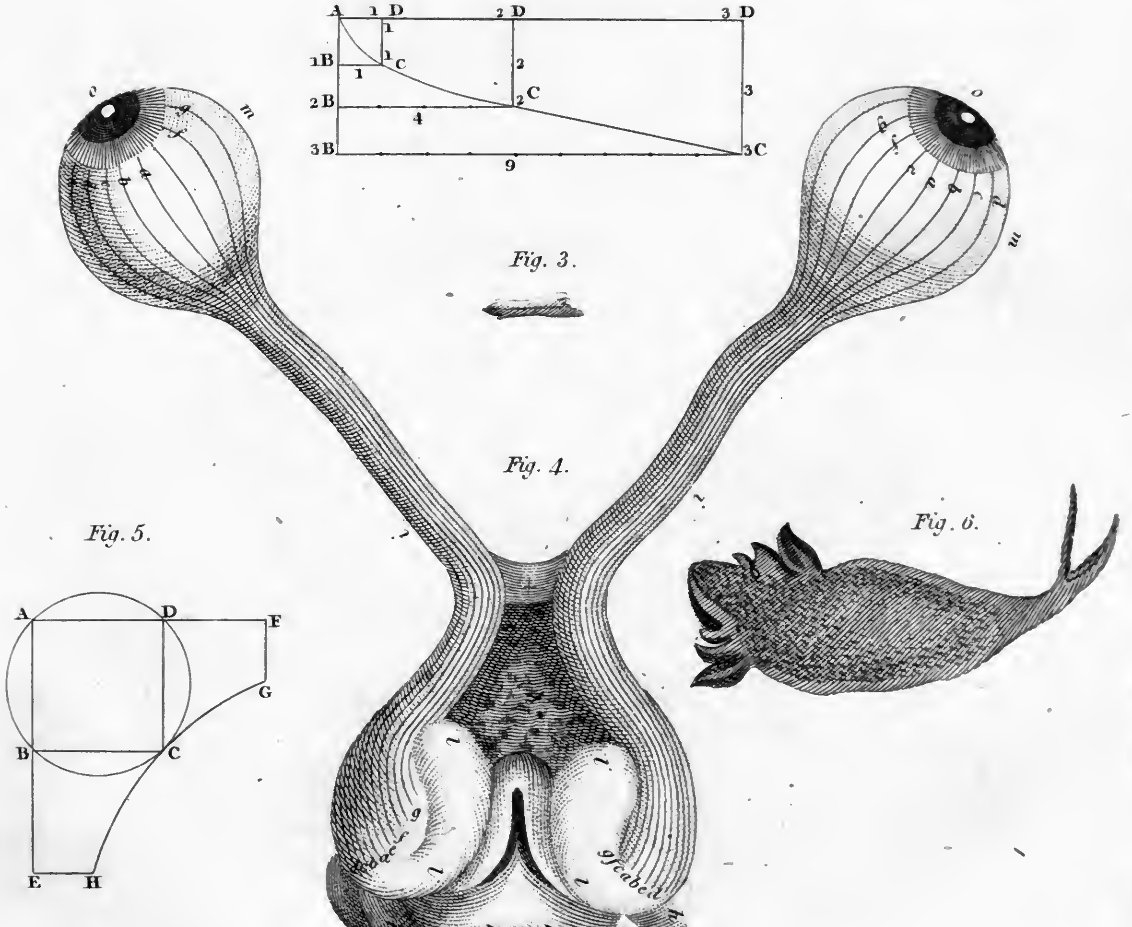

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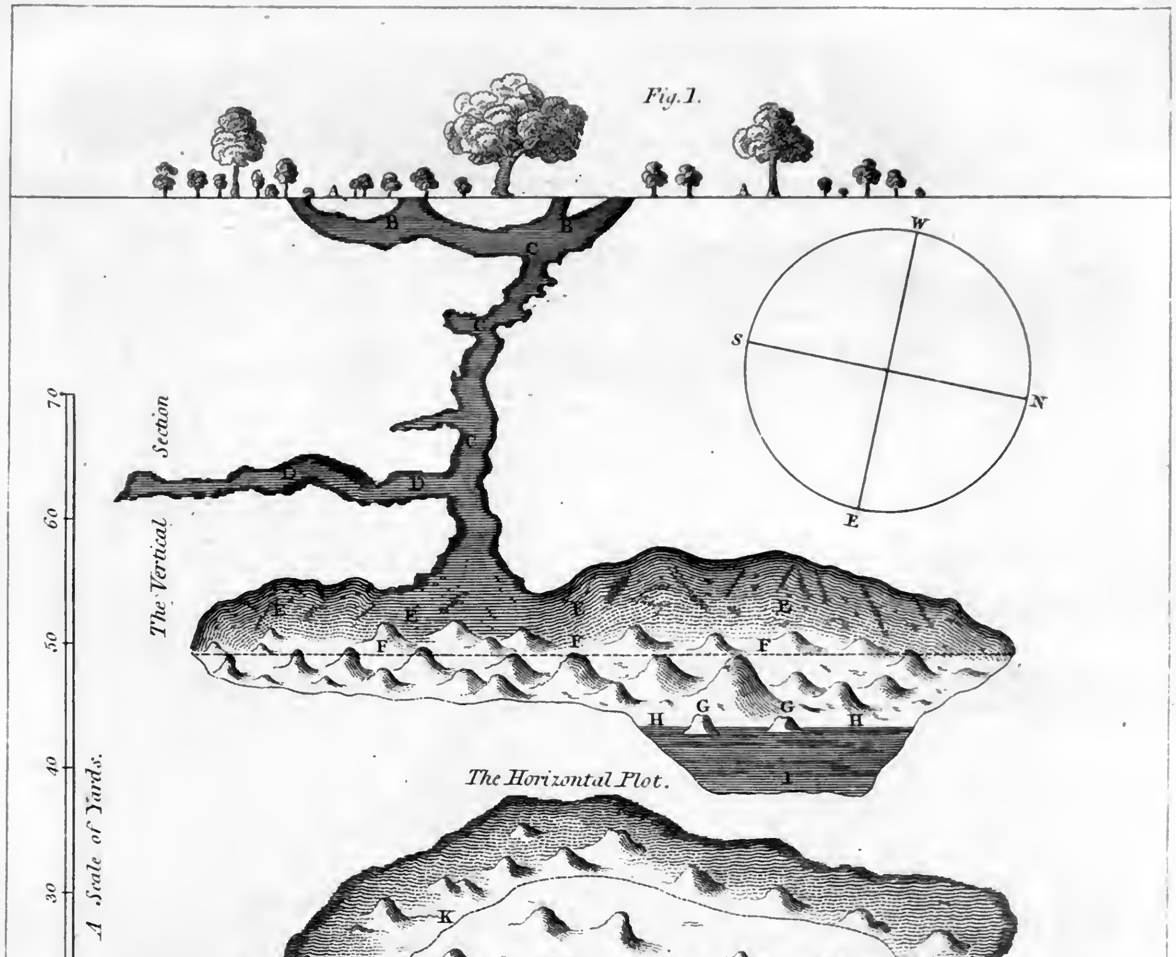

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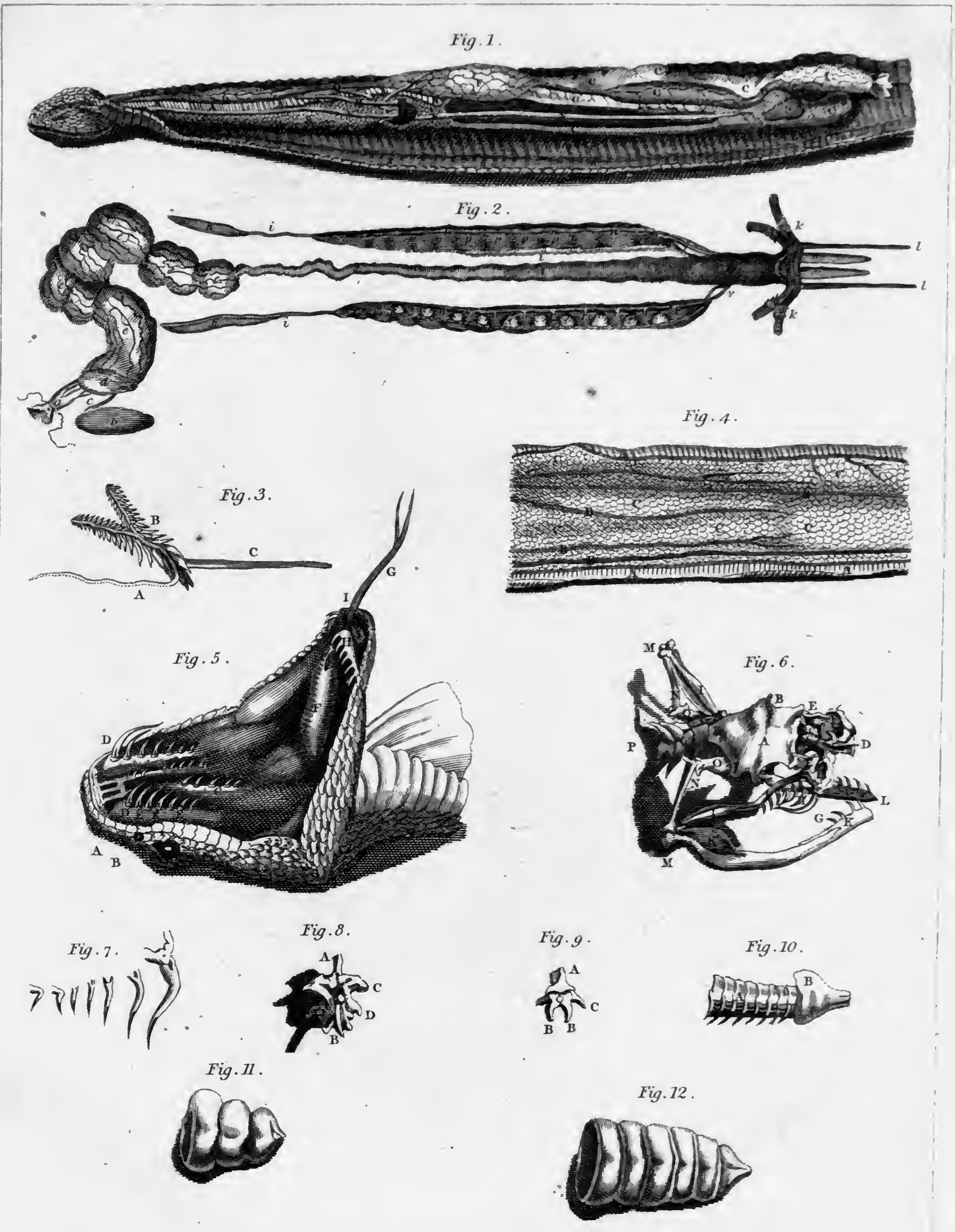

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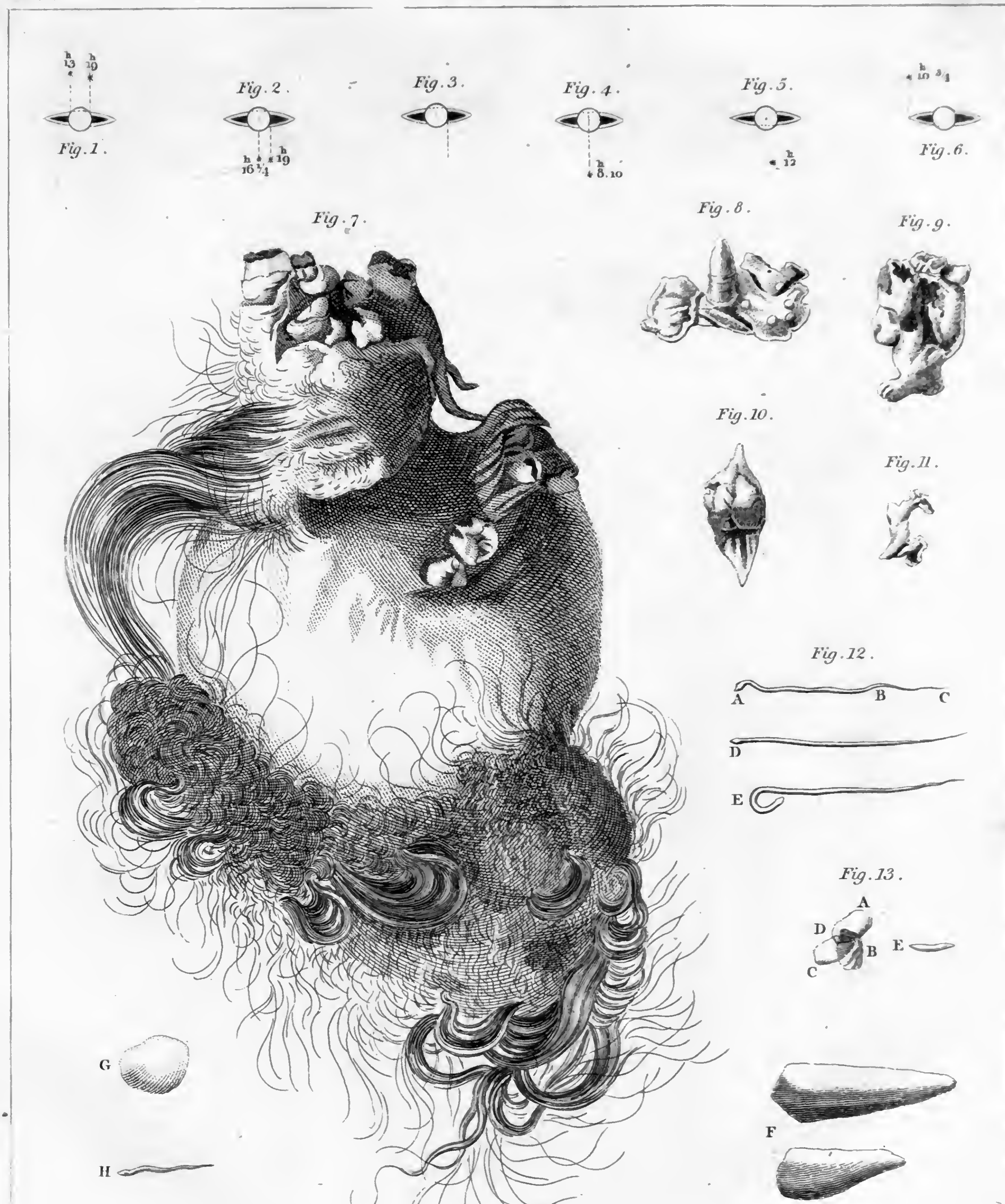

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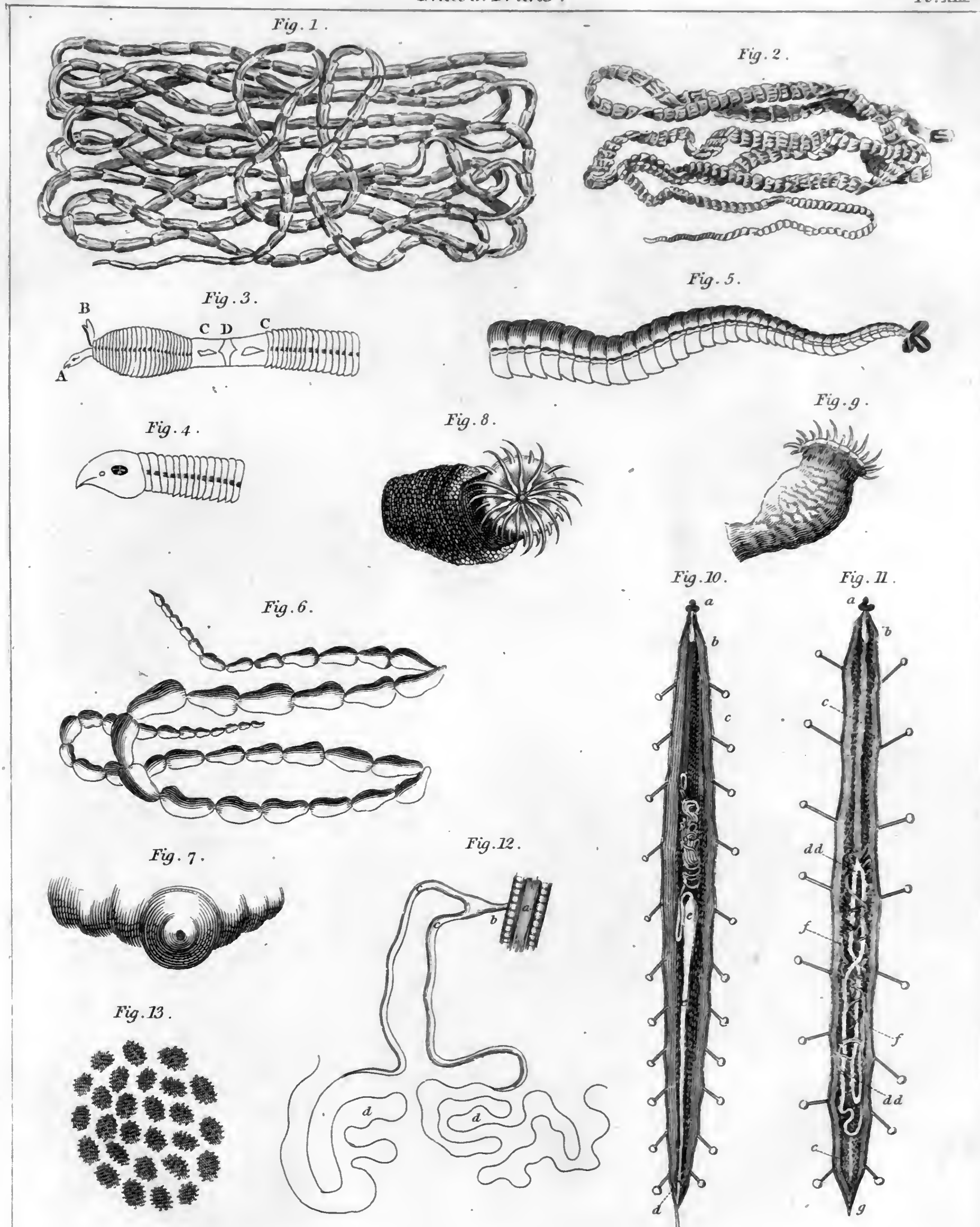

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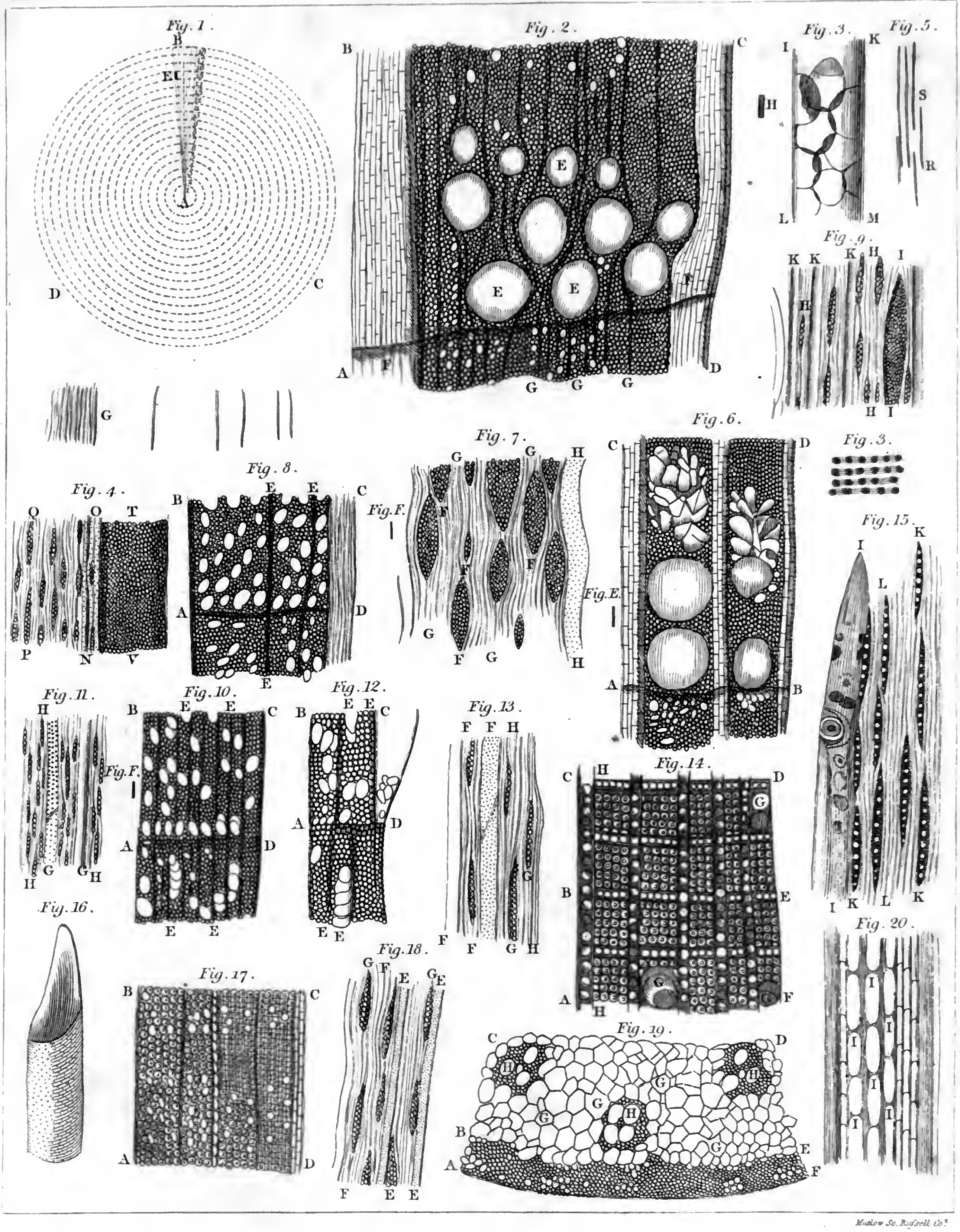

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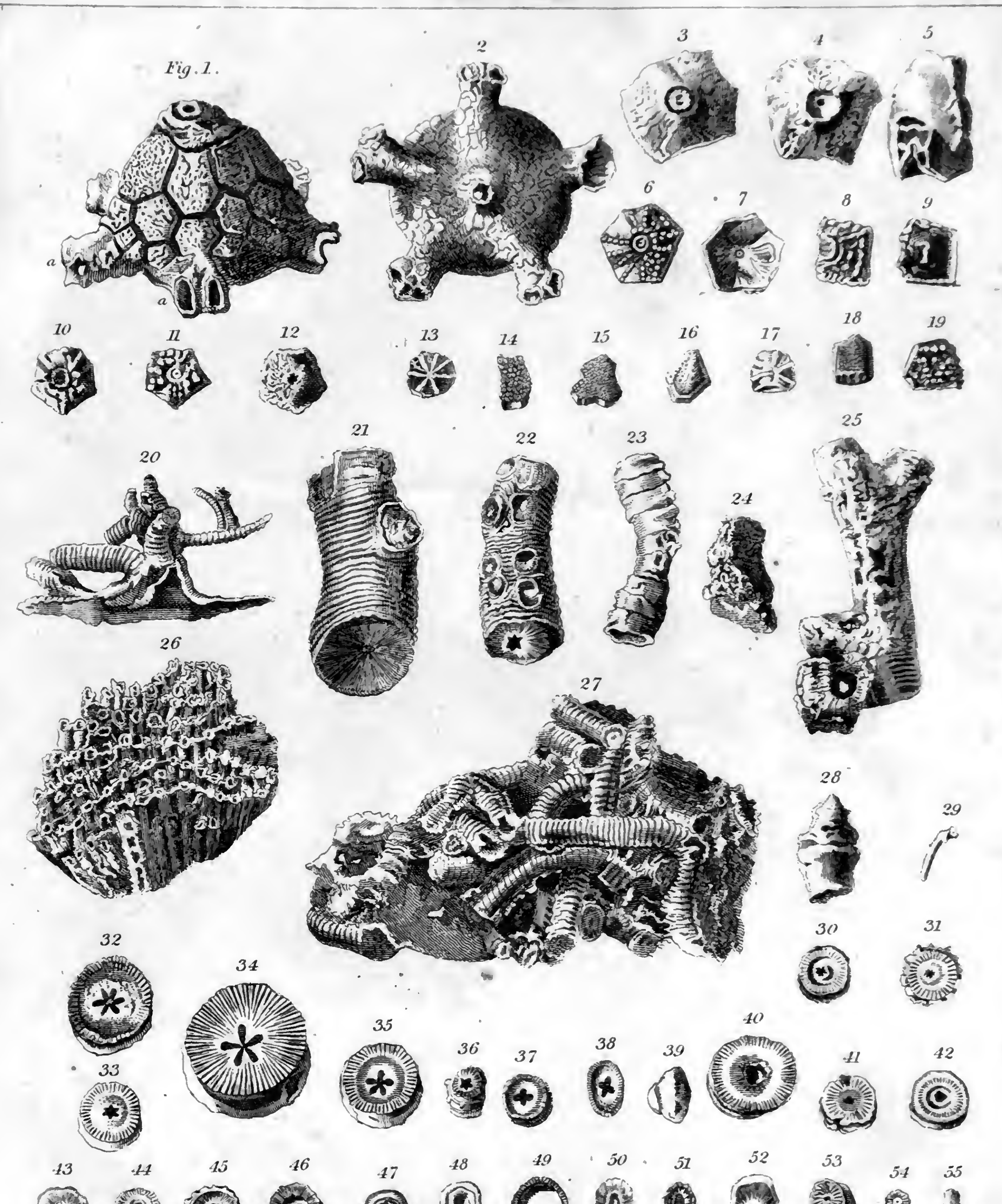

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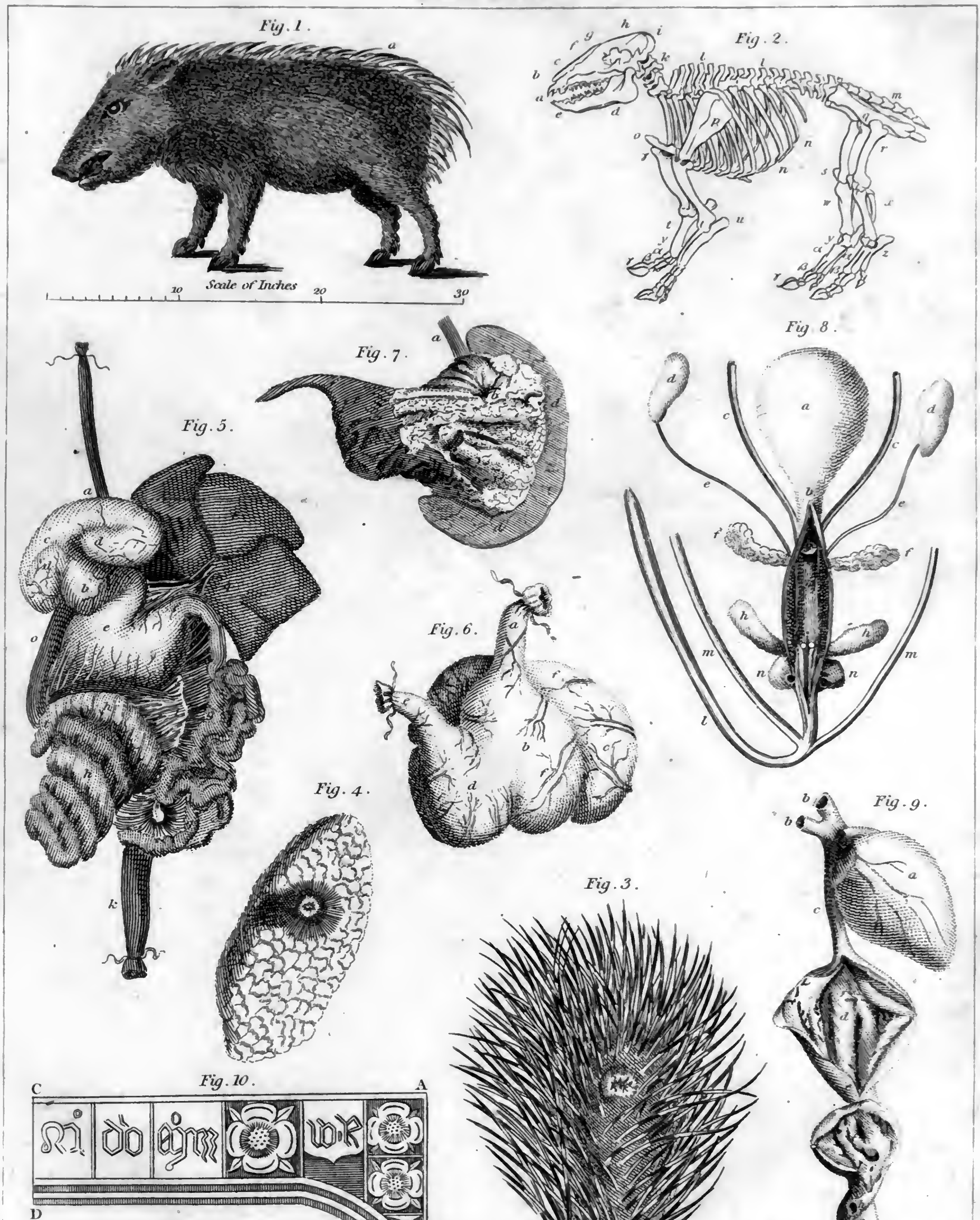

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