THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

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

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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THE

PHILOSOPHICAL TRANSACTIONS

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ABRIDGED.

Remarks by Mr. John Ward, F. R. S. on an Ancient Roman Inscription, in the Possession of Richard Rawlinson, LL. D., F. R. S. found in that part of Italy which formerly belonged to the Sabines. N° 494, p. 293.

This inscription is cut in a small brass plate. The words of the inscription, as they stand on the plate, with some account how and where it was found, were formerly published by Fabretti. They are as follow:

FLORAE
TI PLAVTIVS DROSVS
MAG II
V. S. L. M.

The plate, and the inscription on it, so exactly agree with this account of Fabretti, as to leave no doubt of their being the same with those described by him. The present possessor of the plate purchased it at Rome, in Jan. 1720, N. S. At which time a small brass label was fixed to it, containing the following words cut in capital letters, EX REGIIS CHRISTINÆ THESAVRIS.

The words of the inscription may be read at length, with the proper supplements, in the following manner:

Floræ Tiberius Plautius Drosus, pagi magister anni secundi, votum solvit libens merito.

The goddess Flora was thought by the Romans to preside over fields and trees, and therefore they addressed to her to favour them with prosperous and fruitful seasons. It appears from passages of Varro, referred to by Fabretti, that she was first a Sabine deity, and introduced at Rome by king Tatius in the time of Romulus, many ages before the institution of the Floralia. For that festival was not observed till the year of the city 513, when the expence of it was ordered to be paid out of the fines levied on those persons, who had converted the public lands to their own use, for feeding their cattle.

TI, the two first letters of the prenomen of the person mentioned in the inscription, are the usual abbreviation of Tiberius; as a single T is of Titus.

PLAVTIVS, which follows, denotes the family name, and often occurs in Roman writers, as also on coins, where it is sometimes written Plotius, and at other times Plutius.

DROSVS, the cognomen, he does not remember to have seen so spelt elsewhere, but he doubts not of its being the same as Drusus, which is frequently met with. For thus, as was just now observed, his family name is written three several ways, Plautius, Plotius, and Plutius.

MAG. $\overline{\text{II}}$. according to the explication given above, are an abbreviation of the words magister secundi, which stand for pagi magister anni secundi, was the whole to be expressed at length. The word pagus signifies a division or large portion of land, not much unlike what we call a shire or county.

That the characters $\overline{\text{II}}$. stand for anni secundi, the date of the time, during which this Drosus had then held that office, is confirmed by several inscriptions published by Gruter. In one of which we have MAG. ANNI. $\overline{\text{V}}$; in another MAGISTRI. ANNI. $\overline{\text{VI}}$; and in two others MAG. ANNI. PRIMI, where the word denoting the time is expressed at length. As these different ways therefore of expressing the time relate to persons, who all bore that title, though not the same office, as appears by the inscriptions, they plainly show in what sense those characters are to be taken here.

The concluding letters V. S. L. M. which stand for votum solvit libens merito, contain the usual form of dedicating votive monuments.

But the thing dedicated is not mentioned here, which was most probably a statue or an altar; and probably the latter, from the number of such inscriptions in Gruter, and other collectors of ancient monuments, taken from altars.

Observations of the Comet seen at Pekin in 1748. Also of an Occultation of Mars by the Moon, Dec. 6, 1747; and a Conjunction of Mars and Venus in March 1748; also a Conjunction of Jupiter and Venus Jan. 1, 1748. By the Rev. Father Aug. Hallerstein. N° 494, p. 305. *From the Latin.*

April 26, 1748, about 3 in the morning, this comet was first seen from the observatory at Pekin, in China; when the place of it was rudely taken, viz. in 18° of ♓, with 27° north lat. Its head was equal to a star of the 3d order, and the tail about 1° long. Other observations, when the weather was favourable, were as follow.

April 27, 2^h morn.	21°	20′ ♓ long.	31°	35′n lat.
28, 2	25	15	36	0
29, 2	29	10	40	0

The comet was seen several times after, till the 18th of June; chiefly among several unknown small stars; but in its progress it passed just by the star γ in Cepheus.

Dec. 6, 1747, was an occultation of the planet Mars by the moon; viz. at $5^h\ 34^m\ 34^s$ true time, Mars entered under the moon's obscure limb, and wholly disappeared. At $6^h\ 46^m\ 2^s$ the planet emerged from under the moon.

March 15, 1748, at $6^h\ 28^m$, Mars and Venus were observed, in exact conjunction, almost touching, being nearly equal both in longitude and latitude.

Jan. 1, 1748, was observed a conjunction of Jupiter and Venus. At $5^h\ 15^m\ 41^s$ their distance was $1^\circ\ 3'\ 49''$, Venus being $50'\ 35''$ more south, and $2^m\ 52^s$ of time more west than Jupiter.

An Observation of the Comet of 1748, *and some other Astronomical Observations made at Pekin. By Father Antony Gaubil. From the Latin.* N° 494, p. 316.

These observations nearly agree with those of F. Hallerstein preceding. From June 2 to 7, the right ascension of the comet increased 6° and some minutes, and the declination decreased 55′.

In the conjunction of Mars and Venus 1748, March $15^d\ 8^h\ 10^m$, he observed the distance of the western limbs of the planets to be $1'\ 29''$.

Some eclipses of Jupiter's satellites were observed as below; viz.

True Time.

Oct.	13^d	9^h	40^m	30^s	Emersion of the 3d satellite.
	15	8	37	26	Ditto of the 1st.
	20	10	7	56	Total immersion of the 3d.
	21	5	52	12	Emersion of the 2d, doubtful.
	28	8	29	20	First emersion of the 2d.
Nov.	7	8	52	59	First emersion of the 1st.

Of an Aurora Australis, seen Jan. 23, 1749-50, *at Chelsea. By John Martyn, F.R.S.* N° 494, p. 319.

Jan. 23, 1749-50, about half after 5 in the evening, looking to the S.S.W., Mr. M. thought he saw a reddish light about the planet Venus, which then shone exceedingly bright. Being suspicious of some fire in the neighbourhood, he went immediately to a window on the stair-case, where he saw a reddish light, which shone with such exceeding brightness, that the lustre of the fine constellation of Orion was almost effaced. He then went to a window facing the N.N.E. where he presently saw a very broad band of crimson light, like that which he observed from the same window, March 18, 1738-9; an account of which is printed in the Phil. Trans. N° 461. But in the former the red band was bounded on the north by streams of a greenish blue; whereas the band now observed was entirely of a deep crimson colour, being of a much darker red than the former.

Thence he withdrew into the garden, where he plainly saw a band or arch, of a very deep crimson colour, in appearance about 15° broad, the southern edge of which passed just above Canis Minor, and the shoulders of Orion. It was terminated to the westward, near Venus, then about 20° high: but it extended to the eastward as far as he could see; and the farther it went that way, the deeper was the colour, and the broader the band. About a quarter before 8, there was formed a crown, about 30° to the southward of the zenith. From this crown a great many rays darted to the east, south, and west, but not towards the north, where only some whitish streaks were to be seen, but very faint. Presently after this, the part of the arch extending to the east seemed to be suddenly kindled, as if some train had been fired; grew extremely bright and vivid; and as if all the red matter had been then consumed, put an end to the phenomenon before eight. During the rest of the evening, a pale light covered the south part of the heavens, as if the moon had shone.

Observations made at Rome of the Eclipse of the Moon, Dec. 23, 1749; and of that of the Sun, Jan. 8, 1750. By Mr. Christopher Maire. N° 494, p. 321.

The place of both observations is in the latitude of 41° 54′ 0″, and 4 seconds of time eastward of St. Peter's.

Eclipse of the moon, Dec. 23, 1749.

Chord of the part eclipsed 13′, as deduced from the map of the moon	7^h	47^m	18^s
Hence beginning of the eclipse	7	40	53
End of the eclipse, as far as could be perceived through a thin cloud	10	0	16

He judged the eclipse to be somewhat less than 5 digits.

Solar Eclipse, Jan. 8, 1750.

Beginning by a reflector of Mr. Short, Jan. 7	20^h	34^m	35^s
Quantity of the eclipse 7 dig. 48′	21	49	4
Again more exactly .. 7 43	21	51	28
The sun appears for a moment; horns nearly horizontal	21	56	15
Two digits remain eclipsed	22	55	37
One digit exactly	23	3	42
End of the eclipse	23	11	32

Some Observations on the Dragon-fly or Libella of Pennsylvania, collected from Mr. John Bartram's Letters. Communicated by Peter Collinson, F.R.S. N° 424, p. 323.

About the beginning of May many deformed water-insects, by naturalists called hexipodes, creep up out of the water, and fix themselves on the shrubs and rushes; in this situation they continue but a few hours before their back splits open; and from this deformed case creeps out a beautiful fly, with shining transparent wings: at its first appearance there is only what one may call the rudiments of wings; but it is a most entertaining sight to observe how they shoot out, and expand themselves: thus, in less than an hour, they have attained their complete dimensions. During all this operation the creatures are immoveable, and so continue till their wings are dry; and then they fly swiftly away, roving about the sides of ponds and rivers, seeking their food, being insects of prey, are very voracious, and, like the hawks among birds, are very swift of flight, and nimbly secure their prey, which is mostly flies, and small green grashoppers: they delight in sun-shine: in cloudy weather they are rarely to be seen; but seek protection under the leaves and boughs of trees.

Towards the end of May the female is ready to deposit her eggs. She then seeks the warm quiet sides of ponds and water courses, continuing in a hovering posture, dodging up and down in the water: in this action the male seizes her, and with the end of his tail catches fast hold by the back of her head, and so flies away with her. It is uncertain how long they continue in this position before the female bends the end of her body, so as to penetrate the part between the belly and breast of the male. In this singular and surprising manner she is impregnated; she then repairs again to those still shallow waters, whose bottoms are covered with moss, sticks, and weeds, which may be a security to the little grubs. Here in a hovering posture she deposits her eggs in the water, which immediately sink, and find a proper nidus in the aquatic moss, &c. The eggs are soon hatched; the young reptiles creep among the stones and weeds, &c. and so continue water animals the greatest part of the year, till the season comes round for their appearance in that beautiful fly before mentioned, which is different from the European; but their process agrees with the European, as it is described by M. Reaumur. There is a great variety of this tribe of insects in America as well as in Europe.

Some Experiments on Respiration. By Albert Haller, Archiater, Professor of Physic at Gottingen, and F.R.S. Abstracted from the Latin. N° 494, p. 325.

The experiments related in this communication were made with a view to determine 2 disputed points on the subject of respiration. 1. Whether the internal intercostal muscles elevate the ribs, as Mayow asserts, or depress them, as Galen, Boyle, and others maintain? 2. Whether there is air, in an elastic state, between the pleura and lungs, or whether the lungs come into close contact with the pleura?

Relatively to the first question, Professor H. describes the following experiment. Having tied down a dog, let the pectoral muscles be laid bare by dissecting away the skin, especially from the upper part. Next let both the pectoral muscles be removed, and let so much of the external intercostal muscles be cut away, as shall suffice for observing the condition and action of the internal intercostals. It will be desirable to force the animal to breathe as strongly as possible; which may be done either by puncturing one side of the thorax, so as to let in the air (thus rendering one of the lungs useless); or, without puncturing the thorax, by applying some spirit of wine to the wounded parts. In this state of things, it will be seen, that in the act of inspiration, the spaces between the ribs are diminished more than one-half; that the internal muscles are brought into violent action; that they swell and become hard; that all the ribs ascend except the first rib, which is scarcely moved; that they all turn round an imaginary point, which is in the cartilaginous appendix not far from the sternum; that part of the rib which is joined to the sternum descending, while the part which is remote from it, ascends and is turned outwards. In expiration all the ribs descend, with the exception again of the first rib, which is scarcely moved; the spaces between the ribs, during a violent expiration, are increased, and the internal muscles remain inactive.

In regard to the other question: let an animal (no matter whether dead * or alive) be plunged under water, and let the pleura be perforated. If there be any air, in an elastic state, between the pleura and lungs, bubbles will rise up through the water. If no bubbles ascend, there is no air. Professor H. tried this experiment repeatedly; but no bubbles ever appeared. He therefore infers, that no air is present, in an elastic state, between the pleura and the lungs.

On the Knowledge of Geography among the Chinese; and of Paper-money current there. By Father Anthony Gaubil, Jesuit. Translated from the French by T. S., M. D., F. R. S. N° 494, p. 327.

Father Gaubil received from M. de L'isle part of a map of the world, found among the papers of the late Dr. Kæmpfer. In this map were several Chinese characters, some well, some ill written, which the late professor Bayer had attempted to decypher. In his answer to M. de L'isle, Father G. informed him that it was by no means a Chinese work; that it could be of no service to a learned European; and that Mr. Bayer's explanations were full of faults. There is no monument extant to prove, that before the arrival of the Jesuits in this country, the Chinese had charts or maps of the world, any way resembling that found among Kæmpfer's writings.

It is now above 1600 years since they tolerably well knew the northern and

* Recently dead.

eastern countries of India, and those which lie between China and the Caspian sea. On these different countries their history affords several informations, which are not to be found in the Greek, Latin, or other historians. They had some notions, but very confused, of the regions beyond the Caspian sea; such as Syria, Greece, Egypt, and some parts of Europe. He does not speak of the times of Gentchiskan and his successors; for then the Chinese were made acquainted with Russia, Poland, Germany, Hungary, Greece, &c. from accounts given by their own countrymen who followed that prince, his sons, and grandsons: but the monuments that remain of this their knowledge are very confused. As to the countries to the east of China, there are proofs remaining in books, that above 1700 years since, the Chinese were well acquainted with the eastern part of Tartary as far as the sea, and the river Ameur, Corea, and Japan. Their books speak also in general, and without sufficiently entering into particulars, of many countries to the east and to the north of Japan. With regard to the monuments of the Cape of Good Hope, which have been mentioned by some, there are none in China; and if there have been any, they are now lost. It was from the Europeans that the Chinese have learnt the name and the situation of the Cape.

Two paper money-bills are of the reign of Hongvou. The year of Christ 1368, was the first of the empire of Hongvou, founder of the dynasty of Ming. During the dynasty of Yuen (who were Mogul Tartars) which Hongvou destroyed, there was a great deal of paper money. There had also been some, 140 years before, under the dynasty of Kin (Oriental Tartars) who reigned in the northern provinces of China, and in Tartary. The Yuens destroyed this dynasty, as well as that of the Song's, who were Chinese, that reigned in the southern provinces of China. We find no paper money of the dynasties of the Yuens and Kins; and that of Hongvou is scarce. The Bonzes and Chinese empirics superstitiously say, that this paper money laid upon children, brings them good luck.

These 2 bills are the same with those, the figures and explanations of which are to be seen in Father du Halde's Description of China, tom. 2, p. 168.

A Catalogue of 50 *Plants from Chelsea Garden, presented to the Royal Society, by the worshipful Company of Apothecaries, for the Year* 1747, *pursuant to the Direction of Sir Hans Sloane, Bart., F.R.S.* N° 494, p. 331.

[This is the 26th presentation of this kind, completing to the number of 1300 different plants.]

Concerning the Green Mould on Fire-wood; with some Observations of Mr. Baker's on the Minuteness of the Seeds of some Plants. By the Rev. Henry Miles, D. D., F. R. S.* N° 494, p. 334.

Happening to take notice of a quantity of what is commonly called mould, of a bright verdigrise colour, on the bark of some fire-wood, Dr. M. viewed it with a lens, of about an inch focus, which he then found to consist of numbers of minute funguses, whose regular appearance invited him to examine them in the microscope, with a good magnifier; when their spherical heads seemed as if they had been nothing else but globules of seeds; at the same time, he observed several seeds adhering to the transparent foot-stalks, which supported the heads, and many scattered on the glass plate, where the substance was placed, in order to be viewed. And here he saw many distinct seeds, which appeared nearly of an oval form, but several times larger than the seeds of common mushrooms, even when seen with the second magnifier, and the latter with the first.

Having often viewed the heads of a small kind of fungus, which are about $\frac{1}{8}$ inch diameter, of a coriaceous substance, he always found the seeds, which are produced on the gills, much larger than those of any mushrooms he ever examined, though rather less than those produced by this unregarded plant.

Now, that a body whose form is not to be distinguished by the unassisted eye, should produce seeds several times larger than another of the same genus does, which exceeds it many millions of times in bulk, must suggest very curious thoughts to our mind.

Some Observations on the above-mentioned Plants and Seeds. By Henry Baker, F.R.S. N° 494, p. 337.

Mr. B. carefully examined the plants and seeds sent him by Dr. Miles, in order to determine their real size; and he found that the diameter of these fungous bodies was at a medium, the 210th part of an inch. The seeds were oval; and the medium of each diameter was the 2430th part of an inch.

And according to these calculations, 44,100 of the fungous heads, or 5,904,900 of the seeds may lie by each other in the surface of an inch square. Yet, minute as the seeds of this little fungus are, Dr. Miles observes, very justly, that they are larger than the seeds of some mushrooms, which exceed it many millions of times in size. As to which, Mr. B. takes notice, that the proportion in size, of the fruits or seeds of trees or plants, to the size of the trees or plants that bear them, comes under no regulations that correspond with our conceptions. For the vast bulk of some sorts of timber-trees, the beech and ash for instance, is produced from a seed smaller than that of the common garden bean. The towering and mighty oak produces for its fruit only a little acorn, whereas

* The plant here described appears to be a species of the Linnæan genus *Mucor*.

the pumkin, sometimes weighing above a hundred pounds, is the production of a feeble creeping plant, unable to support itself, and much less its enormous fruit. The vanilla (a plant that rises to the height of several feet, by clasping about whatever it finds near it), produces, in long pods, seeds so small, that their diameter is not more than the 100th part of an inch. Supposing therefore the cavity of the pod to be equal to a cylindrical tube of $\frac{1}{10}$ of an inch diameter, and the length of the pod to be 6 inches, the number of seeds contained in one single pod will be more than 47000. Most kinds of fern, of which some are pretty large plants, bear seeds so extremely minute, that they appear to the naked eye only like a fine dust; while seeds of a considerable size are produced by plants of a much smaller size.

An Observation of the Eclipse of the Sun on Jan. 8, 1750, N.S. *taken at the Observatory at Berlin. By M. Grischow, jun. and M. Kies. Translated from the French.* N° 494, p. 339.

The beginning was at .... 8^h 59^m $19\frac{1}{4}^s$ true time.
The end of the eclipse at .... 11 20 $5\frac{1}{4}$

M. Euler observed in his own house, which stands a little to the west of the s.w. of the observatory, at the distance of 190 Rhinland yards (verges) in a straight line, that

The beginning was at .... 8^h 58^m 30^s true time.
And the end at 11 19 50

That is, 34^s more than at the observatory.
The diameter of the umbra was $6\frac{1}{4}$ Rhinland inches.

Observations on the Course or Passages of the Semen. By Albert Haller, Prof. of Physic at Gottingen, F.R.S., &c. From the Latin. N° 494, p. 340.

Professor H. observes, that the filamentous structure of the testicles, composed of minute yellowish vessels, is sufficiently known; but that respecting the passages by which the semen, secreted in those vessels, is transmitted to the epididymis, much uncertainty has always prevailed. De Graaf alone saw this matter in its true light, succeeding writers having added nothing to his discoveries.

Let the epididymis be slowly and cautiously injected with quicksilver, the operator stopping every now and then, or plunging the testicle into warm water, that the vessels may gradually expand; for if the epididymis were suddenly filled with quicksilver, it would burst.

By repeated injections conducted in this manner, Professor H. found that the epididymis, throughout its whole length, where it adheres to the testicle, (the head excepted) is composed of a single canal, which may be unravelled from its plicæ or folds, connected together by cellular membrane. This was rightly seen by De Graaf, de Part. Genital. Viror. p. 65.

In the upper part of the epididymis, which he calls the head, and which is firmly attached to the tunica albuginea, the structure is different. For there this single canal is divided into 10, 12, or more, canals. These Prof. H. calls vasa efferentia semen.

Folded up, and collected into a cone, each forms a distinct fasciculus, and by a retrograde duct returns towards the lower part of the testicle, and enters into the middle of it, at the place where the epididymis, loose on one side, adheres only on the other. De Graaf has given a pretty good delineation of these vessels and cones. These vessels are gradually extended on the surface of the testicle, which is continuous with the albuginea, and running parallel and conjoined,* they form a net (rete).

From this conjunction † it often happens that when only 1 or 2 of the vasa efferentia are injected from the epididymis, all of them become filled. De Graaf alone saw and delineated this structure; but he has represented the vessels too parallel, and not joined together by intermediate branches. He has also made them too long. All other anatomists have taken them either for a single duct, or for a blind membrane.

From the rete or net are sent forth vessels without plicæ or folds, and which from their straight direction, Prof. H. terms recta vascula testis. They are larger than might be supposed, and more tender than the canal of the epididymis, so as to be easily burst by the weight of the quicksilver. Into these straight vessels (recta vascula) are inserted the yellowish serpentine vessels of the testicle, which Ruysch so beautifully resolved into hair-like filaments. It has sometimes happened that the quicksilver has entered even these exceedingly minute and tender vessels, so as to place it beyond all doubt that they are hollow canals.

Thus it appears that the yellowish-coloured semen is generated in the serpentine vessels (vascula serpentina); that it passes from thence into the straight vessels, (vasa recta); that by the straight vessels it is conveyed into the rete or net; and that from the rete or net it is carried through the tortuous vascula efferentia into the epididymis.‡

From the epididymis the passage of the semen is two-fold, one by no means obvious, the other well known, and leading to the vesiculæ. The first of these

* By intermediate branches. † Connexion by intermediate branches.

‡ The structure of the testicles in man, and other animals, was afterwards farther illustrated by the present professor of anatomy at Edinburgh, Dr. Monro, in his inaugural thesis De Testibus et Semine in Variis Animalibus, published in 1755.

passages Prof. H. had thrice succeeded in filling with quicksilver, at the date of this memoir (1750).

A single vessel (for he had not seen more than one) goes straight from the middle of the epididymis, and ascends with the vessels of the testis. Professor H. could not trace this vessel to its termination, but he doubts not that it belongs to the lymphatic vessels, which he had often seen in the spermatic cord of the human subject. These are the exceedingly minute vessels in the abdomen (for though Prof. H. had hitherto seen but one, he would not deny there might be more) which, in the hare, Ruysch (Catal. Mus. p. 152) injected by the vas deferens. Professor H. thinks it highly probable that this vessel (or vessels) may serve for absorbing the more fluid part of the semen, and rendering it thicker.

The other passage, by which the semen is conveyed to the vesiculæ seminales, is easily traced. By injecting this passage with quicksilver, Prof. H. observed some things which deserve to be mentioned. After slightly noticing that the ductus deferens is continued straight into the urethra, and that the excretory duct of the vesicula seminalis is inserted into it at a very acute angle, and that notwithstanding the excretory duct is smaller than the urethral duct, yet liquors injected into the ductus deferens easily pass into the vesicula; he proceeds to what he principally wishes to point out, viz. that each vesicula seminalis is a small intestine (intestinulum), into which are inserted many blind appendages (cæcæ appendices). This is clearly seen when the vesicula is filled with quicksilver or wax, and afterwards dissected, carefully removing the portions of cellular membrane, by means of which both the principal intestinulum and blind appendages are held together. In several preparations thus conducted, Prof. H. found that the appendices varied considerably in length, simplicity, direction, and diameter; but in all instances he found the principal intestinulum of the vesicula to terminate in a thick, obtuse, blind cone. Into this intestinulum are inserted 8 or 10 appendices, the first of which are commonly, but not constantly, branched; the last are rather simple. Something similar was observed by Leal Lealis, and Henricus Bassius, Obs. Anat. Chir. Dec. 1, n. V. T. 2 ff; but both these authors have made the appendices too small, and Bassius moreover has added an anulus, which Prof. H. asserts to be a plica, and not a true circle. He adds, that these appendices are so large and so complex, that it is difficult to determine which is the trunk, and which the appendix.

Fig. 1, pl. 13, represents the testicle filled with quicksilver; a the ductus deferens; b the lower part where it begins to ascend under the name of epididymis; c the whole epididymis injected, composed of a single serpentine vessel; d the head of the epididymis; eeeee so many coni vasculosi, of which the head of the epididymis is composed; ff the vascula efferentia arising from the cones. They are marked with few letters, not to spoil the engraving; gg the rete of the testicle; hh some rectilinear ducts. The other globules were separated from the quicksilver, effused through some of the ruptured vessels, as here delineated, that the engraving might in every respect exactly represent the original preparation; i the flesh of the testicle laid bare (caro testis nuda).

In fig. 2, pl. 13, aa is the urinary bladder; b the posterior plane of the longitudinal fibres; c the prostate; dd the ureters; e the arteries of the vesiculæ; fg the ductus deferentes at their cellular extremity; h the right vesicula seminalis, as it naturally appears; i the seminiferous duct, which perforates the prostate; l the left vesicula seminalis, filled with wax; mm the blind appendages of the vesiculæ, which appendages were short in this subject; nn some branched appendages; o the seminiferous duct going through the prostate; p the excretory duct of the vesicula inserted into it.

Concerning an Aurora Borealis seen Feb. 16, 1749-50. *By John Martyn, M.D. F.R.S. Dated Chelsea, Feb.* 21, 1749-50. N° 494, p. 345.

On Friday the 16th there was a bright aurora borealis, the northern part of the sky being entirely filled with a pale light, in which frequent coruscations were visible. Besides these lights, there was a perfect uniform arch, extending from east to west, the colour of it was the same with that of the aurora; with which however it did not seem to have any communication, being placed several degrees to the southward. The shoulders of Orion were visible through this luminous arch, in the western part of it, and Cor Leonis in the eastern part.

Concerning an Aurora Borealis seen Jan. 23, 1750-51. *By the Rev. Henry Miles, D.D., F.R.S.* N° 494, p. 346.

On Tuesday, Jan. 23, last, about 6 in the evening, Dr. M. saw a cloud (not large) of an obscure red colour, but much deeper than any he had ever seen before, which rose from the S. W. it was then advancing apace to the N. E. and quickly reached the zenith, when there appeared a luminous zone, about the breadth of the galaxy, its edges regularly defined, compassing the hemisphere, from the horizon in the N. E. to the zenith, in the same direction in which the above-mentioned cloud had passed, from the S. W. The colour was much fainter, and more luminous, resembling the usual colour of an aurora, and the laminæ or streamers soon appeared.

A Letter from Mr. William Watson, F.R.S. to the Royal Society, declaring that he as well as many others have not been able to make Odours pass through a Glass by means of Electricity; and giving a particular Account of Professor

Bose's (at Wittemberg) Experiment of Beatification, or causing a Glory to appear round a Man's Head by Electricity. N° 494, p. 348.

The inquiry into the nature and properties of electricity has been, within these few years, the pursuit of many excellent and ingenious persons; and most of its extraordinary phenomena, which have been made to appear in one place, have, with proper attention to the requisite circumstances, appeared in others: but there have happened two very remarkable exceptions to this rule. The first is, that the odours of odoriferous substances do not only pervade, from friction, the glasses which contain them, but that these odours were carried along with the current of electricity into such non-electric bodies as were destined to receive them, and manifested themselves in those bodies by communicating to them their smell and other properties. These, and other things yet more extraordinary, were said to have been performed by Mr. Pivati at Venice, and to have been repeated by Mr. Winkler at Leipsic; but, though no care or expence has been spared, either by Abbé Nollet at Paris, Mr. Jallabert at Geneva, Mr. Bose at Wittemberg, Pere Garo at Turin, and by Mr. W. himself at London, to bring about the same effects, they have hitherto been unsuccessful. For which reason the truth of these relations has been greatly questioned by many; as Mr. Buccamare, in a treatise* since published, says, that Mr. Pivati confessed to those who addressed themselves to him to see the experiments, that more especially made with balsam of Peru, that it never succeeded but once, and that he could never repeat it. Mr. W. likewise received a letter from the Abbé Nollet, who is just returned to Paris from Turin and Italy. He says, that his first care was to inquire into the truth of those wonders in electricity, of which we have heard so much for almost 3 years: and he imagines the Royal Society would be glad to know what they really were: for which reason he has just now sent a memoir to the duke of Richmond, in which will be seen the most circumstantial account he has been able to procure of them at Turin, at Venice, and at Bologna. For his own part, he thinks that there has been a great deal of prejudice, credulity, and exaggeration; to which may be added, very little care and caution in making these experiments. He is now sorry he has lost so much time in attempting to make them; and thinks Mr. Winckler has been too hasty in asserting, that he had repeated these Italian experiments: but why should he call them Italian, when the nation he says will not allow the appellation, and, except 3 persons, he finds there no defender of what has been said to be done; and adds, that there is not a philosopher of repute there, who believes them any more than himself?

The other is, an experiment called by Professor Bose at Wittemberg, the

* Tentamen de Vi Electric. &c. p. 183.—Orig.

apotheosis or beatification. The making this experiment, in the manner mentioned by this gentleman in his writings, has been attained to by none. He says, if in electrizing you employ large globes, and place a man on a large cake of pitch, by little and little a lambent flame arises from the pitch, and spreads itself around his feet; from hence by degrees it is propagated to his knees, his body, and at last to his head: that then, by continuing the electrization, the man's head is surrounded by a glory, such a one in some measure, as is represented by painters in their ornamenting the heads of saints: that in this state if the electrized man is touched by one that is not, the pain felt by both is very severe, reaches from the finger to the shoulder, and remains a long time. Professor Bose, in another part of his writings, says that the beatification indeed does not always succeed with him; that sometimes, when other circumstances have been very favourable, a man will be beatified by 1 sphere in 2 minutes; at other times, 2 or 3 globes will not do it under 6 or 8 minutes; and even at sometimes after 20 minutes, when 5 or 6 globes were made use of, no light has been visible: that under the same circumstances, when one person was capable of being beatified, another was not. This is a short account of Professor Bose's beatification, given in his writings, in which nothing of what he says essential to the operation is omitted.

This experiment, which was not only a desirable thing to be seen, but as it seemed to communicate to non-electric bodies a greater quantity of electricity than any other did, that of Leyden excepted, Mr. W. was very desirous of repeating: but though he omitted no trouble, and varied not the least circumstance, that could any ways conduce to it, he was disappointed. He tried the combined force of many globes, of different machines, in the best weather, and with different persons, but no radiation in the manner before mentioned. When he underwent this operation himself, supported by solid electrics per se of more than 3 feet high, and as much distant from the sides of the room as possible, to prevent the escaping of the electric matter, he found in himself, as several others did, a tingling on the skin in his head, and in many parts of his body such a sensation as would be felt from a vast number of insects crawling on our bodies at the same time; but he constantly observed this sensation to be greatest in those parts of his body which were nearest any non-electric; but still no light on the head, though to make the eye more ready to observe it, this experiment was made in the dark for some continuance. The sensation of the snaps in this state were very acute. If the hand of a bystander was brought near the back of the hand of the person electrized, the hairs on it sent forth a great number of luminous points; and if a bunch of fine lace wire was placed on his head, you saw a great deal more of the same appearance; but this was always most brilliant in those parts nearest the non-electric, and still more, when the non-electric was brought to a proper distance. But this was vastly short of that mentioned by Mr. Bose, not only in its lustre, but as it never was general, hardly ever showing itself in 2 parts of the body at the same time. This want of success after many trials, as he by no means doubted Mr. Bose's veracity, induced him to conclude, that either some very essential part of the apparatus had been suppressed by the author, or that the air of Germany, being on the continent, was more dry, and more fit, than that of our island. It was difficult indeed to allow this last, as the experiment had failed here, after the long continuance of a very dry season. This want of success occasioned many persons here, well versed in these matters, to conclude, that the experiments in electricity had been carried farther in Germany than in England.

However, some time after, Mr. W. found that this experiment, in the manner before mentioned, had been made no where on the continent, Wittemberg excepted; and Mr. Jallabert at Geneva, in his Treatise on Electricity, says, that he had likewise attempted it; but instead of beatification, he saw from the hair of the head of the person electrized, especially from the back part, a great number of luminous points. These, he says, were likewise observable on his clothes, which were made of a mixture of thread and cotton, more especially on their borders. When the person electrized changed his situation on the pitch, on which he stood, the place he left appeared luminous. What this gentleman mentions besides is very nearly like to what Mr. W. experienced, as above related. Mr. J. says also, that he believes Mr. Bose had been the only person, who had made the beatification succeed.

A person here however exhibited to the public the famous experiment of beatification, found out, as he says, by a German professor. Whether he knew how this experiment was said to be done, or whether it was with him as with many of the discoverers of the longitude, and of the quadrature of the circle, Mr. W. does not determine; but thus it is, that his experiment has been exhibited as Mr. Bose's for 2 or 3 years.

Mr. W. is unwilling to be thought to detract from the merit of this experiment, which he thinks a very beautiful one; but he takes upon him to say, that it differs as essentially from every part of that said to have been made by Prof. Bose, as any 2 electrical experiments whatever.

In a letter Mr. W. wrote afterwards to Mr. Bose, among other things acquainting him of not being able to make the beatifying experiment succeed; and that nobody any-where had been able to do it, so that the power of seeing this extraordinary phenomenon was yet with himself alone. Mr. W. desired of him farther, that if any material part of the process had been omitted in his writings, he would communicate it; for that some people here were not quite satisfied of its having ever been made. To this he was so obliging as to send an answer nearly in the following words: 'As to my beatification, I am highly obliged to you for writing to me so freely and candidly about it; and I will discover to you my whole artifice without any retention, though I concealed the same from all my friends and correspondents. Now, sir, it is true that I have embellished a little my beatification by my style and expressions; but it is also true, that the basis of the phenomenon is constant. I found in our armoury at Leipzic, a whole suit of armour, which was decked with many bullions of steel; some pointed like a nail; others in form like a wedge; others pyramidal. In the dark, you well know, that not all, but very many, of the said bullions will sparkle and glister with tails like comets: and it is clear, that when the electricity is very vigorous, the helmet on the head of the person electrized will dart forth rays like those round the head of a canonized saint; and this is my beatification. You are the first, sir, with whom I trust my mystery, which if you communicate to the R. S. I hope you will take care of its being inserted in the Phil. Trans., that the beatification did not succeed until I communicated my method. Many people have imagined this experiment of mine to be extravagant and false. If the armour is not ornamented with steel bullions, I believe it will not succeed. If the armour is well enriched with bullions, and well polished, the comets appear twice, once in the air, and once by reflexion from the armour. A stomacher, or a doublet, set with nails or needles, will exhibit a small degree of beatification.'

Part of a Letter from Mr. Professor Euler, to the Rev. Mr. Wetstein, Chaplain to his Royal Highness the Prince, concerning the Contraction of the Orbits of the Planets. Translated from the French by T.S., M.D., F.R.S. N° 494, p. 356.

You have done me much honour, says Mr. E. in communicating an extract of my last letter * to the illustrious R. S., November 2, 1749. I am still thoroughly convinced of the truth of what I advanced therein, that the orbs of the planets continue to be contracted, and consequently their periodical times grow shorter. But in order to put this fact out of doubt, we ought to be furnished with good ancient observations, and also to be very sure of the time elapsed, since those observations, to this day: which we are not, with regard to the observations that Ptolemy has left us. For chronologists, in fixing the moments of those observations, ran into a mistake, by supposing the sun's mean motion to be known; which ought rather itself to be determined by these same observations. Now, if we reduce the days marked by Ptolemy to the Julian calendar, we run the risk of committing an error of a day or two, in the whole number of days elapsed,

* See Phil. Trans. N° 493.—Orig.

from that to our time; because the course of the Julian years, according to which every 4th ought to have been bissextile, has been frequently interrupted by the pontifices; of which we find some sure marks in Censorinus and Dion Cassius. Hence it might well happen, since the times marked by Ptolemy, that there has really been a day or two more than we reckon, and consequently, that Ptolemy's equinoxes, ought to be put a day or two back; which would lengthen the years of those times. I was in hopes that the Arabian observations would not be liable to this inconvenience; because the Julian calendar has not been interrupted for these last past 1200 years. The late Dr. Halley had also remarked, that the revolutions of the moon are quicker at present than they were in the time of the ancient Chaldeans, who have left us some observations of eclipses. But as we measure the length of years by the number of days, and parts of a day, which are contained in each of them; it is a new question, whether the days, or the revolutions of the earth round its axis, have always been of the same length. This is unanimously supposed, without our being able to produce the least proof of it: nor indeed do I see how it could be possible to perceive such an inequality, in case it had really existed. At present we measure the duration of a day by the number of oscillations, which a pendulum of a given length makes in this space of time: but the ancients were not acquainted with these experiments, by which we might have been informed, whether a pendulum of the same length made as many vibrations in a day formerly as now. But even though the ancients had actually made such experiments, we could draw no inferences from them, without supposing, that gravity, on which the time of an oscillation depends, has always been of the same force: but who will ever be in a condition to prove this invariability in gravity? thus, even supposing that the days had suffered considerable changes; and that gravity had been altered suitably to them, so that the same pendulum had always completed the same number of vibrations in a day; it would nevertheless be still impossible for us to perceive this inequality, were it ever so great. And yet I have some reasons, deduced from Jupiter's action on the earth, to think, that the earth's revolution round its axis continually becomes more and more rapid. For the force of Jupiter so accelerates the earth's motion in its orbit round the sun, that the diminution of the years would be too sensible, if the diurnal motion had not been accelerated nearly in the same proportion. Therefore, since we hardly at all remark this considerable diminution in the years, from thence I conclude, that the days suffer much the same diminution; so that the same number will answer nearly to a year.

A Catalogue of the 50 Plants from Chelsea Garden, presented to the Royal Society by the Worshipful Company of Apothecaries for the Year 1748, pursuant to the Direction of Sir Hans Sloane, Bart. &c. N° 494, p. 359.

[This is the 27th annual presentation of this kind, completing to the number of 1350 different plants.]

A Surprising Inundation in the Valley of St. John's near Keswick, in Cumberland, August 22, 1749, in a Letter from a Young Clergyman to his Friend. Communicated by John Lock, Esq., F.R.S. N° 494, p. 362.

This remarkable fall of water happened at 9 o'clock in the evening, in the midst of the most terrible thunder, and incessant lightning, ever known in that part in the memory of the oldest man living, the preceding afternoon having been extremely hot and sultry. And what seems very uncommon, and difficult to account for, the inhabitants of the vale, of good credit, affirm they heard a strange buzzing noise like that of a malt-mill, or the sound of wind in the tops of trees for two hours together before the clouds broke. From the havock it has made in so short a time, for it was all over in less than 2 hours, it must have far exceeded any thunder-shower that we have ever seen. Most probably it was a spout or large body of water, which, by the rarefaction of the air, occasioned by that incessant lightning, broke all at once on the tops of these mountains, and so came down in a sheet of water on the valley below.

This little valley of St. John's lies east and west, extending about 3 miles in length, and half a mile broad, closed in on the south and north sides, with prodigious high, steep, rocky mountains: those on the north side, called Legburthet Fells, had almost the whole of this cataract. It appears also that this vast spout did not extend above a mile in length; for it had effect only on 4 small brooks, which came trickling down from the sides of the rocky mountains. But no person, that does not see it, can form any idea of the ruinous work occasioned by these rivulets at that time, and in the space of an hour and half. At the bottom of Catcheety Gill, which is the name of the greatest, stood a mill and a kiln, which were entirely swept away, in 5 minutes time, and the place where they formerly stood, now covered with huge rocks, and rubbish, 3 or 4 yards deep. One of the mill-stones cannot be found, being covered, as is supposed, in the bottom of this heap of rubbish.

In the violence of the storm, the mountain has tumbled so fast down, as to choak up the old course of this brook; and it has forced its way through a shivery rock, where it now runs in a great chasm, 4 yards wide, and between 8 and 9 deep. In the course of each of these brooks, such monstrous stones, or rather rocks, and such vast quantities of gravel and sand, are thrown on their little meadow-fields, as render the same absolutely useless, and never to be recovered.

It would surpass all credit to give the dimensions and weight of some rocks, which are not only tumbled down the steep parts of the mountains, but carried a considerable way into the fields, several thrown on the banks larger than a team of 10 horses could move. Near a place called Lobwath, one was carried a great way, which was 676 inches, or near 19 yards about. The damage done to the grounds, houses, walls, fences, highways, with the loss of the corn and hay then on the ground, is computed variously, by some at 1000l. by others at 1500l.

One of these brooks, which is called Mose or Mosedale Beck, which rises near the source of the others, but runs north from the other side of Legburthet Fells, continues still to be foul and muddy, having, as is supposed, worn its channel so deep in some part of its course, as to work on some mineral substance, which gives it the colour of water hushed from lead mines, and is so strong as to tinge the river Derwent, into which it empties itself, even at the sea, near 20 miles from their meeting.

Of an Extraordinary Fireball Bursting at Sea. By Mr. Chalmers. N° 494, p. 366.

Nov. 4, 1749, in the latitude of 42° 48′, longitude 9° 3′, the Lizard then bore N. 41° 5′ about the distance of 569 miles, as Mr. C. was taking an observation on the quarterdeck, about 10 minutes before 12 o'clock, one of the quartermasters desired he would look to windward, which he did, and observed a large ball of blue fire rolling on the surface of the water, at about 3 miles distance from them. It came down upon them so fast, that before they could raise the main tack, they observed the ball to rise almost perpendicular, and not above 40 or 50 yards from the main chains: it went off with an explosion as if hundreds of cannon had been fired at once; and left so great a smell of brimstone, that the ship seemed to be nothing but sulphur. After the noise was over, which did not last longer than half a second, they found the main-topmast shattered into above a hundred pieces, and the mainmast rent quite down to the heel. There were some of the spikes, that nailed the fish of the mainmast, drawn with such force out of the mast, that they stuck in the main deck so fast, that the carpenter was obliged to take an iron crow to get them out: five men were knocked down, and one of them greatly burnt, by the explosion. They thought that when the ball, which appeared to be of the size of a large millstone, rose, it took the middle of the main-topmast, as the head of the mast above the hounds was not splintered. The ball came down from the N. E. and went to the S. W.

An Examination of Certain Phenomena in Electricity, published in Italy. By the Abbé Nollet, F. R. S., and translated from the French by Mr. Watson, F. R. S. N° 494, p. 368.

Electricity, after having excited every where the emulation of the ingenious, after having filled us with wonder by an infinite number of phenomena more singular and more admirable one than another, seems, within these few years, to have shewn itself equally surprizing, but more useful, in Italy, than it had done in England, France, Germany, &c. where, for these 20 or 25 years, so great a progress had been made. We have heard of nothing less than the cure, or the almost sudden relief, of distempers of every kind, and of purging all sorts of persons in a manner of all others the most proper to avoid the repugnance and disgust we naturally have to medical potions. Even that disease which we are most desirous of concealing, was not by these means without its remedy; the mercury being volatilized, and carried, by the electric matter, into the body of the patient, tinged his skin of a leaden colour, and procured him a certain cure by a copious salivation.

The manner in which this was done was not less to be wondered at than the thing itself; persons afflicted with inveterate gouts, rheumatisms, fluxions, tumours, &c. were relieved by being electrized for a few hours, and often a less time was sufficient. Sometimes the rubbing a glass tube only, or at other times a glass tube lined with some medicine appropriated to the disease of the patient, was employed. These medicines, to exert their operation on the patient, passed through the glass; and this they were very certain of, as they saw them sensibly diminish in their quantity, though the glass containing them was stopped as close as though sealed hermetically. To promote stools, it is only necessary that a person should be electrized for 6 or 8 minutes, holding in his hand a piece of scammony or gamboge; the effects were as certain, as though these drugs were taken internally. Besides, if a person was desirous of being perfumed from head to foot, nothing more was necessary than being electrized with a glass vessel lined with balsam of Peru, benjamin, or some such drug; and from this electrization the odours were perceptible for 2 or 3 days, even so much as to incommode those to whom these smells were disagreeable.

Effects no less wonderful than these were published every day, by writings printed, and printed again, or by particular letters and memoirs in manuscript addressed to the ingenious all over Europe. They were also confirmed by respectable witnesses, and by such as were capable of imposing them on persons the most guarded against the exaggerations, which never fail accompanying the relations of interesting novelties.

The importance of the facts themselves, and the appearance of authenticity which attended them, demanded that they should be considered; and indeed they roused every where the attention of those philosophers, who had for any time turned their thoughts to these inquiries. Every one of them was desirous of repeating what Mr. Pivati said had been done at Venice, Mr. Verati at Bologna, and Mr. Bianchi at Turin; and to begin them, as the experiment seemed more simple, they attempted at first the transmission of odoriferous substances through the pores of the glass, the first foundation of intonacatores, so called by Mr. Pivati; and which we shall, in the progress of this paper, call medicated glasses; and they endeavoured to purge persons of all ages, and of both sexes, by making them hold in their hand, while they were electrized, scammony, gamboge, aloes, and such like. But it was very extraordinary, that of all the persons who were engaged in these experiments, no one could succeed; and, from a sort of shame, each of them expected, that some one would complain of his want of success: but this was retarded, as yet, by the haste with which Mr. Winkler sent to the Royal Society, and to some ingenious men in France, the result of his own experiments, which well agreed with those of Italy, and on the credit of which he had made them.

Mr. N. declares he will speak without any restraint: when he found his attempts were fruitless, he communicated it to all the philosophers, with whom he corresponded: he desired them to let him know if they had been more successful than himself, and to acquaint him how they had proceeded, that he might conform himself to the same. He was much more willing to confess his inability, and to learn from others the method which must of necessity be observed, than to be deprived longer from seeing those phenomena which ought to result from it. Instead of instructions, which might conduct him to the desired success, he received nothing but such confessions as his own: from these he saw that all methods had been tried; and that nothing remained to be done, but either to believe every thing on the faith of others, or to doubt, without hopes of being better informed. From this moment he formed the project of travelling; and, among the different motives which made him undertake the journey to Italy, one of the most pressing was, the desire of seeing succeed, in the hands of those who had said they had, those phenomena in electricity, towards the verification of which he had made so many fruitless efforts. He formed to himself a great pleasure in seeing balsam of Peru, benjamin, camphire, cinnamon, &c. pervade an electrized glass, which he had taken care to stop himself; to see people purged by the palm of their hands; to see an old gouty man, as the bishop of Sebenico, clap his hands together, strike the ground with his feet, and walk freely, after an electrization of 2 minutes: but what still more piqued his curiosity was, to learn, if possible, why the Italian electricity should enjoy these prerogatives, to the exclusion of that of every other country.

Abbé N. arrived at Turin about the beginning of May 1749: and one of his first cares was to visit Mr. Bianchi, a celebrated anatomist, and the first author of purging by electricity. And he begged of him, that all the experiments, which had neither succeeded with the Abbé, nor a great many others, might be repeated between them. Mr. B.'s complaisance easily granted what was desired: they set about it; and Pere Garo, a minim, and professor of philosophy in the University, caused to be carried to the place, where they determined to make the experiments, his electrifying machine.

May 21, about 4 in the afternoon, the weather cool, but uncertain, Mr. Bianchi having procured a lump of scammony, and another of gamboge, each of which was about the size of a hen's egg; Mr. N. took the former in his right hand, and having applied his left near the surface of the glass globe, and standing on a cake of resin, he was electrized 15 minutes without interruption. This day the electricity was indifferently strong.

After him, a young man, aged 22, of a pale complexion, was electrized; whom a few days before Mr. N. had taken into his service. They then electrized a young woman of about 16 or 17, of a weakly constitution; but who at that time was tolerably well. After that M. Beccari, professor of philosophy in the University, aged about 35, of a dry habit, was electrized. They then electrized a servant belonging to the house where the experiments were made, aged about 24, who did not appear to be indisposed. They also made the same experiment on another servant, a strong man of about 40; and each of these persons was electrized the same time, viz. 15 minutes successively.

Mr. N. did not perceive in himself any effect, which he could attribute to the electricity; no extraordinary motion or pain in his bowels; and it was the same with M. Beccari, with the servant aged 40, and with the young woman. But the young man of 22, being interrogated after the others, said, that he had had in the night 2 stools, and some complaints of the colic. The servant of the house, who was asked the same questions, declared, that he had had a very large stool, as though he had taken a purge.

These last 2 depositions were, as the others, taken on the spot; and Mr. N. began to consider them as important, when he learned, from the confession of the last, that he had taken, for some days, a decoction of wild succory, for an indisposition which he had not spoke of till then. The young man who said he had had 2 stools, rendered his testimony more than suspicious, by certain singularities* which he was desirous of adding some hours after; and since that

* This young man made himself very happy in relating to every body, that he had been electrized; and that he had been purged by it, as though he had taken physic: and added, that an hour after his electrization, having had the curiosity of visiting his wife, to see what would be the consequence, he had communicated this eleetricity to her, and that she had been purged as well as himself.—Orig.

time he has conducted himself in such a manner, as to prevent Mr. N.'s having any confidence in what he said.

What has just been mentioned of these two servants, one of which kept Mr. N. ignorant some time of his having taken broth with succory; and the other having testified such a love for the marvellous, that one ought in prudence to suspect every thing he said; this made him very delicate in the choice of the persons who should be admitted to the experiments. He declared he was not willing to receive to them either children, servants, or people of the lower class; but only that reasonable people should be admitted, and of an age sufficient to leave nothing to be feared of the truth of what they might depose.

The next day, Mr. N. was again electrized 15 minutes successively, as the day before, holding in his hand a large piece of scammony, and after him there went successively through the same trial, Dr. Scherra a physician, Mr. Verne, demonstrator of anatomy, the Marquis of Sirié, the Abbé Porta, a professor in the University, the preceptor to the children of the Marquis D'Ormea, and the preceptor to the young Messieurs D'Osa. This day the electricity was indifferently strong.

Of all these persons who were electrized, not one felt any pains in his belly, no one had any evacuation which could be attributed to the electrical power. Thus of 7 persons there was not one who suspected the operation of electricity to have had any sensible effect upon him.

May 23, the electricity being stronger than the preceding days, we chose a piece of new scammony, very strong in its flavour, and which weighed 4 ounces: The Marquis D'Ormea, Dr. Allion, a physician, the 2 above-mentioned preceptors, Pere Garo, Count Ferrero, and Mr. N. held, one after the other, this piece of scammony, and each was electrized 15 minutes, as had been done in the former experiments. After which, 2 days passed, and absolutely none of these persons perceived any thing, that could be attributed to the electricity.

The same day they repeated an experiment, which M. Bianchi had written of some months before, and which had not succeeded with Mr. N. at Paris. This experiment was the transmission of odours along a chain, or an iron bar electrized. They prepared and applied a little piece of linen, covered with balsam of Peru, on the iron bar, which received the electricity from the globe: they fastened to this rod the end of an iron chain, which was electrized by communication; and they expected that the odour of the balsam would be transmitted to the other end of the chain, to which was hung a ball of metal. But this was expected in vain; nobody could perceive the slightest sign of this transmission.

M. Bianchi, seeing that the result of all these experiments did not agree with those which he had believed to have taken place before, said, that this difference might arise from having now employed an electricity too strong: because that which he had experienced with success had always appeared more weak. Mr. N. submitted to this reason, having no other to give him more plausible; and to bring the whole operation as near as might be to its first circumstances, they met together, to the number of 14, at M. Bianchi's, and were electrized, one after the other, by him, as long a time as he judged proper, sometimes with scammony, and sometimes with gamboge, which he himself had chosen.

The machine used this day was the same, with which M. Bianchi had always made his own experiments. It consisted of a hollow glass cylinder, 3 inches in diameter, and something more than half a foot in length, mounted between 2 supporters on a board, which was fastened to a table with screws. This cylindrical vessel was turned round, without any other intermediate apparatus, by a handle, which was at least 4 inches in its radius; so that the hand, by which this machine was turned, revolved with greater velocity than the surface of the glass cylinder, which was put in motion by it.

This machine had this convenience, that one person only might turn the handle with one hand, and rub the surface of the glass vessel with his other: but there is no difficulty in comprehending, that the electricity could not but be always very weak with such a cylinder, and from such friction; so that, in the experiments of this day, they were scarcely able to perceive any snaps, in touching the iron chain, by which the electricity was communicated, or from the person electrized; but this was precisely what was desired.

These experiments were made on Thursday May 29, between 4 and 6 in the afternoon, in a very hot and serene day: on Sunday evening, all the persons who had been electrized, being interrogated, answered without hesitation, and in a manner absolute in all respects, that they had perceived nothing which could be attributed to these experiments: these persons were the Marquis de Siria, Count Ferrero, the Marquis D'Ormea, Mons. de Tignola, an officer of artillery, Pere Beccari, Pere Garo, Dr. Allion, Monsieur Verne, Dr. Scherra, the Abbé Porta, the two preceptors, the young woman, whom Mr. N. mentioned before, and himself.

The night following, viz. that between Sunday and Monday, Mr. N. was troubled with an indigestion, and felt pains of the colic; but he attributed them much less to the being electrized the preceding Thursday, than to some roots he had eaten the day before at dinner, and to a very large glass of iced lemonade, which he had drank some time after, and contrary to his usual custom. Yet as some persons were desirous of saying that the electrizing had purged him, and that he had not the candour to speak of it, he thought it his duty to add here for his justification, that during his whole life he has had a weak stomach; that he could never take ice, nor liquors very cold, without a good deal of circumspection, and always at the hazard of being incommoded; and that these roots, which are called ravanelle in Piedmont, notwithstanding his attention to eat sparingly of them, had often disturbed his digestion, during his stay there, and at times when he had no concern in electrical experiments. Besides, the not being incommoded for 3 days, and more, was sufficient to prevent attributing what happened to him to the electrical power.

The extreme circumspection, with which he was desirous of choosing the persons for all the experiments; the difficulty of procuring and moving such sick people, as were in a condition and disposition to leave nothing to be feared on their parts from their prejudice, and their heated imagination; that of reconciling his time with that which a physician of great practice could grant him; these obstacles prevented his attempting with M. Bianchi such cures, as he believed to have been brought about by means of the electric virtue, either by its own action, or by joining medicines appropriated to the condition of the sick, and contained in glass vessels electrized by friction. But Mr. N. testified a great desire of seeing those persons who had been cured, or considerably relieved, by this method before this time. He asked, for this purpose, the gentlemen of the profession, who had been witnesses of the experiments, and who were yet in a condition of seeing every day some of the persons, cited in a manuscript which he had of M. Bianchi's; and of whom the exact history is mentioned in the 9th chapter of a Treatise of M. Pivati: Mr. N. went himself to the shoemaker, in whose shop the young man of 21 years of age worked, mentioned in the 110th page of the above treatise. The obligation of saying the truth, to which philosophers ought to sacrifice every human regard, would not permit him to dissemble that his inquiries made with all possible diligence, and without any other interest than that of knowing the truth, have made him see sufficiently clear, that these facts have been greatly exaggerated. He was willing to believe, that it is the fault of the sick, who, being prejudiced perhaps by too great hope, and possessed by a kind of enthusiasm, have said and made others believe, more than really was the case. One might have examples enough to cite of such illusions; but be that as it will, he cannot help believing, that a great part of the electrical cures of Turin, have been no other than temporary shadows, which have been taken with a little too much precipitation, or complaisance, for realities.

Mr. N. carried with him to Venice the same curiosity, and the same desire of being instructed, on the subject of the transmission of odours from medicated tubes, and of the cures, or of being relieved from disorders almost suddenly, by the electrical power. One of his first cares was, to find out some friends or acquaintance of M. Pivati, to acquaint him of his arrival, and to obtain of him the favour of being admitted into his laboratory; and that he would have the complaisance to satisfy his great desire of seeing him cause odours to pervade the sides of a well stopped glass, or by electrifying to diminish sensibly any substance therein contained. Mr. Angelo Quirini, a Venetian gentleman, a great friend to the sciences, accordingly acquainted M. Pivati; and on the 1st of August, 1749, we waited on him, and found there a large company, among which were several persons of distinction: among others were Mr. Antony Mossinigo, heretofore embassador in France, Abbé Horter, &c. At the sight of this great assembly he believed that his curiosity had been suspected of disbelief, and of an obstinacy to doubt; this company therefore was called together to be an evidence of his conviction. But how great were his surprize and regret, when M. Pivati declared in the presence of this whole company, that he would not attempt to show Mr. N. the transmission of odours; that that phenomenon had not succeeded but once or twice, as he had said in his first letter printed at Lucca, though since that he had made many attempts to repeat that experiment, with the same as well as with other glasses; that this cylinder had been since broken; and that he had not so much as kept the fragments of it!

But at least, Mr. N. said he wished to see him use one of his medicated tubes, and weigh it before and after electrifying, to see the included matter diminish sensibly. This fact Mr. P. then said had succeeded with him a great many times; but that now there was too much company; that it was too hot, and in consequence that the electricity would be too weak for it. He might perhaps be in the right: but why did he call together so numerous a company? Mr. N. then asked him concerning the cures related in his works, and especially concerning that of the bishop of Sebenico. He acknowledged, (and in part Mr. N. knew it already,) that the prelate was not cured; and that since the electrification he had been as he was before.

Mr. N. took his leave of M. Pivati, and acquainted him, that he proposed to continue about a week in Venice; and he very earnestly begged of him to collect together his best vessels, to renew the substances in them, and to let him know, that if they succeeded, he might wait upon him, that he might be able to publish them as an eye-witness, &c. M. Pivati promised him he would; but, as Mr. N. heard nothing from him afterwards, he presumed that he had nothing to show him.

Mr. Nollet then relates a number of other trials that he had seen, and that had been told him by different persons, in several other parts of Italy, much to the same purport as the foregoing: from all which he draws a conclusion to the following purport. That he learned nothing in the other cities of Italy, which did not strengthen his doubts in regard to those electrical phenomena, which he had a desire to verify in the course of his travels. Pere la Torre, professor of philosophy at Naples; M. de la Garde, director of the coinage at Florence, one who has been much engaged in these inquiries; M. Guadagni, professor of experimental philosophy at Pisa; the Marquis Maffei, at Verona; Dr. Cornelio, at Placentia; Pere Garo, at Turin; all these, with very excellent and well contrived machines, and with a great desire of succeeding, have attempted many times to transmit the odours, as well as the powers of drugs closed (carefully) in tubes or spheres of glass, by electrizing them: all these have attempted to purge a number of persons; and, according to the accounts they gave, have never gained their point; or the little success they had, appeared too equivocal to draw any consequences conformable to those M. Pivati had believed to have seen in his experiments.

I am now then, says Abbé Nollet, as it were, certain of what I began to believe last year, when I printed my Treatise on Electricity, that M. Pivati has been deceived by some circumstance to which he had not given sufficient attention; and what makes me believe it more than ever is, that he assured me himself, that this transfusion of odours, and of drugs, through electrized glass vessels, had never manifested itself to him but once or twice directly; I mean by a sensible diminution of bulk, and by such emanations as the smell was capable of perceiving. It is however on this pretended transmission, and with a glass tube, which was cracked from one end to the other, as M. Pivati tells you himself, on this fact, than which nothing can be less certain, that they have established the use and effects of lined tubes, of which they are willing to abate nothing. I am disposed to believe, that the electricity may have cured or relieved distempered persons; but I do not find the proofs of M. Pivati sufficiently strong, or sufficiently certain, to make me conceive that the lined glasses have contributed to these good effects. I think, and M. Verati himself appeared to me pretty much of the same opinion, that if any one has been so happy as to cure distempers by electrifying with glasses containing drugs, all that can be said in favour of these substances is, that they have not hindered the operation of electricity.

It remains to say, that in these researches I have coveted truth, only for her own sake; and have no interest in convincing those who may think proper still obstinately to believe, what has been published concerning lined tubes, electrical purgations, instantaneous cures, &c. I do not pretend to make any of my opinion, but those who, having read without prejudice what I have here related, may find themselves touched by my reasons: but if after this there can be any one, on whom the love of the marvellous can make a victorious impression, I shall not think ill of them, if they embrace opinions opposite to mine; qui vult decipi, decipiatur.

An Extraordinary Case of a Fracture in the Arm. By Mr. John Barde, Surgeon in New York. Communicated by Mr. John Freke, F. R. S., Surgeon to St. Bartholomew's Hospital. N° 494, p. 397.

This fracture occurred to a lady about the 3d month of her pregnancy, in consequence of which the bones did not unite until 9 days after her delivery. From that time, in less than a month, the callus was entirely confirmed, and the patient recovered the use of her arm. This case, (adds the author,) is similar to 2 cases related by Hildanus, in which the formation of a callus, was retarded by pregnancy.

A further Account of the Libellæ or May-flies, from Mr. John Bartram of Pennsylvania, communicated by Mr. Peter Collinson, F. R. S.* N° 494, p. 400.

The May-flies of America have no very remarkable difference from ours; excepting a few days in the fly state, they live all the year a water insect. Their bodies being replenished with an oily matter, they easily quit their husks, and rise up to the surface of the water, and disperse themselves a mile or more back in the woods, while others stay near the water.

May the 4th 1749, Mr. C. perceived many had attained wings, and were very thick spread on the bushes and grass, by the river sides. The second day after their leaving their aquatic abode they cast another skin, after which their tails are longer, and their wings drier, and more transparent. The 5th and 6th was rainy, the 7th windy; so very few came out. The 8th was cool; so few were seen: but the 9th and 10th, being warm, many swarmed late in the evening; and the 11th, 12th, 13th, they swarmed abundantly. What he calls swarming, was their gathering thick as bees, near the rivers, to lay their eggs in the water.

In their flight they mount to the tops of trees, 20 or 30 feet high: their motion is surprizing, hovering up and down, rising and falling, 7 or 8 feet at a time: this he takes to be the time and manner of their impregnation. After which they fly to the brooks, cast out their eggs, and perish immediately: their eggs sink directly to the bottom, and lodge among the mud and gravel, and may be food for some minute water animal. From their eggs proceeds a deformed grub, which subsists under water, and is food for eels, till next season, that it attains its fly state, and then is food for fish and fowl. It is remarkable, the males are black, and live several days after the females.

The reason of their being so long in coming forth this year was, the cold chilly weather: other years, in a warm season, in five days they would have performed all their functions, and disappeared.

We have two other smaller kinds, which very much resemble the former.

* See page 290 of volume ix.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the Worshipful Company of Apothecaries for the Year 1749, *Pursuant to the Direction of Sir Hans Sloane, Bart. &c.* N° 495, p. 403.

[This is the 28th presentation of this kind, completing to the number of 1400 different plants.]

The Case of a Young Lady who had an Extraordinary Impostume formed in her Stomach. By Daniel Peter Layard, M. D., F. R. S. N° 495, p. 406.

A young lady of 17 being at a boarding school about 3 miles from this city, was, on the 28th of November 1745, taken with profuse sweats, which, after some continuance, and weakening her much, were stopped by means of saline draughts, made with elixir vitrioli. On the removal of those sweats, an obstruction of the menses, with all its symptoms, ensued. A shortness of breath, a dry cough, an acute pain in the left hypochondrium, rigors, &c. were taken for the signs of a peripneumonia; and the medicines usually prescribed having no effect, a blister was applied on the left hypochondrium. The fœtids, and musk, as in a nervous case, were also administered in large quantities, but with as little success.

It being thought adviseable to bring the young lady to town, Dr. L. first saw her on the 12th of Feb. 1745-6, when he observed a large prominent tumor on the left hypochondrium, which reached to part of the right, filling up the epigastrium and scrobiculus cordis, where she complained of a constant acute pain. The muscles of the larynx, pharynx, and neck, were much swelled, and the glands indurated. The other symptoms were a continual quick pulse, thirst, hoarse cough, difficulty of breathing, cardialgia, and obstruction in the œsophagus, so that, as soon as any liquid "fell down," as she expressed it, "to the pit of her stomach," she instantly threw it up with violent pain, borborigmi, eructations, and singultus.

On the 14th, finding the symptoms increase, especially the obstruction in the œsophagus, and apprehending that an abscess was forming in the stomach, he desired Dr. Mead should be called in, who confirmed Dr. L.'s opinion. In order to assuage the inflammation, a cooling mucilaginous mixture, &c. were prescribed, as also a laxative glyster. Next day being told that not a drop of the mixtures could be admitted into the stomach, Dr. Mead took his leave, advising the repetition of the glyster every 3 or 4 days, as necessity might require, and that nature should be watched, in case of a favourable turn, which he did not much expect, having observed, that those abscesses more frequently terminate in a gangrene than by suppuration.

On the 16th the glyster brought away with the fæces some pieces of membranes, about a finger's length, and 2 in breadth; to lubricate the intestines Dr. L. directed 10 oz. of plain mutton broth to be injected, which, after the first time, was constantly absorbed, and was repeated twice every day, till the 3d of May.

In this state, with no other sustenance than these broth-glysters, the laxative one repeated every third day, and a warm carminative plaister applied on the tumor, did the patient continue till the 17th of March, when, observing an intermission in the pulse, and hoping that a decoction of the cort. Peruv. might corroborate the solids, if absorbed, as the broth glysters were, he directed 8 oz. of the decoction to be injected, and repeated 4 hours after: by accident, the first was not half thrown up; the 2nd on the 18th of March, at 2 o'clock in the morning, had a very extraordinary effect; for being entirely absorbed, about 2 hours after, the young lady complained of a most acute pain in her stomach, which by its violence brought on a profuse sweat, and threw her into a syncope, in which she remained a full quarter of an hour; then, shrieking, made signs to her nurse to bring her the basin; she vomited near 2lb. of grumous blood, and then some purulent matter; then she discharged by stool above 4 quarts of well digested pus, with several pieces of membranes, like those before-mentioned. The purulent discharge continued gradually decreasing till the 23d of April. Balsamics, and small quantities of thin veal and mutton-broth, were daily given. April the 29th the patient was purged with pulp of cassia. May the 3d the mutton-broth glysters were omitted, the stomach now performing its office. The 7th 10 oz. of blood were taken from the foot, which brought down the menses. The tumefaction and induration of the muscles and glands of the neck were removed by the continual application of the emplastr. saponac. And after the use of stomachics, and mineral waters, the young lady was perfectly cured on the 17th of June 1746, and continued well ever after.

It may not be improper to observe, that the stomach, on account of the number of blood-vessels it is furnished with, is as liable to inflammations, and abscesses, as any part of the human body. These are occasioned by a stagnation of the blood, which, if not speedily removed, must greatly endanger the patient's life, by obstructing the necessary vital functions of that viscus. The speedy progress of this disease, and the remoteness of the part from proper applications, render its termination mostly fatal; and these abscesses more frequently terminate by a gangrene, than come to suppuration. Those that do suppurate, generally form ulcers, penetrating into the cavity of the abdomen, and sometimes also perforate the integuments.

Luetus (De His Qui Vivunt Sine Alimento) relates several instances of persons who have lived a long while without food, but then they could admit of liquids; and the young lady which Mons. Littre (Mem de l'Academ. des Sciences. Ann.

1716,) nourished with broth-glysters, in which a yolk or 2 of eggs, and sometimes a glass of wine, were mixed, could also keep water in her stomach, though no other fluid. An advantage which this patient was deprived of.

From what has been related it appears, that this young lady had an abscess in her stomach, which gradually ripened, and then broke, suppurated, digested, and cicatrized, as all other abscesses do; and that during this time, which was near 3 months, she was almost all the while nourished solely by the mutton-broth glysters.

Account of an Irregular Tide in the River of Forth. By Mr. Edward Wright. N° 495, p. 412.

There are in this river, at ebbing and flowing, certain irregular motions, not to be found in any other river in Scotland, perhaps in Great Britain, or even in all Europe, called by the common people leakies; which means that when the river is flowing, before high water, it intermits and ebbs for a considerable time, after which it resumes its former course, and flows till high water; and, vice versa, in the ebbing, before low water, the river flows again for some time, and then ebbs till low water. The leaky begins at a place called Queen's Ferry, 7 miles above Leith, at neap tide, and low water, and goes to the house of Maner, which is about 25 miles above Queen's Ferry, that is, going by water; though it be but 4 miles by land. This is noticed as he takes these windings to be the cause of the leakies. At neap tide and high water, as also at spring tide and low water, the leaky reaches as far as the sea fills, which is to the groves of Craigforth, 19 miles above Maner house, and 3 above the town of Stirling. At Queen's Ferry there are no leakies at neaps and springs at high water, nor in the latter at low water; they begin between Borrowstowness, a village about 7 miles above Queen's Ferry, and the mouth of a rivulet called Carron, 5 or 6 miles farther up the river than Borrowstowness. It is very remarkable, that in the very lowest neaps, the leaky, after it has ebbed for some time, before high water, makes up again, and will be 2 feet higher than the main tide. In the beginning of the spring tides, it does not rise so high by a foot: at the dying of the stream, it is often 2 feet higher than the main tide, which is to be understood, before high water, when the leaky makes up again. At neap tide and low water it will ebb 2 hours, and fill as much, and at full water ebb an hour, and fill another.

It is observable, that at full moon, there are no leakies, either at high or low water, in the spring tides which are at that time, but in the neaps which follow them, these motions are observable, as before described; as also in the spring tides, which happen on the change of the moon, there are leakies both at high and low water. All this is to be understood, when the weather is seasonable; for, otherwise, these motions are not so discernible.

Case of a Tumour growing on the Inside of the Bladder, successfully extirpated by Joseph Warner, Surgeon to Guy's Hospital. N° 495, p. 414.

Mary Bevan, aged 23, June 24, 1747, strained herself by endeavouring to lift a great weight; she was immediately seized with violent pain in the small of her back, and a total suppression of urine; which symptoms, notwithstanding the several methods used for her relief, continued till the 29th of the same month; when an eminent physician and man-midwife was called to her assistance; who drew off her urine with the catheter. During the suppression she was seized with an acute fever, and for 18 or 20 hours before her urine was drawn off, she discharged by the mouth a great quantity of saltish water tinged with blood; which, on lying down, flowed in so great quantities as to threaten suffocation.

In April 1750, she applied to Mr. W. On inquiry he learnt she had never been able, from the moment of the accident, to void a drop of urine without the assistance of the catheter, which had been used ever since 2 or 3 times every 24 hours; that she was in continual pain, and had been lately much weakened by having several times lost considerable quantities of blood, occasioned by the force used for the introduction of that instrument. On examining her with his fore-finger, which he introduced with great difficulty through the meatus urinarius, he discovered a considerable tumour, which seemed to be of a fleshy substance, and took its rise from the lower part of the bladder near its neck; the extent of which he could with difficulty reach. She informed him, she first discovered this swelling about 20 months before. He observed it to protrude a little way out of the meatus urinarius on straining to make water when the bladder was full; but on ceasing to strain it presently returned. It had preserved pretty nearly the same appearance ever since it was first noticed; and about 18 months before a small incision was made into it, on presumption of its containing a fluid, but without any effect.

The method he took for the extirpation of the tumour was this: having first prepared the patient as before the operation for the stone; when her bladder was full, he made her strain as though she was going to make water, on which he perceived the tumour to protrude a little; this he effectually secured from returning into the bladder by the help of a crooked needle and ligature passed through the tumour in different directions, and endeavoured to draw it out through the meatus urinarius, but could not effect it by reason of its size; on this he dilated the meatus urinarius on the right side by cutting it upwards about half way towards the neck, when by pulling the tumour forwards, he had sufficient room for tying a ligature round its basis, which was very broad.

For the first 3 days after the operation, she complained of a good deal of pain in the abdomen. On the 6th day the tumour dropped off. From the first of the operation, she voided her urine without any assistance, and was afterwards perfectly well in every respect.

*Remarks on the Solar and the Lunar Years, the Cycle of 19 Years, commonly called the Golden Number, the Epact, and a Method of finding the Time of Easter, as it is now observed in most Parts of Europe. Being Part of a Letter from the Right Honourable George Earl of Macclesfield.** N° 494, p. 417.

Of the Solar Year.—The mean tropical solar year, or that mean space of time wherein the sun, or earth, after departing from any point of the ecliptic, returns to the same again, consists, according to Dr. Halley's tables, of $365^d\ 5^h\ 48^m\ 55^s$: which is $11^m\ 5^s$ less than the mean Julian year, consisting of $365^d\ 6^h$. Hence the equinoxes and solstices anticipate, or come earlier than the Julian account supposes them to do, by $11^m\ 5^s$, in each mean Julian year; or $44^m\ 20^s$ in every 4; or $3^d\ 1^h\ 53^m\ 20^s$ in every 400 Julian years. To correct this error in the Julian year, the authors of the Gregorian method of regulating the year, when they reformed the calendar in the beginning of Oct. 15, 1582, directed that 3 intercalary days should be omitted or dropped in every 400 years; by reckoning all those years whose date consists of a number of entire hundreds not divisible by 4, such as 1700, 1800, 1900, 2100, &c. to be only common, and not bissextile or leap years, as they would otherwise have been; and consequently omitting the intercalary days, which, according to the Julian account, should have been inserted in the month of February in those years. But at the same time they ordered that every 400th year, consisting of a number of entire hundreds, divisible by 4, such as 1600, 2000, 2400, 2800, &c. should still be considered as bissextile or leap years, and of consequence that one day should be intercalated as usual in those years. This correction however did not entirely remove the error: for the equinoxes and solstices still anticipate $1^h\ 53^m\ 20^s$ in every 400 Gregorian years. But that difference is so inconsiderable as not to amount to 24 hours, or to one whole day, in less than 5082 Gregorian years.

Of the Lunar Year, Cycle of 19 Years, and the Epact.—The space of time

* This nobleman had been a pupil of the celebrated Wm. Jones, Esq. vice-president of the Royal Society, who was father of the no less celebrated Sir Wm. Jones, chief judge in India. Earl M. was elected president of the Royal Society about the year 1751, on the resignation of Martin Folkes, Esq. His lordship was greatly instrumental in procuring the introduction of the New or Gregorian stile into use in this country, which took place in 1752. And he died in the year 1764.

between one mean conjunction of the moon with the sun and the next following, or a mean Synodical month, is equal to $29^d\ 12^h\ 44^m\ 3^s\ 2^{th}\ 56^f$, according to Mr. Pound's tables of mean conjunctions. The common lunar year consists of 12 such months. The intercalary or embolimæan year consists of 13 such months. In each cycle of 19 lunar years, there are 12 common, and 7 intercalary or embolimæan years, making together 235 synodical months.

It was thought, at the time of the general council of Nice, which was holden in the year of our Lord 325, that 19 Julian solar years were exactly equal to such a cycle of 19 lunar years, or to 235 synodical months; and therefore that at the end of 19 years, the new moons or conjunctions would happen exactly at the same times as they did 19 years before: and on this supposition it was, that some time afterwards, the several numbers of that cycle, commonly called the golden numbers, were prefixed to all those days in the calendar, on which the new moons then happened in the respective years corresponding to those numbers; it being imagined, that whenever any of those numbers should for the future be the golden number of the year, the new moons would invariably happen on those days in the several months, to which that number was prefixed.

But this was a mistake:

For 19 Julian solar years contain	6939^d	18^h	0^m	0^s	0^{th}
Whereas 235 synodical months contain only	6939	16	31	56	30
And are therefore less than 19 Julian solar years by	0	1	28	3	30

This difference amounts to a whole day very nearly in 310.7 years, the new moons anticipating, or falling earlier, by 24 hours in that space of time, than they did before: and therefore now in the year 1750, the new moons happen above $4\frac{1}{2}$ days sooner, than the times pointed out by the golden numbers in the calendar.

In order therefore to preserve a sort of regular correspondence between the solar and the lunar years, and to make the golden numbers, prefixed to the days of the month, useful for determining the times of the new moons, it would be necessary, when once those golden numbers should have been prefixed to the proper days, to make them anticipate a day at the end of every 310.7 years, as the moons will actually have done; that is to set them back one day, by prefixing each of them to the day preceding that against which they before stood.

But as such a rule would neither be so easily comprehended or retained in memory, as if the alteration was to be made at the end or at the beginning of complete centuries of years; the rule would be much more fit for practice, and keep sufficiently near to the truth, if those numbers should be set back 9 days in the space of 2800 years; by setting them back one day, first at the end of 400 years, and then at the end of every 300 years for 8 times successively: by which they would be set back, in the whole, 9 days in 2800 years. After which they must again be set one day back at the end of 400 years, and so on, as in the preceding 2800 years. By which means the golden numbers would always point out the mean times of the new moons, within a day of the truth.

It is plain however that the lunar year will have lost one day more than ordinary, with respect to the solar year, whenever the new moons shall have anticipated a whole day; as they will have done at those times, when it is necessary that the golden numbers should, by the rule just now given, be set back one day: and consequently the epact, for that and the succeeding years, must exceed by an unit the several corresponding epacts of the preceding 19 years.

For the epact is the difference, in whole days, between the common Julian solar and the lunar year; the former being reckoned to consist of 365, and the latter of only 354 days. If therefore the solar and the lunar year at any time should commence on the same day, the solar would, at the end of the year, have exceeded the lunar by 11 days; which number 11 would be the epaet of the next year: 22 would be the epact of the year following, and 33 the epact of the year after that, the epacts increasing yearly by 11. But as often as this yearly addition makes the epact exceed 30, those 30 are rejected as making an intercalary month, and only the excess of the epact above 30 is accounted the true epact for that year. Thus when the epact would amount to 31, 32, 33, 34, &c. the 30 is rejected, and the epact becomes 1, 2, 3, 4, &c.

Since therefore the lunar year will have lost a day more than ordinary, in respect of the solar year, whenever it is necessary to set the golden numbers one day back, as before observed; it follows, that the epact must at the same time be increased by a unit more than usual; the difference between the solar and the lunar year having been just so much greater than usual. That is, 12 must be added, instead of 11, to the epact of the preceding, in order to form what will be the epact of the then present year. Which addition of a unit extraordinary to one epact, will occasion all the subsequent epacts (which will follow each other in the usual manner, each exceeding the foregoing by 11) to be greater by 1, than their respectively corresponding epacts of the preceding 19 years.

If therefore, instead of the golden numbers, the epacts of the several years were prefixed, in the manner the Gregorians have done, to the days of the calendar, in order to denote the days on which the new moons fall in those years of which those numbers are the epacts; there would never be occasion to shift the places of those epacts in the calendar; since the augmentation by 1 extraordinary of the epacts themselves would answer the purpose, and keep all tolerably right. Thus in a very easy method may the course of the new moons be pointed out, either by the golden numbers, or by the epacts, according to the Julian account or manner of adjusting the year, which goes on regular and uniform without any variation.

But the regulating these things for those who use the Gregorian account, is an affair of more intricacy; and for them it will require more consideration to determine, when the epacts are to be more than ordinarily augmented, and at what times they are to continue in their usual course; nay, to know when they are not only not to be extraordinarily augmented, but also when they are to be diminished by a unit, by increasing one of them by 10 only instead of 11 as usual: and this happens much oftener with the Gregorians, than the increasing one of them by 12 instead of 11. For in every Gregorian solar year, whose date consists of any number of entire hundreds not divisible by 4, it is supposed that the equinox has anticipated one whole day; and therefore 1 day, that which ought to be the intercalary one, is omitted; and consequently the preceding solar year, where one day was lost, exceeded the lunar year by 10 days only, instead of 11.

In order therefore to adapt the before-mentioned rule to the Gregorian account, and to know in what years the epacts should either be extraordinarily augmented or diminished, and the golden numbers should either be set backwards or forwards in the calendar; the following rules and directions must be observed.

First, that in the years 1800, 2100, 2700, 3000, &c. where the number of entire hundreds is divisible by 3, but not by 4, the Gregorian solar, as well as the lunar year, will have lost a day; and consequently the difference between them will be the same as usual: therefore in those years there must be no alteration, either in the epacts or the golden numbers; but the former must go on in the same manner, and the latter stand prefixed to the same days in the calendar, for another, as they did for the last 100 years.

2dly. The like will happen in the years 2000, 2800, 3200, &c. where the number of entire hundreds is divisible by 4, but not by 3: for neither the Gregorian solar nor the lunar year is to be altered; and therefore the epacts must go on, and the golden numbers stand, as they did before.

But 3dly, in the years 2400, and 3600, whose number of entire hundreds is divisible both by 3 and 4, the Gregorian solar year goes on as usual, and the lunar year has lost a day. The difference therefore between them being 12, the epact of the preceding year must be augmented by that number instead of 11, in order to form the epact of the then present year; by which a new set of epacts will be introduced, exceeding their precedent corresponding epacts by 1: and the golden numbers must be set 1 day back in the calendar.

4thly and lastly, in the years 1900, 2200, 2300, 2500, &c. where the number of hundreds is divisible neither by 3 nor 4; the Gregorian solar year having lost one day, and the lunar none, the difference between them being only 10;

that number only, and not 11, is to be added to the epact of the preceding, to form the epact of that, the then present year; by which a new set of epacts will be introduced, all less by one than their precedent corresponding epacts: and the golden numbers must be set a day forwarder in the calendar; that is, be prefixed to the day following that against which they stood in the precedent hundred years.

This method would preserve a sort of regularity between the solar and the lunar years; and, by means of the rules and directions before mentioned, the days of the new moons might be pointed out, either by the golden numbers or by the epacts, placed in the calendar for that purpose; according to the Julian account for ever, and according to the Gregorian account till the year 4199 inclusive, after which there must be some little variation made in the 4 last precepts or rules; but it would be to little purpose now, to attempt the framing of a new set of rules for so distant a time.

The Gregorians have chosen to make use of the epacts to determine the days of the new moons, and follow pretty nearly the rules prescribed above; except that they order the epacts to have an additional augmentation of a unit 8 times in 2500 years, beginning with the year 1800, as at the end of 400 years; to which 400 years if there be added 3 times 700, or 2100 years, the period of 2500 years will be completed in the year 3900. After which they do not make their extraordinary augmentation of a unit in the epacts, till at the end of another term of 400 years; which defers that augmentation from the year 4200 to the year 4300. And this is the reason that the rules above delivered will require a variation in the year 4200; whereas it is directed in this paper that the epacts should be augmented, or (which is the same thing) the golden numbers be set back in the calendar 9 times in 2800 years. This arises from the Gregorians supposing, that the difference between 19 solar and as many lunar years, would not amount to a whole day in less than $312\frac{1}{2}$ years; whereas it has appeared above, that it would amount to a whole day in 310.7 years. But though the rule prescribed in this paper comes much nearer the truth, yet the error in either case is very inconsiderable, being so small as not to amount to a whole day in many thousand years; and therefore is not worth regarding.

Of finding Easter.—From what has been said, a method may be obtained for fixing, with sufficient exactness, the time of the celebration of the feast of Easter, which is governed by the vernal equinox, and by the age of the moon nearest to it. The former of which, when once rightly adjusted, may, (by the corrections mentioned in that part of this paper which relates to the solar year) be made to continue to fall at very near the same time with, or at most not to differ a whole day from the true equinox: and the same rules and directions which, as before shown, would without any great error, point out the times of the first day of the moon, would with equal certainty point out the 14th, 15th, or any other: and thus the times of the oppositions, or the full moons, might be as well marked out, as those of the conjunctions or the new moons.

The method now used in England, for finding the 14th day of the moon, or the ecclesiastical full moon, on which Easter depends, is by process of time become considerably erroneous: as the golden numbers, which were placed in the calendar to point out the days on which the new moons fall in those years of which they are respectively the golden numbers, now stand several days later in the same than those new moons really happen. Which error, as before observed, arises from the anticipation of the moons since the time of the council of Nice: and as the vernal equinox has also anticipated 11 days since that time; neither that equinox, nor the new moons, now happen on those days on which the church of England supposes them so to happen.

When pope Gregory the 13th reformed the Julian solar year, he also made a correction as to the time of celebrating the feast of Easter, by placing the epacts (which he directed to be used for the future instead of the golden numbers) much nearer to the true times of the new moons, than the golden numbers then stood in the old calendar: he says, much nearer to the true times; because in fact the epacts, as placed by him, were not prefixed to the exact days on which the new moons then truly fell. And this was done with design, and for a reason which it is not material to the purpose of this paper to mention.

But the church of England, and that of Rome or the Gregorians, still agree in this; that both of them mark (the former by the golden numbers, and the latter by the epacts corresponding to them) the days on which their ecclesiastical new moons are supposed to happen: and that 14th day of the moon inclusive, or that full moon, which falls upon, or next after, the 21st day of March, is the Paschal limit or full moon to both: and the Sunday next following that limit, or full moon, is by both churches celebrated as Easter-day. But the 21st of March being reckoned, according to the Gregorian account or the new style, 11 days sooner than by the Julian account or the old style, which is still in use among us; and their ecclesiastical new moons being 3 days earlier than those of the church of England; it happens that though the church of England and that of Rome often do, yet more frequently they do not, celebrate the feast of Easter on the same natural day.

It might however be easier for both, and could occasion no inconvenience, now that almanacs, which tell the exact times of the new moons, are in most people's hands; if all the golden numbers and epacts now prefixed to those days of the calendar, in our book of Common Prayer, and in the Roman breviary, on which the respective ecclesiastical new moons happen, were omitted in the places where they now stand; and were set only against those 14th days of the moon, or those full moons, which happen between the 21st day of March and the 18th of April, both inclusive. Since no 14th day or full moon, which happens before the 21st of March, or after the 18th day of April, can have any share in fixing the time of Easter. By which means the trouble of counting to the 14th day, and the mistakes which sometimes arise from it, would be avoided. We do as yet in England follow the Julian account or the old style in the civil year; as also the old method of finding those moons on which Easter depends, both of which have been shown to be very erroneous.

If therefore this nation should ever judge it proper to correct the civil year, and to make it conformable to that of the Gregorians, it would surely be adviseable to correct the time of the celebration of the feast of Easter also, and to bring it to the same day on which it is kept and solemnized by the inhabitants of the greatest part of Europe, that is, by those who follow the Gregorian account. For though their method of finding the time of Easter is not quite exact, but is liable to some errors: yet all other practicable methods of doing it would be so too; and if they were more free from error, they would probably be more intricate, and harder to be understood by numbers of people, than the method of determining that feast either by a cycle of epacts, as is practised by the Gregorians, or by that of 19 years or the golden numbers, in the manner proposed in the following part of this paper: and it is of no small importance, that a matter of so general a concern, as the method of finding Easter is, should be within the reach of the generality of mankind, at least as far as the nature of the thing will admit.

For which reason, in case the legislature of this country should, before the year 1900, think fit to make our civil year correspond with that of the Gregorians, and also to celebrate all the future feasts of Easter on the same days on which they celebrate them; this last particular might be easily effected, without altering the rule of the church of England for the finding of that feast; and this only by advancing the golden numbers, prefixed to certain days in the calendar, 8 days forwarder for the new moons, or 21 days forwarder for the 14th days or full moons, than they now stand in our calendar.

In order to explain this, it must be observed, that the Gregorian account, or the new style, is 11 days forwarder than the Julian account or the old style, which we still make use of; that is, the last day of any of our months, is the 11th day of their next succeeding month. If therefore their ecclesiastical new moons fell on the same days with those of the church of England, the golden number 14, which now stands against the last day of February in our, that is, the Julian calendar, should, when we should have adopted the Gregorian calendar, be prefixed to the 11th day of March. But since their ecclesiastical new moons happen 3 days earlier than our ecclesiastical new moons at present do; so much should be deducted from those 11 days, by which the golden numbers ought otherwise to be advanced; and the golden number 14 should not be placed against the 11th, but the 8th day of March; which being reckoned the first day of the moon, if we count on to the 14th day of the same inclusive, that would be found to fall on the 21st day of March; on which day the Gregorian paschal limit or full moon will happen, when the golden number is 14. And the like course should be taken with the rest of the 19 golden numbers; which ought to be placed 8 days forwarder than they now stand, if they are to point out the new moon; or 21 days forwarder than they are at present, if they are to mark the 14th day of the moon, or the full moon; the latter of which, as has been shown, would be more eligible, than to prefix those numbers to the days on which the new moons happen.

Thus may the rule and method now used in the church of England, be most easily adapted to show the time of Easter, as it is observed by the Gregorians, till the year 1900; at which time, and at the other proper succeeding times, if the golden numbers in the calendar shall either be advanced or set backward a day, according to the foregoing rules and directions for that purpose, they will continue to show the new or the full moons of the church of Rome, or the Gregorian calendar, with great exactness, till the year 4199: when, as has been already mentioned, there must be a little variation made in those rules and directions. There is however one exception to those general rules and directions, which will be taken notice of in the next paragraph.

On these principles is framed the table accompanying this paper, and showing, by means of the golden numbers, all the Gregorian paschal limits or full moons, from the reformation of the calendar, &c. by pope Gregory, to the year 4199 inclusive. Which space of time is there divided into 16 unequal portions or periods; at the beginning of each of which, all the golden numbers, when once they shall have been properly placed in the calendar, must either be advanced or set back 1 day, with respect to the place where they stood in the preceding period, agreeably to the foregoing rules; except those numbers which shall happen to stand against the 4th and 5th of April, to show the paschal new moons, or against the 17th or 18th of the same month to mark out the paschal full moons; both which numbers at some times, and only one of them at others, must keep the same place for that which was allotted to them in the immediately preceding period.

In order to determine at what times, and on what occasions, this exception is to take place; let it be observed, that in the months of January, March, May, and some others in our present calendar, as well as in the table above-mentioned, some of the golden numbers stand double or in pairs, and follow one the other immediately: while others, on the contrary, generally stand single and by themselves. Now, when any of those pairs, or two numbers which usually accompany each other, happen, in pursuance of the foregoing rules, to be prefixed the one to the 4th and the other to the 5th of April for the new moons, or the one to the 17th and the other to the 18th of April for the paschal limits or full moons; and when any of those numbers, which generally stand single, are prefixed, according to the said rules, to the 5th of April for the new moons, or to the 18th for the full moons; in these cases those pairs or single numbers that are so situated, must not be set forward, or advanced at the beginning of the next period, but must keep their places during another period, if the foregoing rules direct all the golden numbers to be advanced a day; which must be complied with in respect to all the other golden numbers, except those so situated as above. Instances of which may be seen in the table, under the respective periods beginning with the years 1900, 2600, 3100, and 3800. But if, in conformity to the foregoing rules, all the golden numbers are to be set one day backward, those pairs or single numbers, though situated as above-mentioned, must not keep their places, but must move one day backward, like all the other golden numbers; as they may be seen to do in the periods beginning with the years 2400 and 3600.

A TABLE showing, by means of the golden numbers, the several days on which the Paschal limits or full moons, according to the Gregorian account, have already happened, or will hereafter happen; from the reformation of the calendar A.D. 1582, to the year 4199 inclusive.

Golden Numbers from the Year 1583 to 1699, and so on to 4199, all inclusive.																Paschal full Moons.
1583 to 1699	1700 to 1899	1900 to 2199	2200 to 2299	2300 to 2399	2400 to 2499	2500 to 2599	2600 to 2899	2900 to 3099	3100 to 3399	3400 to 3499	3500 to 3599	3600 to 3699	3700 to 3799	3800 to 4099	4100 to 4199	Days of the Month, and Sund. letters.
3	14	.	6	17	6	17	.	9	.	1	12	1	12	.	4	Mar. 21. C
.	3	14	.	6	.	6	17	.	9	.	1	.	1	12	.	.... 22. D
11	.	3	14	.	14	.	6	17	.	9	.	9	.	1	12	.... 23. E
.	11	.	3	14	3	14	.	6	17	.	9	.	9	.	1	.... 24. F
19	.	11	.	3	.	3	14	.	6	17	.	17	.	9	.	.... 25. G
8	19	.	11	.	11	.	3	14	.	6	17	6	17	.	9	.... 26. A
.	8	19	.	11	.	11	.	3	14	.	6	.	6	17	.	.... 27. B
16	.	8	19	.	19	.	11	.	3	14	.	14	.	6	17	.... 28. C
5	16	.	8	19	8	19	.	11	.	3	14	3	14	.	6	.... 29. D
.	5	16	.	8	.	8	19	.	11	.	3	.	3	14	.	.... 30. E
13	.	5	16	.	16	.	8	19	.	11	.	11	.	3	14	.... 31. F
2	13	.	5	16	5	16	.	8	19	.	11	.	11	.	3	Apr. 1. G
.	2	13	.	5	.	5	16	.	8	19	.	19	.	11	.	.... 2. A
10	.	2	13	.	13	.	5	16	.	8	19	8	19	.	11	.... 3. B
.	10	.	2	13	2	13	.	5	16	.	8	.	8	19	.	.... 4. C
18	.	10	.	2	.	2	13	.	5	16	.	16	.	8	19	.... 5. D
7	18	.	10	.	10	.	2	13	.	5	16	5	16	.	8	.... 6. E
.	7	18	.	10	.	10	.	2	13	.	5	.	5	16	.	.... 7. F
15	.	7	18	.	18	.	10	.	2	13	.	13	.	5	16	.... 8. G
4	15	.	7	18	7	18	.	10	.	2	13	2	13	.	5	.... 9. A
.	4	15	.	7	.	7	18	.	10	.	2	.	2	13	.	.... 10. B
12	.	4	15	.	15	.	7	18	.	10	.	10	.	2	13	.... 11. C
1	12	.	4	15	4	15	.	7	18	.	10	.	10	.	2	.... 12. D
.	1	12	.	4	.	4	15	.	7	18	.	18	.	10	.	.... 13. E
9	.	1	12	.	12	.	4	15	.	7	18	7	18	.	10	.... 14. F
.	9	.	1	12	1	12	.	4	15	.	7	.	7	18	.	.... 15. G
17	.	9	.	1	.	1	12	.	4	15	.	15	.	7	18	.... 16. A
6	17	17	9	.	9	.	.	12	12	4	15	4	15	15	7	.... 17. B
14	6	6	17	9	17	9	9	1	1	12	4	12	4	4	15	.... 18. C
																.... 19. D
																.... 20. E
																.... 21. F
																.... 22. G
																.... 23. A
																.... 24. B
																.... 25. C

To find the day on which the Paschal limit or full moon falls in any given year; look, in the column of golden numbers belonging to that period of time in which the given year is contained, for the golden number of that year; over-against which, in the same line, continued to the column entitled Paschal full moons, you will find the day of the month, on which the Paschal limit or full moon happens in that year. And the Sunday next after that day is Easter day in that year, according to the Gregorian account.

Of the Morbus Strangulatorius. By John Starr, M.D. N° 495, p. 435.

Dr. S. mentions, that there had been raging in the neighbourhood of Liskard for some time previous to Jan. 10, 1749, (the date when this account was written) a disease formidable in its advances, and fatal in its consequences, viz. an occult angina, called with some propriety morbus strangulatorius. Dr. Fothergill's sore throat with ulcers, and Dr. Cotton's St. Alban's scarlet fever, &c. are in his opinion but its shadows. None practising in those parts have reason to boast their success in attempting its cure. The way to cure disorders is first to know them. Where the deviations of nature are hidden, where we cannot discern how and in what manner the distressed functions suffer, the art of healing must have its difficulties. The sudden, and indeed unexpected death of some patients greatly alarmed him. He concluded the cause deeper than at first imagined. The case herewith sent, confirmed his conjecture. It is extraordinary and uncommon. Does (he asks) medical history afford its like? it is possible it may, but it had not fallen within the compass of his reading, or study. Tulpius's Observation, lib. iv. cap. ix. falls vastly short of it.

The morbus strangulatorius, with great propriety and justice thus denominated, had a few years before reigned in several parts of Cornwall with great severity. Many parishes had felt its cruelty, and whole families of children, whence its contagious nature was but too evident, had, by its successive attacks, been swept off. Few, very few, had escaped. The disorder did not appear with the same train of symptoms in every subject. On the contrary, a vast difference was observable; but then whatever, or how various soever, the symptoms might be, there was a certain degree of malignity, or signs of a putrid disposition of the juices, in all.

Some, he was informed, had had corrosive pustules in the groin, and about the anus, eating quick and deep, and threatening mortification, even in the beginning. Others after a few days illness had numbers of the worst and deepest petechiæ break out in various parts of their body. Such he had not seen. Many on the first attack had complained of swellings of the glands, as tonsils, parotids, submaxillary and sublingual glands, but frequently of no great importance. A few, from an internal tumour, had a large external œdematous swelling of the subcutaneous and cellular tunic, from the chin down to the thyroïd gland, and up the side of the face. One such he was concerned with, the tumour broke in the fauces; but, instead of a laudable pus, some ounces of a coffee-coloured exceedingly fetid matter were spit off. The man recovered. As respiration only suffered here by pressure, he rather chose to call this a malignant angina, than the true morbus strangulatorius.

Not a few early in the disorder had gangrenous sloughs formed in their mouths, and perhaps so early in some, that the disorder was scarcely complained of, till the slough was formed, so quick had it been in its progress. Others again, without any of the preceding symptoms, had only complained of a slight pain in swallowing, succeeded with a hot flesh, feverish pulse, never quick and weak, but as to the stroke quick, and sufficiently full and strong, a short, low, hecking, hoarse cough, the patient generally so hoarse as to be difficultly understood after a day or two's illness, which sooner or later, for he never could observe any certain period, was productive of a difficult, noisy, and strangulating respiration. These last, especially the former of them, he esteems as the pathognomonic symptoms of the real morbus strangulatorius: the above-mentioned were rather symptomata causæ, quam morbi.

He had not mentioned a fœtor oris, which, when it happened, was usually an early symptom, because, though some had it, others had it not.

This respiration, however agonizing it appeared, had, especially in the beginning, its remissions and exacerbations. Its cause could not of course be permanent. He took it to be owing to a lodgement of some matter in or about the glottis, and larynx, through which the inspired air is obliged to pass; while this matter was capable of being expectorated, and happened to be coughed off, the breathing for a time became free, and the patient was delivered from the utmost seeming distress; but, on its recollection, which, if the progress of the disorder could not be stopped, never failed to happen, this symptom again occurred, and the patient either died suddenly, or being worn out, or quite dispirited, sank away gradually, or, falling into convulsions, in these expired.

He was called to a girl of 5 years old. Her tongue was quite clean; she could move it every way as in health. Nothing morbid was seen in her mouth, or indeed fauces: she had a trifling pain in swallowing, it was felt on depressing the epiglottis for the passing the bole, not sufficient to prevent her from eating bread and butter, biscuit, figs. It was on the 4th day of her disorder she had the strangulating respiration, with a cough exceedingly hoarse. After the use of a stimulating gargle, &c. her cough became stronger, and she threw off a large quantity of white rotten flesh, or membranes, mixed with a slimy adhesive matter; her respiration became so easy, that she seemed to ail nothing. In 3 hours it grew again difficult, and gradually increased till it arrived at its former violence. Those about her fancied there was somewhat in the passage which ought to come off: the child gargled, and provoked her cough as far as she was able, but in vain. Her agonies increasing, she said, as well as she was able, "I shall be choaked," and in a few minutes died. This case shocked Dr. S., being satisfied, that somewhat very extraordinary and uncommon could only occasion so sudden, and seemingly, violent a death.

He had frequently examined the matter those patients had at times spit.

Though there was some difference in various subjects, yet he never once saw a well-digested or concocted phlegm, or mucus: on the contrary, the greatest part was of a jelly-like nature, glary, and somewhat transparent, mixed with a white opaque thready matter, sometimes more, sometimes less, resembling a rotten membranous body or slough. Such a slough he had seen generated on the skin of one of these patients in the neck and arm, where blisters had been before applied. The blisters had been dressed with colewort leaves, and ran but little; but, contiguous to them, small red pustules, not exceedingly fiery, arose, which, sweating plentifully in a few hours, became quite white; these, hourly enlarging their bases, united, and covered a large surface, fresh pustules arising in the adjacent parts. This white surface had the aspect of an oversoaked membrane, which, being oversoaked, was become absolutely rotten. The part blistered, if not quite, was in effect dry, and the flux from the slough was incredibly great. If he mistook not, clothes 10 times double, the child's shift, a double bed-gown, were wet quite through, and a large spot was seen in the bed of some hands breadth, and this in a very few hours. He scratched the slough with his nail; it separated with ease, and without being felt by the child. What his nails took off afforded the same appearance with the matter of the spittle before-mentioned. Hence, he thought, he saw sufficient reason to convince him that the disorder in the larynx and aspera arteria was similar to this, generated in the same manner, and arising from the same internal cause; and supposing this conjecture true, the production of every symptom seems easy to be accounted for.

Dec. 1748, while the morbus strangulatorius was at Liskard, a child here and there had red pustules, not unlike the above, which broke out in the nape of the neck, and threw off a surprising quantity of thin transparent ichor, vastly glutinous when dry. These were easily cured in the beginning, if managed aright; but, being drawn with colewort leaves, or poulticed according to the direction of our old female practitioners, the above mentioned slough was soon generated. Dr. S. was desired to look on a poor person's child in this unhappy situation, who, with little intermission for near 2 days, had bled profusely at the nose; her pulse was almost gone; the bleeding was with difficulty stopped; but, being quite exhausted, in about 6 hours she sunk in a faint fit. The slough had spread from shoulder to shoulder, extended full a third down her back, and seemed very thick. All treated in the above manner died. Scarifying afforded no relief.

Now, though this was not properly the morbus strangulatorius, yet he apprehends it was analogous to it, and produced from the same cause; and it is likely, had the anatomical knife been employed, what was seen on the back of one might have been discovered in the aspera arteria of the other. There is a circumstance which adds to the probability of this opinion, viz. in one or more instances, these different disorders appeared in different subjects, in the same family, at the same time.

What he had hitherto said, did not demonstrate the case to be as represented, but the following history throws the strongest light on this dark, mysterious affair, renders the disorder, by its consequences affrightful, even shocking to the imagination, accounts for its too common fatality, and must prove the great difficulty of the cure, if in itself possible, unless attempted with judgment in the very beginning.

Dec. 11, 1749, he was called to the son of Mr. Kitto, an honest and deserving farmer in the parish of St. Eve, a lad aged 10½ years. This was the 7th day of his illness. His first complaints were, a pain in swallowing, not great; a cough, hoarse, vexatious, like an incipient catarrh, a pain on coughing shot into his ears. This was still felt at times; a thin ichor ran from his mouth, in great plenty, supposed to be a quart, or 3 pints daily. His pain in swallowing was now so trifling, that the Dr. saw him drink a considerable draught without removing the vessel. He was now so hoarse that he could scarcely be heard. His cough was rough, low, short, and ineffectual; breathed with much straitness and noise, especially in inspiration; the wheezing or rattling might be heard at a great distance, was always worse during a coughing fit, or for a short time after. When he spit by the cough, it was glary, but glutinous; a whitish rotten sort of stuff would sometimes accompany it; its quantity never great.

Examining his mouth, he could move his tongue every way without the least pain; forward it was clean, but behind a little furred. Depressing it with a spatula, a white body was seen on the velum pendulum palatinum and tonsils. Dr. S. desired Mr. Scotchburn, a surgeon present, to examine with his forceps, if this body adhered firmly to the velum, or was loose; on trial he found it strongly adhered. The lad complained of no pain on his taking hold of it. The circumambient parts of a somewhat deeper red than natural; his breath stinking, and highly offensive. He was but little thirsty, pulse quick, but sufficiently strong; slept but little; what sleep he had was disturbed; he breathed much better up than in bed; here he was always in danger of suffocation, and feared it.

After pronouncing a prognostic disagreeable to himself, and all concerned, the Dr. ordered the slough, as he then thought it, to be well rubbed once in 3 hours a mixture acuated with spir. sal. marin. by means of a silver probe armed with cotton, after which, an astringent, detergent, antiseptic gargle was to be frequently used, and a cordial mixture to be taken at proper intervals.

After rubbing with the probe, &c. twice, and gargling often, in a violent fit of coughing with a deal of slimy filthy stuff from the pipe of the lungs, an irregular membrane separated from the velum palatinum. It was really the external and mucous coat of the part, was not rotten like a slough, but retained, though dead, its membranous structure, was strong, would bear handling, and stretching without breaking. It was at first thick, having its fibres and cavities soaked with a very viscid and slimy matter, which, by washing in water, leaked off, when the membrane became evidently thinner. The lad immediately, it seems, breathed better, without that noise and wheezing heard before, and was less hoarse; not, he thinks, from the separation of the membrane, but from that load of filth discharged at the same point of time from the distressed respiratory passages.

But, as usual, this relief did not prove lasting. In $1\frac{1}{2}$ hour the noisy respiration began anew, his hoarseness increased, and his cough, though short and low, was busy and vexatious; now he appeared as if quite strangled, and in the agonies of death; now he would again revive; for a few days he was interchangeably in these different states; at length his father perceiving somewhat in his mouth, which he thought thick phlegm, thrust in his finger and thumb, and, taking hold of it, drew it out. It was a hollow bag, as he thought, filled with rot and corruption, for a considerable quantity ran out of it. It was, when full, he said, as thick as his thumb, and of many inches in length. The agonies of the child, during these moments, were not to be expressed; his face was livid or black; but, being freed from this burthen, he soon revived, smiled, and said, "now I am easy." Being put to bed, he soon slept, and continued to have short naps for 2 hours.

Dr. S. got to the house, being sent for in the beginning of the lad's extremity, a few minutes after the affair was thus concluded. The account greatly surprised him; but he was more surprised, when, on sight, he found the supposed bag was the mucous coat of part of the larynx, the whole aspera arteria, with the grand division of the bronchial ramifications. He spread it on paper, for the conveniency of carriage, being some miles from home, and thence took its likeness with great exactness. There was something bloody visible about its middle. It was more rotten and tender than the former, also somewhat thicker, excepting where it belonged to the branches of the bronchia. What sweated from it was as sticking as bird-lime. It is probable this morbid affection ran through the whole bronchia; for the ends plainly discovered a laceration; consequently much more remained to be separated and discharged.

He now complained of soreness in the pipe, and pointed to the first and second costa, as the place of its termination. His inspiration was now free, soft, but short; his pulse was become a little more frequent and weaker. Examining his mouth, no ulcer or wound was discernible in that part of the velum, &c. It was smooth, clean, and looked only like a new skin not quite hardened. While the Dr. was in the house, he spit off another membrane of an irregular figure, thinner than either of the former, but more than sufficient to cover a crown-piece.

It came from the fauces. After this he was informed he brought off with difficulty another tubular membrane of some length; and whenever he had strength to expectorate, little bits of the same were observed mixed with a very slimy mucus. He lived 21 hours after the second coat was drawn from him, and died in the end somewhat suddenly, though in his perfect senses. Dr. S. adds, that he never saw one in this disorder attacked with a delirium.*

Of the Strength of several of the principal Purging Waters, especially of that of Jessop's Well. By the Rev. Ste. Hales, D. D. and F. R. S. With a Letter from Swithin Adee, M. D., F. R. S. on the Virtues of the said Well. N° 495, p. 446.

An account of the several quantities of sediment which were found in a pound Avoirdupois of the following purging waters, evaporated away to dryness, in Florence flasks, cut to a wide mouth; viz.

Marybone fields, near London, 24 grains. Peter-street brewhouse, Westminster, 27; Ebsham 34; Scarborough 40;—And it was found nearly the same by Dr. Shaw and Dr. Short: a little more or less according to the wetness or dryness of the seasons, $\frac{1}{3.2}$ of this in calcareous matter; the rest, mostly what is called nitrous salts, on account of the oblong crystals which it shoots into.—Dog and Duck, Lambeth $40\frac{1}{2}$ grains; Kilburn, 4 miles from London, in the way to Edgeware 43; Acton 44; Cheltenham, Gloucestershire, 60.

Dr. Short found the following proportions in Cheltenham water, viz.—Sept. 1738, calcareous sediment $\frac{1}{10.6}$ of 74 grains; Dec. 1738, $\frac{1}{14}$ of 42; July 1739, $\frac{1}{9.8}$ of 70; He says it is the best and strongest nitro calcareous water in England, very bitter, having only a little subtile impalpable earth, mixed with its salt.

Cobham well, a mile south of Church Cobham, Surrey, once 68 grains, another time 60 grains.

Jessop's well, on Stoke Common, in Mr. Vincent's manor, about 3 miles southward of Claremont, Surrey, Sept. 11, 1749, after long dry weather, 82 grains in a pound of the surface water; Oct. 16, after a considerable quantity of rain, the surface water yielded but 60 grains. Nov. 21, the surface water yielded 65 grains.

This great inequality of the strength of the surface-water put him upon trying whether the water at the bottom of the well, near the springs, were stronger than the surface-water. And he found that the lower water yielded 82 grains, the surface-water only 48 grains; and it was the same on a second evaporation of

* The case last described was evidently a case of cynanche trachealis or croup; but some of the preceding observations in Dr. Starr's paper, seem to relate to the cynanche maligna or gangrenous sore throat.

those waters. Hence we see how much stronger the water near the bottom is, than at the surface; even when the preceding rains have been but moderate; for they had not as yet been sufficient to raise the springs in this country much. Hence we see that the stronger lower water may easily be come at by means of a pump; as also, that the upper land springs, soon after rains, make the water near the surface weaker: but, in long dry weather, when there are no land-springs, the surface-water, and that at the bottom, are nearly of an equal strength: for it requires time for the saline mineral virtue to be equally diffused through a mass of that depth of water, whose upper part is incessantly weakened by a land-spring of fresh water. Hence we see how adviseable it is, in order to keep out the land-springs, to dig a narrow trench some feet depth, round the well, to be filled with stiff clay well rammed.

The mineral virtue in this water seems to be much like that of Cheltenham, in its shooting into very bitter, regular, oblong crystals, which are, on that account, called nitrous; though they are not a true nitre: for neither these, nor those of Cheltenham, will deflagrate or flash in touch-paper, nor on burning charcoal, as true nitre will do; some of which will retain their form and firmness for 17 months after being crystallized; whereas the crystallized salts of several other purging waters have crumbled, and in a great measure wasted away in much less time: a greater proportion of the salts of Jessop's well shoot into oblong crystals, than those of Cheltenham; and its water also gives a stronger green tincture, with violet-flowers. The purging quality resides chiefly in these crystalline salts, and a small proportion of common salt; some of which there is in all these mineral waters.

The proportion also of its earthy calcareous matter, is but $\frac{1}{11.4}$ part of it; which, like that of Cheltenham, is but little, in comparison of the much greater quantity of it in other purging waters: it is also soft and impalpable, like that of Cheltenham, and not harsh and coarse, as it is in some other purging waters.

And as the quantity of purging salt in this water is considerably greater than in any other, so it is found by experience, that proportionably a less quantity of it suffices, which makes it sit the better on the stomach. It is also observed to exhilarate those who take it.

It was observable of the sediment of several of these waters, that when dried, and while hot, there ascended plenty of invisible volatile salt fumes, so pungent that the nose could not bear them. Hence we may reasonably conclude, that the waters which abound most with purging salts, such as those of Jessop's well, should be proportionably preferable to weaker waters, which are strengthened by boiling half away; by which not only the more subtile active parts are evaporated; but those that are left are decompounded, and formed into new grosser combinations; as are also the calcareous particles, which are so fine as to pass the filter before evaporation, but not after it. This was the reason which induced him to examine, by various repeated trials, and to give an account of the superior strength of Jessop's well water, above all others that he had examined or heard of.

When Jessop's well was cleaned, Oct. 16, 1749, after a considerable quantity of rain, after about half a foot depth of black muddy filth was taken out, then the natural fat sandy-coloured clay-bottom appeared; through several parts of which the water oozed up at the rate of 160 gallons in 24 hours.

The water which then came fresh from the spring gave a weak blush with galls; but when put into bottles it did not do so next day; a sign that there is some degree of steel in it.

It was very observable, that the man who stood about 3 hours bare-legged in this well-water to clean it, was purged so severely for a week, that he said he would not venture, on any account, thus to clean the well again. And it was the same with another man, who cleaned the same well about 12 years since. And he was credibly informed by a merchant, that being in a warehouse in Egypt to see senna baled up, it had the like purgative effect on him.

To get a satisfactory account of the efficacy of these waters, he desired Dr. Adee of Guildford, who has long prescribed them to his patients, to give his opinion of them; which he did as follows.

I have found very advantageous and uncommon effects from the use of the waters of Jessop's well. Some of my patients, who have drank them steadily and cautiously, have been cured of obstinate scurvies. As I had a long time ago reason to think there was a fine volatile spirit in them, I obliged some to drink them for a course of time at the well as an alterative, with very happy consequences. When I have ordered them as a purge, they have worked very smartly, but have not dispirited. I am glad to have it in my power to confirm your sentiments by my own observations; and am satisfied these waters, if continued a proper time, and taken in a proper manner, may be rendered very beneficial to mankind.

Abstract of a Discourse intitled, Reflections on the Medals of Pescennius Niger, and on some Circumstances in the History of his Life; written in French by Mr. Claude Gros de Boze, Keeper of the Medals in the French King's Cabinet, &c. and sent by him to Dr. Mead, who communicated it to this Society. By John Ward, R. P. G., F. R. S. N° 495, p. 452.

The learned author begins his discourse with observing, that no medals of the Roman emperors, who reigned during the high empire, are more rare, than those of Pescennius Niger; that they are somewhat scarcer in silver, than in brass; and that it is the general opinion of antiquaries, there is not one extant in gold, though there have been counterfeits: as he shows from the account of several authors on such coins.

In the years 1726 and 1727 he received accounts of one and the same gold medal of Pescennius, as brought from 4 different quarters; first from Spain, then from Sicily, afterwards from Malta, and lastly from England. But he found it to be false, as all others had done, who had seen it. It had been cast from a silver one of that prince, on the reverse of which is the figure of the goddess Hope; with the inscription of BONAE SPEI, which is the most common of any. Those in the cabinets of Arschot and Saxe Gotha have likewise the same reverse, and doubtless from the same origin. And the like disappointment attended several other accounts.

At length in July 1748, Mr. de Boze had fresh encouragement to pursue his inquiry; which he did with greater attention, and better success, than before. A barefooted Carmelite of the convent of Paris shewed him a letter, which he had received from one of his own order at Marseilles, who lately arrived from the Levant, where he had been employed as a missionary. His correspondent acquainted him, that he had a gold medal of Pescennius, which the curious at Marseilles were desirous to purchase, and had offered him a considerable sum for it; but as he hoped to get more at Paris, especially if it was not in the king's cabinet, he desired him to let him know that, as also what value Mr. de Boze put upon it. His answer was, that he would certainly give a good price for it, if it was ancient; but that he could offer nothing, till he had seen it. The owner therefore brought him the medal, which was fair, well preserved, and free from any thing, which might occasion the least suspicion; so that he valued it considerably higher, than what had before been offered, and immediately purchased it for the king.

Soon after he shewed it to the greatest connoisseurs and most curious persons at Paris, who were charmed with the sight of so valuable and unexpected a medal in the royal cabinet. And many both natives and foreigners being desirous of a draught of it, he ordered it to be engraved; together with a Greek medallion in silver, no less rare in its kind, of the same emperor, which is also in the same cabinet, having been purchased at London by Mr. Vaillant of Mr. Falkner, father of Sir Everard. A print of both these pieces accompanies this paper. See fig. 3, 4, pl. I.

The gold medal, fig 10, has on one side the head of Pescennius Niger crowned with laurel, with this legend, IMP CAES C PESC NIGER IVSTVS AVG. And on the reverse, the goddess Concord, represented by a female figure standing, with a diadem on her head, one of her hands elevated, and a double horn of plenty in the other; and round the figure only the word CONCORDIA. For the letters PP, placed below in the field, on the 2 sides of the figure, being the usual abbreviation of PATER PATRIAE, are to be considered as part of the inscription surrounding the head of Pescennius.

A Letter from Robert More, Esq.; containing several Curious Remarks in his Travels through Italy. N° 495, p. 464.

Mr. M. thinks that travellers do not seem sufficiently to have considered the force and effects of steam, which may be formed by springs of water falling on a vast surface of the fluid lava, but talk too much of sulphur, deceived by the complexion of a salt that covers the ground in some places there. In the Solfatara he held a cold iron in the vent, and there ran down it a stream of water. When he went down into the crater on the top of Vesuvius, it was full of smoke. Yet he did not perceive it suffocating but thought it steam. What the guides call sulphur, when he got it home, ran per deliquium.

At Arienzo, a village half way to Beneventum, are coppice-woods, from which they make manna. They are of the tree which our gardeners call the flowering ash. The manna is procured by wounding the bark at the season, and catching the sap in cups: it begins to run (they used the scripture term piovere, i. e. to rain) the beginning of August; and, if the season proves dry, they gather it 5 or 6 weeks. The king has a great revenue from it; yet the tree grows as well in England.

The fire among the snows, on the Apennines, he imagined to be of the same sort with that about a little well at Brosely* in Shropshire; of which the Society has had an account; the same as of the foul air sent them from Sir James Lowther's† coal pits; and the like made by a gentleman with filings of iron and oil of vitriol. The flame when he saw it, was extremely bright, covered a surface of about 3 yards by 2, and rose about 4 feet high. After great rains and snows, they said, the whole bare patch, of about 9 yards diameter, flames. The gravel, out of which it rises, at a very little depth, is quite cold. There are 3 of these fires in that neighbourhood; and there was one they call extinct. He went to the place to light it up again, and left it flaming. The middle of the last place is a little hollowed, and had in it a puddle of water: there were strong ebullitions of air through the water. But that air would not take fire; yet what rose through the wet and cold gravel flamed brightly. Near either of these flames, removing the surface of the gravel, that below would take fire from lighted matches.

* See Philos. Trans. N° 482.—Orig.

† N° 482, N° 442.—Orig.

Extract of a Letter from Mr. William Arderon, F. R. S. Containing an Account of a Dwarf; with a Comparison of his Dimensions with those of a Child under 4 Years old. Dated Norwich, May 12, 1750. By David Erskine Baker. N° 495, p. 467.

John Coan, a dwarf, was born at Twitshall in Norfolk, in the year 1728, and has been shewn in this city for some weeks past. Mr. A. weighed him April 3, 1750, and his weight, with all his cloaths, was no more than 34 pounds. He likewise carefully measured him, and found his height, with his hat, shoes, and wig on, to be 38 inches. His limbs are no larger than a child of 3 or 4 years old: his body is perfectly straight: the lineaments of his face answerable to his age; and his brow has some wrinkles in it, when he looks attentively at any thing. He has a good complexion, is of a sprightly temper, discourses readily and pertinently considering his education, and reads and writes English well. His speech is a little hollow, though not disagreeable; he can sing tolerably, and amuses the company that come to see him, with mimicking of cock's crowing, which he imitates very exactly. In 1744 he was 36 inches high, and weighed $27\frac{1}{4}$lb. His father says, when about a year old he was as large as children of that age usually are, but grew very little and slowly afterwards.

On receiving the account of this little man, a * child of 3 years and not quite 9 months old, son of the late very worthy William Jones, Esq. F. R. S. was measured and weighed. This boy, though very lively and handsome, is no way remarkable for his size; and therefore his dimensions and weight, compared with the dwarf's, may give a tolerable idea of the real smallness of the dwarf.

The weight of the dwarf, with all his cloaths on, was no more than 34lb. The child's weight, with its cloaths likewise on, was 36lb.

The height of the dwarf, with his shoes, hat, and wig on $38\frac{5}{10}$ inches; the height of the child, without any thing on his head $37\frac{7}{10}$ inches; and so proportionably in all the other dimensions.

Concerning the Method of gathering Manna near Naples. By Robert More, Esq. N° 495, p. 470.

At Arienzo, a town between Naples and Benevento, he found an ash coppice, of 8 or 10 years growth, from which they collect manna. It seemed to have been tapped 2 years for that purpose; the branches had been barked each year about an inch broad, and 2 feet high; but he was told this was done by an inch at a time.

They place a cup at the bottom of the wound, which they empty every 5 days;

* This child it would seem, must have been that celebrated character, Sir Wm. Jones, chief judge in India.

and the liquor becomes manna. They formerly let it dry on the tree; but the present way keeps it cleaner. The manna begins to run (they say in the scripture style to rain) the beginning of August; and if the season proves dry, they gather it 5 or 6 weeks. The king of Naples has so large a revenue from it, that he is extremely jealous of it, and during the season he guards the woods by Sbirri, who even fire on people that come into them, and he makes the stealing of the liquor death. Mr. M. believes it to be what our gardeners call the flowering ash; the complexion of the bark and bud agrees with one of them he had in his garden at Lindley. The man who shewed the wood said it bore a pretty flower in the spring.——At Pisa, in the physic garden, they shewed that tree in bloom as the manna-ash. The tree is indeed common enough in that neighbourhood: the Italians call it Orno. A botanist at Rome said it was the ornus officinarum.* A physician at Benevento to the same purpose, that it was the ornus used in medicine.

Observations on the Northern Lights, seen Feb. 15 *and* 16, 1749-50. *By John Huxham, M. D., F. R. S.* N° 495, p. 472.

Feb. 15, 1749-50, in the evening there was a very vivid northern light, which darted forth several beautiful, crimson, and fiery-coloured rays; wind N W b N 1, barometer 30. 2; 50 minutes past 8 a surprizingly bright and exceedingly white arch, about the breadth of a common rainbow, appeared in the heavens, extending nearly from east to west; it reached within 5 or 6 degrees of the western horizon, and ended about 8 or 10 above the eastern. It passed exactly between Castor and Pollux, and directly over Aldebaran, which appeared plainly through it. Near the top of the arch several very lucid, white, short, vibrating columns were attached to it; none of them seemed above 6 or 7 degrees long, and did not appear to communicate in the least with the aurora borealis. About $9^h\ 12^m$ the arch vanished; but several white, bright, corruscating nubeculæ remained here and there in the zodiac for 12 or 15 minutes longer. The aurora borealis continued more or less till midnight.

Feb. 16, about 7 p. m. was another aurora borealis, though not quite so fiery and luminous as that of the night before: it continued till near 11. At $8^h\ 56^m$ p. m. exactly, such another arch appeared, very nearly of the same extent and direction, but not altogether so broad or lucid. This at first also passed between the 2 bright stars of Gemini, but declined more and more to the southward, till it was 2 or 3 degrees to the south of Pollux. Its western limb, about 9, passed through the north shoulder of Orion: it quite disappeared about 10 or 12 minutes after.

Of a Horse bitten by a Mad Dog. By John Starr, M. D. N° 495, p. 474.

Dec. 1, 1745, a neighbour's large mastiff dog, mad, broke out in the night

* Fraxinus Ornus, Lin. a more particular account of which is given in the 60th vol. of these Trans.

from the place where he was too carelessly confined; and, by a rotten back window, entered Dr. S.'s stable, fell upon his horse, and bit him in many places, as the shoulder, breast, and right nostril; which was indeed much torn. He bled largely. The town being early alarmed by this mad dog, and the horse being found loose, his collar broke to pieces, wounded in many places, and much blood scattered up and down the stable, it was too justly concluded the dog had fallen upon him.

According to Desault's method, and what Dr. James said, in a letter Dr. S. had from him on another occasion, would effectually prevent the ill consequences of this bite; he immediately ordered the wounds to be well rubbed with a mercurial ointment, ex axung. porc. ℥vj. argent. viv. ℥ij. About ℥ij. were at times expended. Next morning he was bled 2lb. or more; after which he gave him in milk* lichen ciner. terrest. ℥vj. pip. nig. ℥iiij. 5 mornings successively; which he repeated at the end of a fortnight for 4 mornings more.

As the pulv. antilyssus was not in our shops, and no one in town knew the lichen but himself, he went with his servant Sunday forenoon, the day of his horse's misfortune, to seek it. What he found was, he fears too young; for it seemed just coming from the earth, and the leaves were scarcely one third as large as its full growth. He got what he hoped might be sufficient; and, after cleansing, perhaps too hastily dried it at the fire, that it might be ready for use the next day.

The wounds healed up soon, without any other application; and the horse fed uncommonly hearty after a day or 2, during which the fright had made him uneasy and fretful, and seemed to improve considerably in every respect. He omitted riding him for 20 days; but about the 20th rode him 2 short journeys only. He travelled chearful and brisk, and he took care not to heat him. He saw him every day, but could in no respect discover any thing amiss.

Dec. 25, two days before the full moon, his servant told him, that in the morning he trembled much on entering the horse-pool, and refused to drink at the watering trough; but in the evening drank heartily at another well. This alarmed him; but considering that horses frequently refuse to drink there, and that he drank in the evening, he was somewhat easy; but ordered the servant if he refused next morning drinking at one, to try him at the other; and if he refused at both, to let him immediately know it.

Dec. 26, as soon as he entered the horse-pool, he trembled all over in a most surprizing manner, and would by no means attempt to drink. The servant immediately returned with him. Dr. S. ordered him to be led into a small pool of rain water which stood in the court. The trembling returned; every muscle

* See these Trans. N° 237, p. 49, anno 1697.—Orig.

was strangely agitated; he looked as if he were melancholy on the water, smelt to it, but would not touch it. Being put into the stable, a bucket of pure clean water was brought to him; he eagerly thrust his mouth into the water, but, endeavouring to suck it, a convulsion seized him.

Dr. S. was now satisfied he had a true aquæ pavor. He was bled to about 3 pints, musk ʒß. cinnab. ant. ʒj. made into a ball with cons. anthos was given him. In bleeding he once snapt at the smith, though well known to him, having shod him for years: and indeed this was the only time he attempted to bite any one.

In about 2 hours after the musk was given him, Dr. S. offered him with his own hands about 2 gallons of white water warm: he drank it off without the least difficulty or hesitation. Had he dissolved in it 2 oz. of nitre, he had (he thinks) done well.

Had the quantity of musk at first given been greater (for Dr. James writes, that he gives the above quantity of the best musk in a watchfulness remaining after a febrile delirium is removed,) or had Dr. S. now again repeated the same ball, he was apt to think the horse might have been saved (this being the Tonquin method, even after the appearance of the hydrophobia;) for he was as yet quiet and tractable. He went to him as usual, handled him, and he behaved as in his former health; he ate both hay and oats heartily. In the evening, about 9 o'clock, more of the white water was offered him, but he drank none.

Dec. 27. This night the madness increased much; for he had bitten the manger as far as he could reach, and made it quite ragged. In the morning he frequently bit his breast where the wound had been; and when he happened to take hold, violently drew up the skin with his teeth. Both these things he did during the day at times, but most in the morning. Dr. S. put a tub of water before him; he greedily ran his nose into it; but, endeavouring to drink, a dreadful convulsion seized him, which sometimes drew his buttock to the ground; at others his back was so hollowed with it, that his belly was brought almost down on the litter. During the convulsion he would groan in an affecting manner; and frequently cry out. As soon as the convulsion was over, he repeated his endeavours to drink with the same cruel event; and would, he believed, had the water stood before him, have repeated it the whole day.

He still eat his allowance of hay and oats; but when not eating, he was continually thrusting out his tongue, and working with his lips, as if to moisten and cool them. His tongue was exceedingly dry, and of a blackish brown colour on the surface. As he eat oats, Dr. S. sometimes lamented he had not mixed turpeth. mineral. with them. He tried him with water about 9 at night; every thing was as in the morning; only the convulsion was stronger, if possible, and more excruciating; for he groaned deeper, louder, and in a more affecting tone.

His breath was exceedingly hot; it came from his nostrils like smoke from a chimney top; he expanded his nostrils as if he had been violently running; and the steam was visible for more than a yard distance.

Dec. 28. This night he broke his collar in pieces, broke down the partition by which he was separated from the place of Dr. S.'s other horse, traversed the stable, attempted to get out; in order to which he beat down the under half of the stable door; however, in the morning, being spoken to by the servant, he neighed, immediately went to his place, where he stood biting his breast and manger almost continually. His look was now become wild and furious, and about 10 o'clock Dr. S. ordered him to be shot.

P. S. Dr. S. observed he was always worse, every symptom being aggravated at the time the moon came to the meridian; which again, as the day declined, in some degree abated.

Of a Monstrous Fetus without any Distinction of Sex. By Job Baster, Acad. Cæs., F.R.S. N° 495, p. 479.

A woman about the 7th month of her pregnancy, was delivered of a monstrous child. The head was not of the natural round figure, but pointed at the top. The right arm was well formed; but the radius and ulna of the left much shorter. There was no appearance of any genital parts or anus: but instead the skin lay in rolls, with much fat. From the middle of the belly proceeded one foot only, ending as it were in one toe, but without a nail.

Experiments on Substances Resisting Putrefaction. By John Pringle, M.D., F.R.S.* N° 495, p. 480.

Having been led to make some experiments and remarks on putrefaction, from the accident of having had an uncommon number of putrid distempers under his

* Dr. (afterwards Sir) J. Pringle was descended from a good family in Scotland, where he was born in 1707. He studied first at St. Andrew's, next at Edinburgh, and afterwards at Leyden, where he attended the lectures of the celebrated Boerhaave, and took his degree of M. D. there in 1730. Not long afterwards he returned to Edinburgh. Here in 1734 he was appointed joint professor of moral philosophy with Mr. Scott. Through the recommendation of Dr. Stevenson, he was appointed, in 1742, physician to the Earl of Stair, who had the command of the British army that was destined to co-operate with the allies in Flanders. This was the foundation of our author's subsequent celebrity and fortune. He was afterwards made physician-general to the British forces in the Low Countries, and physician also to the Duke of Cumberland, whom he attended in his expedition against the rebels in Scotland. His services as army-physician ceasing at the peace of Aix-la-Chapelle, Dr. P. occupied himself in writing his Observations on the Jail Fever, and in making experiments on septic and anti-septic substances, which he communicated to the R. S., and for which he was honoured with the Copleian medal. In 1752 he published his great work, the result of long and diligent observation while he was attached as physician to the land forces, entitled Observations on Diseases of the Army. This work has gone through numerous editions, and its value is too well

care in the hospitals of the army, Dr. P. ventured to lay before the Society what he had found somewhat different from the common opinion, as well as some facts, which, as far as he knew, had not been mentioned before.

1. Finding it a received notion, that bodies by putrefaction became highly alkaline, he made the following experiments, to inquire how far this was the fact;

The serum of human blood putrefied, made, with a solution of sublimate, first a turbid mixture, and afterwards a precipitation. This is one of the tests of an alkali, but scarcely to be admitted here; since the same thing was done with recent urine (of a person in health), which is never accounted alkaline. The same serum did not tinge the syrup of violets green; and made no effervescence when the spirit of vitriol was poured on it. He made the experiment twice on portions of different serum, both highly putrid; and once on water, in which corrupted flesh had been some time infused; and the most he could find was, that, having given the syrup previously a small reddish cast with an acid, this colour was rendered fainter, but not destroyed by the putrid humours; and as to the effervescence, having dropped the spirit of vitriol into these liquors unmixed, and also diluted with water, the mixture was quiet, and only a few air bubbles

established to need commendation here. It may be remarked however that in the treatment of some disorders he resorted too freely to venesection, his partiality for which was doubtless to be ascribed to the precepts of Boerhaave. In 1758 he was admitted a licentiate of the Lond. Coll. of Phys. Two or 3 years afterwards he was appointed physician to the queen's household. In 1763 he was made physician extraordinary to her majesty, and in the same year he was elected a fellow of the Lond. Coll. of Phys. In 1766 he was raised to the dignity of a baronet. He had been chosen F. R. S. in 1745. After he had been one of the council of that learned body for several years, he had the honour in 1772, of being called to the president's chair. In this situation he delivered, for a succession of years, some admirable discourses, on presenting Sir G. Copley's gold medal to those members who had distinguished themselves by the communication of important improvements and discoveries in science. These discourses were afterwards printed at the request of the R. S. They are 6 in number, and were delivered on presenting the aforesaid medals to Dr. Priestley, Mr. Walsh, Dr. Maskeleyne, astronomer royal, Mr. Mudge, and Dr. Charles Hutton, mathematical professor at Woolwich, for their respective papers. The medal for Captain Cook was assigned to him while he was out on his 3d voyage of discovery (1776), from which he never returned. About 2 years after his elevation to the presidency of the R. S. Sir J. Pringle was appointed physician extraordinary to the king; and various foreign academies had elected him one of their members. In the midst of these honours his health began to decline, insomuch, that in 1778, finding the situation of president of the R. S. to be attended with much inconvenience and fatigue; he felt himself under the necessity of resigning the chair, and was succeeded in that honourable situation by Sir Joseph Banks. After this Sir J. P. went to Scotland, and resided some time at Edinburgh; but deriving no benefit from this change, he again removed to London, where he died in 1782, being then in the 75th year of his age.

Had Sir J. Pringle's merits been confined to his profession, he would still have held a conspicuous place in the list of British physicians; but he possessed a mind capable of mastering more than one branch of science; and if he was justly celebrated for his skill in physic, he was equally entitled to distinction for his acquirements in philosophy. It is this union of general science with professional skill, that gives a physician the fairest title to respect and reward.

appeared on shaking the glasses. On the whole, though there were some marks of a latent alkali in the putrid serum, they were so very faint, that one drop of spirit of hartshorn in a quantity of water equal to that of the putrid liquors, showed more of an alkali than 20 drops of any of the other.

2. It has been a maxim, that all animal substances, after putrefaction, being distilled, send forth a great quantity of volatile salt in the first water; but Mr. Boyle (Nat. Hist. of Human Blood, vol. iv. p. 178, fol. ed.) found that this held good only in urine; and that in the distillation of the serum of human blood putrefied, the liquor which first came over had little strength, either as to its smell or taste, and did not at first effervesce with an acid. And here it may be observed, that the chemists have generally applied those properties they discover in urine, to all the humours indifferently; whereas in fact there is a great diversity. For some animal substances, such as urine and bile, soon putrefy; the saliva and the white of an egg slowly. Yet those that soonest corrupt do not always arrive at the highest degree of putrefaction. Thus the bile is soon corruptible, but the rankness of it is not to be compared to that of flesh; and the white of an egg is not only much less disposed to putrefy than the yolk, but, when corrupted, yields a different and less offensive smell. And it seems particular to stale urine to contain an alkaline salt, which, without distillation, makes a strong effervescence with acids: whereas most other animal humours putrefied, though of a more intolerable fetor, yet contain less volatile salt, less extricable, and not effervescing with acids. But what makes the difference between stale urine and other putrid substances still more specific, is its inoffensiveness with regard to health; while the steams of most other corrupted bodies are often the cause of putrid and malignant diseases.

Now, on finding in urine a much greater quantity of volatile salt, and that more easily separable than in any other humour, and that stale urine is the least noxious of putrid animal substances, so far then from dreading the volatile alkali as the deleterious part of corrupted bodies, from this instance we may rather infer it to be a sort of corrector of putrefaction.

3. Daily experience shows how harmless the volatiles are, both when smelled to, or taken in substance; but still there remains a prejudice, as if these salts, being the produce of corruption, should therefore hasten putrefaction; not only in distempers where these salts are unwarily taken, but also in experiments out of the body.

Now, as to the effects arising from the internal use of them, little can be said, unless the kind of disease was precisely stated. For supposing they were by their nature disposed to promote putrefaction; yet if that is already begun, from a languor of circulation, and obstruction, then may the volatiles, by their stimulating and aperient quality, be the means of stopping its progress: and on the other hand, though they were really antiseptic, yet if the humours are disposed to corrupt from excess of heat or motion, these very salts, by adding to the cause, may augment the disease. So that, on the whole, it will be the fairest criterion of the nature of these volatiles, to inquire, whether out of the body they accelerate or retard putrefaction.

In order to decide this question, Dr. P. made repeated experiments of joining both the spirit and salt of hartshorn to various animal substances; and had constantly found, that so far from promoting putrefaction, they have evidently hindered it; and that with a power proportioned to their quantity. The trials have been made with the serum of the blood, and also with the crassamentum, after it had been dried by keeping. He once separated the thick inflammatory crust of pleuritic blood from the rest of the mass; and dividing it, he put one portion into distilled vinegar, the other into spirit of hartshorn; and after keeping the infusions above a month in the middle of summer, he found the piece which lay in alkaline spirit as sound as that in the acid. Another time he put in one phial about 1½ oz of an equal mixture of ox's gall and water, with 100 drops of spirit of hartshorn; and in another as much of the gall and water without any spirit. The phials, being corked, were set by a fire, so as to receive about the degree of animal heat; by which in less than 2 days, the mixture without the spirit became putrid, but the other was not only then, but after 2 days longer, untainted.

He afterwards infused 2 drs. of the lean of beef with 2 oz. of water and ½ a dr. of salt of hartshorn. Another phial contained as much flesh and water, with a double quantity of sea-salt: in a 3d was the flesh and water only, to serve by way of index. These phials were placed on a lamp-furnace, in a heat varying between 94 and 104 degrees of Fahrenheit's scale. About 18 hours after infusion, the contents of that phial which served as an index, were rank; and in a few hours more that with the sea-salt was also putrid; but the flesh with the volatile alkali was sound, and continued so after standing 24 hours longer, in the same degree of heat: and that the smell of the hartshorn might occasion no deception, the piece of flesh was washed from the salt, and still smelled sweet.

About the same time he took 3 pieces of fresh beef, of the same weight as above; and laying 2 of them in gallypots, he covered one with saw dust, and the other with bran: but the 3d piece being strewed with salt of hartshorn powdered he put into a 4-oz. phial which had a glass stopper. They were all 3 placed in the outside of a window exposed to the sun; and the weather being warm, on the 3d day the flesh in the gallypots began to smell; on the 4th were putrid. Next day the phial was examined; when the flesh was washed from the salt, and found quite sweet. It was then dried and salted again with hartshorn; and having stood in the house some weeks longer in sultry weather, it was looked at a second time, and observed to be as sound as before; neither was the substance at all dissolved, but was of such a consistence as might be expected from common brine.* And lest it might be suspected, that the flesh in the gallypots by being more exposed to the air than that in the phial, became sooner putrid, he afterwards inclosed flesh in phials, as that with the hartshorn, and found the confinement rather hasten the putrefaction.

Now, by these and many other experiments of the kind, finding that volatile alkaline salts not only do not dispose animal substances to putrefaction out of the body, but even prevent it, and that more powerfully than common sea-salt, we may presume that the same taken by way of medicine, will, cæteris paribus, prove antiseptic; at least we cannot justly suppose them corrupters of the humours more than fermented spirits or sea-salt; which taken in immoderate quantities may raise a fever, and thus accidentally be the occasion of corruption.

4. He had likewise made several experiments with the fixed alkaline salts, which have no less antiseptic power than the volatile. The trials were made both with the lie of tartar and salt of wormwood. But here we must not confound a disagreeable smell of such mixtures, with one that is really putrid; nor the power those lixivials have of dissolving animal substances, with putrefaction.

5. From these experiments it was natural to conclude, since acids by themselves were amongst the most powerful antiseptics, and the alkaline salts were likewise of that class, that the mixtures of the two to saturation would resist putrefaction little less than the acid alone. But in the trials he made on flesh with a spiritus mindereri, composed of vinegar saturated with salt of hartshorn, and also with the juice of lemons saturated with the salt of wormwood, he found the antiseptic virtue considerably less than when either the acids or alkalis were used singly.

6. As for the comparative virtues of these salts on flesh, he found $\frac{1}{2}$ oz. of lemon-juice saturated with 1 scr. of the salt of wormwood resisted putrefaction, nearly as much as 15 grains of nitre; but when the trial was made with ox's gall, 2 drs. of this mixture were more antiseptic than 1 scr. of that salt. Again, nitre compared with the dry neutral salts, weight for weight, is more antiseptic than any, in preserving flesh he had yet tried. Crude sal ammoniac came next to it and even exceeded it in the experiment with ox's gall. After these the sal diureticus, tartarus solubilis, and tartarus vitriolatus, seemed to have nearly the same power.

He had mixed vinegar with a large quantity both of chalk and crabs-eyes, in order to neutralize it; but, though seemingly saturated, by the effervescence ceasing, it still retained an acidity, and was found much more antiseptic than

* The same piece after being kept dry a twelvemonth, was untainted, and as firm as at first.—Orig.

lemon-juice neutralized with the salt of wormwood; though this last acid be considerably stronger than vinegar.

7. Thus far have we considered the common neutral salts; which, however powerful in resisting putrefaction, are inferior to some resinous substances, and even some vegetables which he had tried. Thus myrrh, in a watery menstruum, was found at least 12 times more antiseptic than sea-salt. Two grains of camphor, mixed with water, preserved flesh better than 60 grs. of that salt: and he imagined, could the camphor be kept from flying off, or concreting to the sides of the phial, that $\frac{1}{2}$ gr. or even less, would have sufficed. An infusion of a few grs. of Virginian snake-root in powder, exceeded 12 times its weight of sea-salt. Camomile flowers have nearly the same extraordinary quality. The Jesuits' bark has it also; and if he had not found it so strong as the two substances last mentioned, he imputes that in part to his not being able to extract its embalming parts in plain water.

Now vegetables possessing this balsamic quality are the more valuable, in that, being usually free of acrimony, they may be taken in much greater quantities than either spirits, acids, resins, or even the neutral salts. And as in the great variety of substances answering this purpose, there may be also some offensive or useful qualities annexed, it may not be amiss perhaps to review some part of the materia medica for this end.

He adds, that, besides this extraordinary power in preserving bodies, he had discovered in some of these substances a sweetening or correcting quality, after putrefaction had actually begun. But these experiments he should lay before the Society some other time; with a table of the comparative force of salts, and some further remarks on the same subject.

An Attempt to explain an Ancient Greek Inscription, engraven on a Curious Bronze Cup with 2 Handles, and published with a Draught of the Cup by Dr. Pococke, in his Description of the East, vol. ii. part 2, p. 207. By John Ward, F.R.S. N° 495, p. 488.

The diameter of the cup on the inside is about $13\frac{1}{2}$ inches; and the inscription is placed round the upper side of the rim.

As to the circular form of the inscription, we read in Pausanias of an instance not very much unlike this. Iphitus king of Elis is said to have restored the Olympic games, during which all hostilities ceased among the several states of Peloponnesus. Throwing the discus or quoit was one of the exercises performed in those games, and the discus of Iphitus was deposited in the temple of Juno at Olympia; on which the cessation of arms, always observed at that solemnity, being engraved, was then publicly read. Which inscription, as the historian observes, was not cut in straight lines, but in the form of a circle. This inscription Mr. Ward thinks may be thus read in the common Greek characters:

ΜΝΔ
ΒΑΣΙΛΕΥΣ ΜΙΘΡΑΔΑΤΗΣ ΕΥΠΑΤΩΡ
ΤΟΙΣ ΕΝΤΟΣ ΤΟΥ ΓΥΜΝΑΣΙΟΥ ΕΥΠΑΤΟΡΙΣΤΑΙΣ
ΓΥΦΑ vel ΓΟΥΦΑ ΔΙΕΣΩΣΕ.

In Latin thus:
Monumentum dedit
Rex Mithridates Eupator Eupatoridis
in gymnasio [vel intra gymnasium]
Gypha [vel Gupha] servavit.

The letters Μ Ν Δ stand by themselves over the rest, which are placed below them in the form of a circle. And the situation of these 3 letters shows over what words of the circular part they are placed.

Abstracts of several Observations of Auroræ Boreales lately seen. By Mr. Henry Baker, F.R.S. N° 495, p. 499.

On Tuesday, Jan. 23, 1750, some unusual appearances were observed in the sky, at London, and the towns about it, by thousands of people during the whole evening, of which some accounts were laid before the R.S. And as appearances of the like kind were observed in the heavens, the same evening, at great distances from London, the following is a description of what was seen at the city of Norwich by Mr. Wm. Arderon, F.R.S.; and also of what was observed at Wells (a little sea-port town in the same county of Norfolk, about 30 miles nearly due north from Norwich) by Mr. Joseph Sparshal, and sent by him to Mr. Arderon.

This wonderful aurora began at 6 o'clock in the evening, at Norwich, with a blackish cloud in the N.E., out of which sprang up a streak of scarlet-coloured rays, of a surprizing beauty and vividness. This presently extended to within a few degrees of the S.W. horizon, passing directly through the zenith, and so continuing near a quarter of an hour, when red and yellow columns began to rise upwards from every quarter. At 7 o'clock a black cloud rose up in the S.E. and quickly took a semicircular form, with light yellowish vapours ascending out of its upper edge, and representing a glory of an uncommon brightness. At 8 o'clock the black cloud was dispersed, but the yellow glory remained; and round that sprang up another circle of red, which made the whole appear very tremendous. The reddish streams, as well as this last mentioned circle, were sometimes so dense, that even stars of the first magnitude could not be seen through them.

On Tuesday Jan. 23, the air at Wells was clear and serene during the greatest part of the day, with a fresh breeze of wind at S.S.E. which terminated in an evening extremely remarkable for appearances in the heavens of an uncommon aurora borealia. At 15 minutes past 5, Mr. Sparshal noticed the foot of an arch, which formed an angle of about 10° with the N.E. part of the horizon. This arch shot out pointed streams like pyramids, of a fiery red colour, which generally ascended within a few degrees of the zenith, then vanished, and were immediately succeeded by others, from the N.E., where the principal magazine seemed to be. They continually shifted towards the E. and S.W. with sudden flashings and dartings; but towards the west the appearances seldom altered. At 30 minutes past 5, a luminous stream, of a bright flame-colour, shot up on the N. side of the fiery arch, which still kept somewhat of that form, though frequently interrupted by shooting flashes from the N. E. At 40 minutes past 5, there appeared suddenly in the N.E. an elliptical corona, of an amazing brightness, elevated about 9° above the horizon, and having its longest diameter parallel to it. There shot up perpendicularly from this streams resembling columns of flame intermixed with others of bright red. And so on, for various other curious appearances.

At the beginning of these lights the mercury stood at 29.9, but quickly fell to 29.8. The wind at S.E. During part of the time there was an uncommon motion in the magnetic needle. And this evening were seen several of those meteors called falling stars; particularly some which, on taking fire, left a long train of sparks behind them.

Description of a Mariner's Compass contrived by Gowin Knight, M.B., F.R.S. Nº 495, p. 505.

Almost all the compasses on board merchants' ships have had their needles formed of 2 pieces of steel wire; each bent in the middle, so as to make an obtuse angle; and their ends, being applied together, make an acute one; so that the whole represents the form of a lozenge; in the centre of which, and of the card, is placed the brass cap. Mr. M. procured 20 cards, with needles of this kind fixed to them; and after touching them with a pair of large bars, he tried each of them, with the same cup and pin, by drawing them aside 90° from the true point, and then seeing where they would rest. He found them all to vary more or less, either to the east or west; and some of them as far as 8°. Few of them came to the same degree twice together; and when they did, that was never the true point. In short, they not only varied from the true direction, but from each other, and from themselves. He then tried, by drawing them gently aside, how far he could make them stand from the true point, without returning; and found they might frequently be made to do it at the distance of a whole point on either side. One of them, which generally varied 6 or 7 degrees to the east, being drawn the same way, would stand at 16°.

All these irregularities are owing to the structure of the needle: for the wires, of which it is composed, are only hardened at the ends; and that is done by making the ends red-hot, and quenching them in water: if all these ends are not equally hard, or if one end be hardened higher up than the other, when they come to be put together, in fixing them to the card, that end which is hardest will destroy much of the virtue of the other; by which means the hardest end will have most power in directing the card, and must consequently make it vary towards its own direction. If you retouch these wires when fixed to the card, the error will still remain; for that wire which is best hardened will always become the strongest.

The wires being disposed in the form of a lozenge is the reason why these cards had so little force, that they might be made to stand at the distance of several degrees, on either side the point from which they were drawn. For all magnetical bodies receive an additional strength by being placed in the direction of the earth's magnetism, and act proportionably less vigorously when turned out of it. So that, when such needles are drawn aside from their true point, 2 of the parallel sides of the lozenge will conspire more directly than before with the earth's magnetism; and the other 2 will be less in that direction: by which means the first 2 sides will very much impede its return; and the latter 2 will have that impediment to overcome, as well as the friction, by their own force alone.

The needles that are used on board the men of war, and some of the larger trading ships, are made of one piece of steel, of a spring temper, and are broad towards the ends, but tapering towards the middle, where a hole is made to receive the cap. At the ends they terminate in an angle greater or less, according to the skill or fancy of the workman. Now, though the worst of these are infinitely preferable to those of wire, yet the best of them are far from being perfect. Every needle of this form has 6 poles instead of 2. There is one at each end, 2 where it becomes tapering, and 2 at the hole in the middle. This is owing to their shape; for the middle part being very slender, it has not substance enough to conduct the magnetic stream quite through from one end to the other. All these poles appear very distinctly, when examined with a glass that is sprinkled over with magnetic sand. Yet this circumstance does not hinder the needle from pointing true; but as it has less force to move the card, than when the magnetic stream moves in large curves from one end to the other, it is certainly an imperfection.

Two needles, that were quite straight, and square at the ends, were found to have only 2 poles; but about the hole in the middle the curves formed of steel-dust were a little confused. These always came exactly to the same point, after vibrating a long time; and if drawn ever so little on one side, would return to it again without any sensible difference. We may therefore conclude, that a regular parallelopiped is the best shape for a needle, as well as the simplest; with the holes for the caps as small as can well be contrived; or if it can be made to answer the purpose without any hole at all, it will be still more perfect. Yet the common shape has one advantage which this has not: for being made broad at the ends, and slender in the middle, its weight is removed as far as possible from the centre: on which account, if it once points true, the friction at the centre cannot so easily put it in motion; and its vibrations, when in motion, will be slower; so that their limits may be more nicely observed, and the middle point between them is that where it would stand, if at rest.

Being unwilling to part with these advantages, Mr. K. contrived a light circle of brass, of the same diameter with the card, which will supply a weight acting at the greatest distance from the centre of motion, and also serve to support the card; which may now be made of thin paper, without any thing to stiffen it. So that the extraordinary weight of the brass ring is compensated in a great measure by the lightness of the card. This ring is of service in another respect; for being fixed below the card, and the needle above it, the centre of gravity is placed low enough to admit of the cap being put under the needle; by which the hole in the needle becomes unnecessary; and the latter being placed above the card, renders it easier to be touched with a pair of bars.

Having thus completed the needle and card to his satisfaction, what chiefly remained, was to contrive such a cap and point as will have the least friction, and be most likely to continue in a state of perfection. The caps in use are either of brass, a mixed metal, like that of a reflecting telescope, crystal, or agate. The first 2 will only admit of brass points, and the latter are rather too expensive for common use. He therefore thought of trying glass caps: he had 3 of them made by a glass-blower, 2 of which he got polished: they were all set in brass, so as to screw into the same needle, which had also one of agate fitted to it. He compared them with that of agate, by trying with each of them how many vibrations the same card and needle would make, when drawn aside 90°, on the same point; which was a very small sewing needle.

The number of vibrations with the agate cap, on the first trial, were 39, then 37, then 39 again; with one of the glass caps it made 23, and then 20. This difference from the agate cap was so great, that he concluded the point must be damaged, and therefore chose a finer; on which the same glass cap made 41 vibrations; then 43; and another glass cap made 47, and the next time 43. But the agate cap with this point made 51, 57, and 58 vibrations. The unpolished glass cap performed much the same with the others. He had 2 of them polished again by Mr. Smeaton; and in company with him repeated the same experiments; but with no better success. The agate cap made always many more vibrations than the glass one; and generally with the latter the number diminished by repeated trials; whereas with the agate cap it usually increased.

These experiments made him lay aside the glass caps, and put him on thinking how agate ones might be made with as little expence as possible. With this view he got a cap turned of ivory, in such a manner as to receive a small bit of agate at the top. This being ground concave, and polished on that side, where it formed the apex of the hollow cone in the cap, was capable of answering the purpose as well as if the whole had been agate, and was much lighter. These caps may be made cheap enough for common use; and if good at first cannot easily be impaired.

For a point, he chose a common sewing needle, and contrived to fix it in such a manner as to be taken out with the greatest ease, and replaced by another, if necessary; by which means an excellent point may be always had with little trouble or expence. Common needles, when well tempered, have all the qualifications that can be desired for the purpose intended. The smallest are strong enough to bear the weight of a card; and are neither so soft as to be liable to bend, nor so hard and brittle as to break; and they are generally better pointed than any that a common workman could pretend to make extempore.

The specimen of the improved compass, shown to the Society, was made by Mr. Smeaton, a gentleman whose uncommon skill in the theory and practice of mechanics has enabled him to execute whatever Mr. K. proposed in such a manner as always to exceed his expectations: and not only so, but Mr. S. added a considerable improvement of his own. By a very simple contrivance he made the same instrument capable of serving the purposes of an azimuth and amplitude compass; and that in a manner much preferable to any thing hitherto contrived; the description and use of which he has drawn up himself, for the perusal of the Society, as follows.

On some Improvements of the Mariner's Compass, in order to render the Card and Needle proposed by Dr. Knight, of General Use. By John Smeaton, Philosophical Instrument-maker.* N° 495, p. 513.

The cover of the wooden box being taken off, the compass is in a condition to be used in the bittacle, when the weather is moderate: but when the sea runs

* John Smeaton, a celebrated engineer, was born 1724, at Austhorpe, near Leeds; where also he died in 1792, in the 69th year of his age. Mr. S. seems to have been born an engineer. The originality of his genius, and the strength of his understanding, appeared at a very early age. His

high, as the inner box is hung very free on its centres, the better to answer its other purposes, it will be necessary to slacken the milled nut, placed on one of the axes that supports the ring, and to tighten the nut on the outside that corresponds to it. By this means the inner box and ring will be lifted up from the edges, on which they rest, when free; and the friction will be increased, and that to any degree necessary to prevent the too great vibrations; which otherwise would be occasioned by the motion of the ship.

To make the compass useful in taking the magnetic azimuth, or amplitude of the sun and stars, as also the bearings of head-lands, ships, and other objects at a distance; the brass edge, designed at first to support the card, and throw its weight as near the circumference as possible, is itself divided into degrees and

playthings were not those of children, but the tools men work with; and he had always more amusement in observing artificers work, and asking them questions, than in any thing else. Continually occupied in such pursuits, Mr. S. acquired, at 18 years of age, an extensive set of tools, and the art of working in most of the mechanical trades; which he continued to work with occasionally to the end of his life.

Mr. Smeaton's father being an attorney, he thought of bringing up his son to the same profession. Accordingly he was sent up to London in 1742; where after some time employed in that line, finding that the practice of the law did not suit the *bent of his genius*, as he used to express it, he wrote a strong memorial on the subject to his father, whose good sense, from that moment, left Mr. S. to pursue the bent of his genius in his own way. After this, Mr. S. continued to reside in London, where, before the date of the above paper, 1750, he had commenced philosophical instrument maker, which he continued to exercise for some time, and formed an acquaintance with most of the ingenious men of that time.

In 1753 Mr. S. was admitted F.R.S., and in 1759 he was honoured with the Society's gold medal, for his paper on the natural powers of water and wind, to turn mills, and other machines depending on a circular motion. From about 1753 or 1754 Mr. S. seems to have practised as an engineer; soon after which he undertook to rebuild the Edystone light-house, which he completed with stone in 1759. In 1764 he was appointed one of the receivers of the forfeited Derwentwater estate, applied to the uses of Greenwich hospital; which office he held till 1777, when he resigned it in favour of Sir John Turner, a son of the Earl of Sandwich, then first lord of the admiralty.

After this, Mr. S. going into full employment as an engineer, it would be endless to attempt to particularize all the great works he so ably conducted, as mills, wheels, engines, levels, canals, bridges, harbours, &c. in all which he was equally eminent. Particularly he saved from immediate destruction London bridge, after the opening of its great arch. Indeed as a civil engineer Mr. S. was perhaps unrivalled, certainly not excelled by any one. Astronomy was also, for amusement, a favourite pursuit of Mr. S., and he made several curious instruments of this kind for his friends, as well as for himself; with which, to the time of his death, he continued to make many observations. The chief of Mr S.'s publications, was his History of Edystone Lighthouse. Besides which, many of his reports and memorials on the different works he was concerned in, were occasionally printed in his life-time; as well as an additional volume of the same since his death. He had also inserted in the Philos. Trans. a considerable number of valuable papers, both mechanical and astronomical, in most of the volumes from the year 1750 to 1776. A much larger account of this ingenious and worthy man may be seen in Dr. Hutton's Dictionary, from which the above particulars are extracted; or in the account of his life prefixed to the volume of his reports above mentioned.

halves; which may be easily estimated into smaller parts if necessary. The divisions are determined by means of a cat-gut line stretched perpendicularly with the box as near the brass edge as may be, that the parallax arising from a different position of the observer may be as little as possible. Under the card are 2 small weights, sliding on 2 wires, placed at right angles to each other; which, being moved nearer to or farther from the centre, counterbalance the dipping of the card in different latitudes, or restore its equilibrium, where it happens by any other means to be got too much out of level.

There is also added an index at the top of the inner box, which may be put on and taken off at pleasure, and serves for all altitudes of the object. It consists of a bar, equal in length to the diameter of the inner box; each end being furnished with a perpendicular stile, with a slit parallel to the sides. One of the slits is narrow, to which the eye is applied, and the other is wider, with a small catgut stretched up the middle of it, and from thence continued horizontally from the top of one stile to the top of the other: there is also a line drawn along the upper surface of the bar. These four, viz. the narrow slit, the horizontal catgut thread, the perpendicular one, and the line on the bar, are in the same plane, which disposes itself perpendicular to the horizon, when the inner box is at rest, and hangs free. This index does not move round, but is always placed on so as to answer the same side of the box.

When the sun's azimuth is desired, and his rays are strong enough to cast a shadow, turn about the wooden box, till the shadow of the horizontal thread; or, if the sun be too low, till that of the perpendicular thread in one stile, or the light through the slit in the other, fall on the line on the index bar, or vibrate to an equal distance on each side of it, gently touching the box, if it vibrate too far: observe at the same time the degree marked on the brass edge by the catgut line. In counting the degree for the azimuth, or any other angle reckoned from the meridian, make use of the outer circle of figures on the brass edge; and the situation of the index bar, with regard to the card and needle, will always direct on what quarter of the compass the object is placed.

But if the sun do not shine out sufficiently strong, place the eye behind the narrow slit in one of the stiles, and turn the wooden box about, till some part of the horizontal or perpendicular thread appear to intersect the centre of the sun, or vibrate to an equal distance on each side of it; using smoked glass next the eye, if the sun's light be too strong. In this method another observer will be generally necessary to note the degree cut by the nonius, at the same time the first gives notice that the thread appears to split the object. From what has been said, the other observations will be easily performed; only in the case of the sun's amplitude, take care to number the degree by the help of the inner circle of figures on the card, which are the complements of the outer to 90, and consequently shew the distance from east or west.

The azimuth of the stars may also be observed by night; a proper light serving equally for one observer to see the thread, and the other the degree on the card. It may not be amiss to remark further, that in case the inner box should lose its equilibrium, and consequently the index be out of the plane of a vertical circle, an accurate observation may still be made, provided the sun's shadow be distinct: for, by observing first with one end of the index towards the sun, and then the other, a mean of the 2 observations will be the truth.

Fig. 3, pl. 13, is a perspective view of the compass, when in order for observation. The point of view being the centre of the card, and the distance of the eye 2 feet; AB is the wooden box; CD are two milled nuts; by means of which the axes of the inner box and ring are taken from their edges, on which they move, and the friction increased, when necessary; EF is the ring that supports the inner box; GH is the inner box; and I is one of its axes, by which it is suspended on the ring EF; KL is the magnet or needle; and M a small brace of ivory, that confines the cap to its place. See fig. 4.

The card is a single varnished paper, reaching as far as the outer circle of figures, which is a circle of thin brass, the edge being turned down at right angles to the plane of the card to make it more stiff; O is a catgut line drawn down the inside of the box; for determining the degree on the brass edge; PQRS is the index bar, with its 2 stiles and catgut threads; which being taken off from the top of the box, is placed in two pieces, T and V, notched properly to receive it; W is a place cut out in the wood, serving as a handle.

Fig. 4 is the card in plano, with the needle fixed on it; being one third of the diameter of the real card.

Fig. 5 is a perspective view of the backside of the card; where AB represents the turning down of the brass edge; C is the under part of the ivory cap; D and E are the 2 sliding weights to balance the card; and F and G, 2 screws that fix the brass edge, &c. to the needle.

Fig. 6 is the pedestal that supports the card, containing a sewing needle, fixed in 2 small grooves to receive it, by means of the collet C, in the manner of a port-creyon. At D the stem is filed into an octagon, that it may be the more easily unscrewed.

Description of a Fish shown to the Royal Society by Mr. Ralph Bigland, March 22, 1749-50: drawn up by C. Mortimer, M. D., Sec. R. S.* N° 495, p. 518.

This fish is smooth skinned, has no scales, nor teeth. It has one erect fin on

* This fish is the Zeus Luna. *Lin. Syst. Nat. Gmel.* the Opah *Pennt. Brit. Zool.*

on its back, which arises below its neck, and runs within a little of its tail. On each side about the middle, between its back and belly, behind the gills is a fin: from the bottom and middle of its belly, a little forward of the vent, arise 2 fins: from behind the vent runs one fin, within a little of the tail: the tail-fin is large and forked. Its eyes are large; the irides are scarlet, encompassed with a circle of a gold colour verged with scarlet. Its nostrils are placed above its eyes. The back, and upper part of the body quite to the tail, is of a dark blue, or violet colour; these, and the sides of the body, which were of a bright green, are all speckled with oblong white spots; the chaps are of a pale red; the nose, gills, and belly, of a silver colour; and all the fins of a bright scarlet.

It was 3 feet 7 inches long, and 3 feet 10 inches round in the thickest part, and weighed 82 pounds. Its mouth is small: its tongue thick, almost like a human tongue in shape, but rough, and thick-set with beards or prickles, which pointed backwards; so that any thing might easily pass down, but could not easily slip back again; while these might serve instead of teeth for retaining its prey or food. Its gills resemble those of a salmon. Its body grows very taper towards the tail; and, from being compressed to 10 inches thick, becomes near the tail almost round, and about 3 inches thick. The whole shape of this fish much resembles the sea-bream; but it differs in size, being much larger, and in not having teeth nor scales. The fin standing erect on the back, has some aculei next the neck, and rises up 8 inches; but in the middle diminishes to 1 inch; and near the tail rises again to about 3 inches. The belly-fin opposite to this spreads 3 inches near the tail, and diminishes towards the vent. The tail-fin is forked, and spreads 12 inches. The gill-fins are 9 inches long, and 3 wide at their basis. The 2 belly-fins were 11 inches long, and 3 wide at their basis. It seems to be a new species of fish, not yet described by any author: but on the coast of Guinea is known by the name opah.

Mr. Bigland said, that, on opening it, all its bowels would have gone into a quart mug; that the flesh of the fore part was firm, and looked like beef, and the hinder part like fine veal; that the bones were like those of quadrupeds; particularly the shoulder blades, which resembled those of sheep.

On the Extirpation of an Excrescence from the Womb. By John Burton of York, M. D. N° 495, p. 520.

The wife of one Chapman, a whitesmith, at Selby, 10 miles from York, upwards of 7 years since, lay in of her last child, and had a tolerable easy labour: soon after which, she had what she called the fluor albus, that continued ever since, and increased on her; insomuch that she says, she has sometimes had such a discharge as to wet the place she sat on through all her clothes.

For some months before Dr. B. was concerned for her, she began to complain of a pain and weight in the uterus; which increased as the substance grew in bulk; and at last the excrescence was so large as to appear outwards, and then it grew very fast. The patient consulted her midwife, who thought the womb had come out; but was so prudent as not to do any thing; and desired they would call in better advice. Accordingly they sent for Mr. Fell, an eminent man-midwife and surgeon in this city; who, not having met with a case like that, desired Dr. B. also to go and see her, which was in December last (1749.) The substance not only filled, but extended, the entrance into the vagina. Dr. B. introduced a finger into the passage, and soon found the excrescence to be less in bulk there, than what appeared without the body. He followed the substance till he reached the os uteri, which he found chiefly filled up with the neck or smallest part of this substance, leaving only a small part of the os tincæ to be perceived on the left side, obliquely towards the back. He tried to penetrate the os tincæ with the end of his finger, but could not; however, he so far opened it, as to let out a sort of bloody ichor, which was a little offensive in smell. Hence, and from other inquiries, he concluded that she had an ulcer just within the os uteri, from the edge of which the fungus or excrescence grew. The patient complained of a pain in the uterus and back, was very faint, and frequently was provoked to vomit, with a feeble pulse, and sometimes sweat.

On consultation it was thought proper to tie a ligature as high up within the vagina as the surgeon could reach: which being done, and some internal medicines being ordered, they left her. And 4 or 5 days after, the excrescence dropped off at the ligature.

The patient afterwards, in part, recovered her strength; though she was not in a good state of health, and her fluor albus, as she called it, was still troublesome to her. The excrescence was very solid, of a dark liver colour, and, while adhering to the uterus, was quite insensible. When cut in two, it resembled the solid substance taken out of cancers.

The Eclipse of the Moon, June 8, 1750, observed in Surry-street in the Strand. By Mr. John Catlin and Mr. James Short, F. R. S. N° 496, p. 523.

About half an hour after 9 o'clock, the clouds clearing away, they saw the moon then totally eclipsed; though considerably brighter on the east than on the west side; by which they found that she was then past the middle of the eclipse. They then observed the

Emersion, or end of total darkness, at ...... $9^h\ 45^m\ 0^s$
End of the eclipse at 10 51 30

Continuation of the Experiments on Substances Resisting Putrefaction. By John Pringle, M.D., F.R.S. N° 496, p. 525.

1. Three pieces of the lean of fresh beef, each weighing 2 drs. were put separately into wide mouthed phials. Two ounces of cistern-water were added to each; in one were dissolved 30 grs. of sea-salt; in another 60; but the 3d contained nothing but flesh and water. These bottles were little more than half full; and, being corked, were placed in a lamp furnace, regulated by a thermometer, and kept about the degree of human heat.

About 10 or 12 hours after, the contents of the phial without salt had a faint smell; and in 3 or 4 hours more were putrid.* In an hour or 2 longer the flesh with the least salt was tainted; but that which had most, remained sweet above 30 hours after infusion. This experiment was often repeated with the same result, making allowance for variations of the degree of heat.

The use of this experiment was for making standards, for judging of the septic or antiseptic strength of bodies. Thus, if water with any ingredient preserved flesh better than without it, or better than with the additions of the salt, that ingredient might be said to resist putrefaction more than water alone, or with 30 or 60 grs, of sea-salt. But if, on the other hand, water, with any addition, promoted corruption more than when pure, the substance added was to be reckoned a septic, or hastener of putrefaction.

The following experiments were therefore all made in the same degree of heat with the quantity of flesh, water, and air, as above specified; together with such septic or antiseptic substances, as afterwards mentioned, and were all compared with the standards. But whereas the least quantity of salt preserved flesh little longer than plain water, Dr. P. always compared the several antiseptic bodies with the greatest quantity of salt; so that whenever any substance is said to oppose putrefaction more than the standard, he means more than 60 grs. of sea-salt.

He began with examining other salts, and compared them in the same quantity with the standard; which being of all the weakest, he supposed it equal to unity, and expressed the proportional strength of the rest in higher numbers in the following table.

A Table of the Comparative Powers of Salts in resisting Putrefaction.

Sea-salt	1	Tartar vitriolated	2
Sal gemmæ	1 +	Spiritus mindereri	2

* It is to be observed, that these pieces were all entire; but when they are beat to the consistence of a pap, with the same quantity of water, the putrefaction then begins in less than half the time mentioned here.—Orig.

Tartarus solubilis	2	Salt of hartshorn	4+
Sal diureticus	2+	Salt of wormwood	4+
Crude sal ammoniac	3	Borax	12+
Saline mixture	3	Salt of amber	20+
Nitre	4+	Alum	30+

In this table the proportions are marked by integral numbers; it being hard, and perhaps unnecessary, to bring this matter to more exactness; only to some the sign + is added, to shew that those salts are stronger than the number in the table by some fraction; unless in the last 3, where the same sign imports that the salt may be stronger by some units.* The tartar vitriolated is rated at 2; though more than 30 grs. of it was taken to equal the standard: but perceiving it was not wholly dissolved, an allowance was made accordingly. On the other hand, as part of the hartshorn flies off, its real force must be greater than what appears by the table. The salt of amber is likewise volatile; and as 3 grs. of it were found more preservative than 60 grs. of sea-salt; it may therefore be much more than 20 times stronger. This is indeed an acid salt; but as the acid part of it is inconsiderable, this high antiseptic power must be owing to some other principle. The sp. minder. was made of common vinegar and salt of hartshorn; the saline mixture of salt of wormwood saturated with lemon juice. The alkaline part in either of these mixtures with water only, would have resisted with a power of 4 +; so that the acid added rendered these salts less antiseptic; viz. the sp. minder. by a half, and the saline mixture by a 3d part: which was a circumstance very unexpected.

Next he proceeded to try resins and gums, and began with myrrh. As part of this substance dissolves in water, 8 grs. were made into an emulsion; but most of it subsiding, he could not reckon on a solution of more than 1 or 2 grs. which nevertheless preserving the flesh longer than the standard, we may account the soluble part of myrrh perhaps about 30 times stronger than sea-salt.

Aloes, asa fetida, and the terra japonica, dissolved in the same manner as myrrh, like it subsided, and with the same antiseptic force. But gum ammoniac and sagapenum shewed little of this virtue: whether it was that they opposed putrefaction less, or that all the antiseptic principle fell with the grosser parts to the bottom. Three grains of opium dissolved in water did not subside, and resisted putrefaction better than the salt. But more air than usual was generated, and the flesh became tenderer than with any of the stronger antiseptics.

* Five grs. of borax was the smallest quantity compared with sea-salt; but holding out so much longer, he suspected 3 grs. would have been sufficient; in which case the force of this salt was to be estimated at 20: a singular instance of the strength of salt not acid. One grain of alum was weaker than 60 grs. of sea-salt; but 2 grs. were stronger. The power therefore of alum lies between 30 and 60; but it seemed nearer the first number.—Orig.

Of all the resinous substances, camphire resisted most; 2 grs. dissolved in 1 drop of sp. of wine, 5 grs. of sugar, and 2 oz of water, exceeded the standard: though during the infusion most of the camphire flew off, swam at top, or stuck to the phial. Suppose only the half lost, the remainder is at least 60 times stronger than salt; but if, as he imagined, the water suspended not above a 10th part, then camphire will be 300 times more antiseptic than sea-salt. That nothing might be ascribed to the minute portion of the spirit, used in this experiment, he made another solution of camphire in a drop or 2 of oil, and found this mixture less perfect, but still beyond the standard.

4. He made strong infusions of camomile-flowers, and of Virginian snake-root; and finding them both greatly beyond the standard, he gradually lessened the quantity of these materials, till he found 5 grs. of either impart a virtue to water superior to 60 grs. of salt. Now as we cannot suppose these weak infusions contained $\frac{1}{2}$ gr. of the embalming part of these vegetables, it follows, that this must be at least 120 times more antiseptic than common salt.

He also made a strong decoction of the bark, and infused a piece of flesh in 2 oz. of it strained; which flesh never corrupted, though it remained 2 or 3 days in the furnace, after the standard was putrid. In this time the decoction became gradually limpid, while the grosser parts subsided: by which it appears, that a most minute portion of the bark intimately mixed with water (perhaps less than of the snake-root, or camomile-flowers) is possessed of a very extraordinary antiseptic force.

Besides these, pepper, ginger, saffron, contrayerva-root, and galls, in the quantity of 5 grs. each, as also 10 grs. of dried sage, of rhubarb, and the root of wild valerian,* separately infused, exceeded 60 grs. of salt. Mint, angelica, groundivy, senna, green tea, red roses, common wormwood, mustard, and horse-radish, were likewise infused, but in larger quantities, and proved more antiseptic than the standard. And as none of these can be supposed to yield in the water above 1 gr. or 2 of the embalming principle, we may consider them all as very powerful resisters of putrefaction. Further, he made a trial with a decoction of white poppy-heads, and another with the expressed juice of lettuce, and found them both above the standard.

By these specimens we may now see how extensive antiseptics are; since, besides salts, fermented spirits, spices, and acids, commonly known to have this property, many resins, astringents, and refrigerants, are of the number; and even those plants called anti-acids, and supposed hasteners of putrefaction; of which class horse-radish is particularly antiseptic. And indeed after these trials,

* Though the experiment was only made with 10 grs. of the powder of this root, yet considering how long that quantity resisted putrefaction, we may reckon the valerian among the strongest antiseptics.—Orig.

he expected to find all dissolvable substances endowed with some degree of this quality; till on further experiments, he perceived some made no resistance, and others promoted corruption. But before entering on that part of the subject, he deems it proper to relate some other experiments more nearly connected with the preceding.

5. Having seen how much more antiseptic these infusions were than sea-salt, he then tried whether plants would part with this virtue without infusion. For this purpose, having 3 small and thin slices of the lean of beef, he rubbed one with the powder of the bark, another with snake-root, and a third with camomile flowers. It was in the heat of summer, yet, after keeping these pieces for several days, he found the flesh with the bark but little tainted, and the other 2 quite sweet. The substance of all the 3 was firm, particularly that with the camomile, which was so hard and dry, that it seemed incorruptible. Why the bark had not altogether the same effect, was probably owing to its close texture.

6. He had also made some attempts towards the sweetening of corrupted flesh, by means of mild substances; because distilled spirits, or strong acids, the only things known to answer this intention, were of too acrid and irritating a nature to be thoroughly useful, when this correction was most wanted. As for salts, besides their acrimony, it is well known that meat once tainted will not take salt.

A piece of flesh weighing 2 drs. which in a former experiment had become putrid, and was therefore very tender, spongy, and specifically lighter than water, was thrown into a few ounces of the infusion of camomile-flowers, after expressing the air, to make it sink in the fluid: the infusion was renewed twice or thrice in as many days; when, perceiving the fætor gone, he put the flesh into a clean bottle, with a fresh infusion; and this he kept all the summer, and had it then by him, quite sweet, and of a firm texture.* In like manner he had been able to sweeten several small pieces of putrid flesh, by repeated affusions of a strong decoction of the bark; and he constantly observed, that not only the corrupted smell was removed, but a firmness restored to the fibres.

Now, since the bark parted with so much of its virtue in water, it was natural to think it would still yield more in the body, when opened by the saliva and bile; and therefore it was by this antiseptic virtue it chiefly operated. From this principle we might account for its success in gangrenes, and in the low state of malignant fevers, when the humours are so evidently putrid. And for intermittents, in which the bark is most specific, were we to judge of their nature, from circumstances attending them in climates and seasons most liable to the distemper, we should assign putrefaction as a principal cause. They are the

* This piece was kept a twelvemonth in the same liquor, and was then firm and uncorrupted.—Orig.

great endemic of all marshy countries, and rage most after hot summers, with a close and moist state of air. They begin at the end of summer, and continue through autumn; being at the worst, when the atmosphere is most loaded with the effluvia of stagnating water, rendered more putrid by vegetables and animal substances that rot in it. At such times all meats are quickly tainted; and dysenteries, with other putrid distempers, coincide with these fevers. The heats dispose the humours to acrimony; the putrid effluvia are a ferment; and the fogs and dews, so common to those climates, stop perspiration, and bring on a fever. The more these causes prevail, the easier it is to trace this putrefaction of humours. The nausea, thirst, bitter taste of the mouth, and frequent evacuations of putrid bile, are common symptoms and arguments for what is advanced. We shall add, that in moist countries, in bad seasons, the intermittents not only begin with symptoms of a putrid fever, but, if unduly managed, easily change into a putrid and malignant form, with livid spots and blotches, and mortification of the bowels. But, as a thorough discussion of this question might carry him too far from the present subject, and be unseasonable here, he refers it to its proper place, and only remarks, that whatever medicines (besides evacuations and the bark) have been found useful in the cure of intermittents, they are, so far as he knows, all highly antiseptic; such are myrrh, camphire, camomile-flowers, wormwood, tincture of roses, alum with nutmeg, vitriolic or strong vegetable acids with aromatics.

Thus far are only related experiments on flesh, or the fibrous parts of animals; he next intends to shew what effects antiseptics have on the humours; for though from analogy we may conclude, that whatever retards the corruption of the solids, or recovers them after they are tainted, will act similarly on the fluids; yet, as this does not certainly follow, he judged it necessary to make new trials; which, with some experiments on the promoters of putrefaction, the reverse of the former, will be given hereafter.

Concerning a Flat Spheroidal Stone having Lines Regularly Crossing it. By Mr. Joseph Platt, of Manchester. N° 496, p. 534.

A man found a stone at Ardwick, 7 feet deep, near this town, in driving a slough through some gret stone. It is what is called a nodule, of a close, compact, smooth matter; was incrustated with coarser earth, or soft stone; is 3 inches and a half diameter; formed not unlike one of the echini marini; except the papillæ or small protuberances, which it wants. It had 4 white seams, about the thickness of a horse-hair, which quarter the stone very correctly. The angles are exactly the same, and correspond so well, that it would require the nicest mathematical head and hand to draw the like.

On Bees, and their Method of Gathering Wax and Honey. By Arthur Dobbs, Esq. N° 496, p. 536.

The only 2 things in which Mr. Dobbs differs from M. Reaumur, are, that he apprehends Mr. R. says, the bees range from flowers of one species to those of another, while they are gathering one load; so that the farina, or crude wax, loaded on their legs, is from different species of flowers; which is contrary to what Mr. D. observed. The other thing that he differed from him in is, that he says the wax is formed in the bee, from the crude wax, or farina (so far Mr. D. agrees with him:) but by his observations, he says, after digestion it is discharged upwards by the mouth; whereas, by Mr. D.'s observations, it is the fæces, husks, or shells of the farina or crude wax, after digestion, discharged by the anus.

As to the first, says Mr. Dobbs, I have frequently followed a bee loading the farina, bee-bread, or crude wax, on its legs, through part of a great field in flower; and on whatever flower it first alighted and gathered the farina, it continued gathering from that kind of flower; and passed over many other species of flowers, though very numerous in the field, without alighting on, or loading from them; though the flower it chose was much scarcer in the field than the others: so that if it began to load from a daisy, it continued loading from the same, neglecting clover, honeysuckles, violets, &c.: and if it began with any of the others, it continued loading from the same kind, passing over the daisy. So in a garden, on the wall-trees, I have seen it load from a peach, and pass over apricots, plumbs, cherries, &c. yet made no distinction between a peach and an almond.

Now M. Reaumur, in his memoir on the bee's making honey, mentions Aristotle's observation of the bee's loading or gathering from one species of flower without changing; not quitting a violet to gather from a cowslip; which he says is not justly founded; for he has observed frequently a bee on a large border gathering from flowers of different species. If M. Reaumur only means that, when the bee gathers honey, it takes it indifferently from any flower, I can say nothing against it; but, if he intends it to mean the bee's loading the farina on its legs, then my observation directly contradicts it.

What further confirms my observation is this, that each load on the legs of a bee, is of one uniform colour throughout, as a light red, an orange, a yellow, a white, or a green, and is not on different parts of the load of a different colour; so that as the farina of each species of flowers, when collected together, is of one uniform colour, the presumption is, that it is gathered from one species. For, if from different kinds, part of the load might be of one colour, and part of another.

Another observation to confirm the same fact is, that bees, in the height of the season, return to their hives with loads of very different magnitudes, some having loads as large as small shot, while others have very small loads; it cannot be conceived that this difference is from the inactivity or sloth of the bee in collecting its load, but rather from the scarcity of the flowers, on which it first began to load.

Now, if the facts be so, and my observations true, I think that Providence has appointed the bee to be very instrumental in promoting the increase of vegetables; but otherwise, might be very detrimental to their propagation; and at the same time they contribute to the health and life of their own species.

From the late improvement made by glasses, and experiments made, in observing the works of nature, it is almost demonstrable, that the farina on the apices of flowers, is the male seed; which entering the pistillum or matrix in the flower, impregnates the ovum, and makes it prolific. It is often necessary to have wind and dry weather to waft this farina to the pistillum, and from flower to flower, to make the seed prolific: and we find in wet seasons, that grain, nuts, and fruit, are less prolific, by the farina's not being properly conveyed to the pistillum; and also in very hot dry weather, from clammy honey-dews, or, more properly sweet exudations from the plants themselves, which clog the farina, and cause blasts and mildews. Now if the farina of specifically different flowers should take the place of its own proper farina in the pistillum, like an unnatural coition in the animal world, either no generation would happen, or a monstrous one, or an individual not capable of further generation.

Now if the bee is appointed by Providence to go only, at each loading, to flowers of the same species, as the abundant farina often covers the whole bee, as well as what it loads on its legs, it carries the farina from flower to flower, and by its walking on the pistillum and agitation of its wings, it contributes greatly to the farina's entering into the pistillum, and at the same time prevents the heterogeneous mixture of the farina of different flowers with it; which, if it strayed from flower to flower at random, it would carry to flowers of a different species.

Besides these visible advantages, it may be of great benefit to their own species and society; for, as this farina is the natural and constant food of the bees, during one half of the year, and from this digested, as accurately observed by M. Reaumur, is the bouillée and jelly formed; which is lodged for the food of the young bees, till they become nymphæ: it is also necessary that its stores should be lodged in the cells adjoining to the honey, for their winter provision; without which, M. Reaumur observes, they would be in danger of dying of looseness, their most dangerous malady.

It seems therefore highly reasonable to believe, that different kinds of farina may have different physical qualities: so that, by making collections of the same kind in each cell, they may have proper remedies for themselves against ailments we have no knowledge of, which otherwise they would not have, if they were filled at random from all kinds of flowers. These further advantages, directed to them by Providence, seem to add weight to my observations, and are a presumptive proof that they are true.

The only thing, besides the former, in which my observations differ from M. Reaumur, is in the manner the wax is made and emitted by the bee. I absolutely concur with him, that the wax is formed by digestion in the bodies of the bees, and is emitted by them, and then becomes wax; and that it is almost impracticable to form wax any other way, unless the wax extracted from the myrtle-berries in America by boiling be an exception from it. By M. Reaumur's observations, he forms his opinion, that after the bee has fed upon the farina, or bee-bread, and it has passed through the first stomach (which is the reservoir where the honey is lodged, from whence it is discharged upward by its mouth into the cells) it is conveyed into the second stomach; and yet, when there, great part of it continues in its spherical or oval form, still undigested, as viewed by him with his glasses; and consequently must be conveyed further, before it be thoroughly digested, and the particles broken; yet this he supposes is reconveyed upwards through both the stomachs, and is emitted by its mouth; and he forms his judgment from his observation, that the bee, when working, and finishing the cells, nips with its teeth the wax, where it is too thick, or wrong laid; and has observed a motion of its tongue as it were smoothing or laying on more materials, which he thinks must be then discharged from the stomach by its mouth.

What makes me disagree from him in his opinion and observations, is from the remarks I have made, that the fæces of the bee discharged by the anus, after the farina is digested, is the true wax. We may with truth believe, that the farina, which is the male seed of all vegetables, consists of a spirit or moving principle, floating in a sweet oil, surrounded by an exterior coat or shell, in which is that monade that impregnates the grain or fruit, and makes it prolific; that on separation or digestion, this spirit and sweet oil becomes the nourishment of the bee; which spirit is of the same nature with the animalcules in semine masculino of animals, and becomes the animal spirits in the bee and other animals; and perhaps the true honey is the sweet oil included in the farina: and as all vegetables abound with these vegetable vivifying atoms, since from many every bud is capable of increasing each species, so the true honey breaking through its shell by great heat, occasions those honey-dews observed in hot weather on the leaves and flowers of most vegetables; which is no more than an exudation from the leaves and blossoms of these vessels that break with the heat;

besides those that appear on the apices of flowers, which afterwards impregnates the fruit.

Of this inner substance of the farina, diluted with water, after digestion, is formed the bouillée and jelly, which the bees discharge upward by the mouth, into the cells, to nourish the young bees till they become nymphæ; while the husk or outer coat is discharged by the anus, and becomes the genuine wax. I have frequently, when bees have been swarming, had them alight on my hands and cloaths; and many, at different times, have discharged their fæces on them: this I have taken off, and found it of the consistence of warm wax, with the same glutinous adhering quality, not crumbling like the farina. I have also distinguished it by the smell to be wax; but it had a heavier stronger smell, as it was fresh and warm from the bee.

What further confirmed me in this fact, was from my observation of the bees when working up their comb in a glass hive; where I have constantly seen (and must believe it impossible not to be observed by so accurate an observer as M. Reaumur) that several bees, soon after one another, have by hasty steps, walked along a comb then forming, for the length of 2 or 3 cells, bending their tails to the comb, and striking it with a wriggling motion from side to side, in a zigzag way; which I was convinced was discharging their fæces, or the wax, against the border of the cells, as they ran along, and repeated it as long as they had any to discharge, and then quit it; which is the reason why the outer border of the cells is so thick and strong: immediately afterwards, other bees came along the cells, and with their fore feet raised up the borders like paste, and thinning it, while other bees were ripping off with their teeth, and pruning away any irregular excrescences, so as to make the divisions of the cells vastly thinner than the borders or edges, which were always thick and strong, from the discharging the fæces or wax upon them.

M. Reaumur has very justly observed that, besides the 3 transparent smooth eyes which the bee has placed in a triangle between the antennæ on the top of its head, the bee has also on each side of its head and eye, or rather a multitude of eyes, formed by a number of distinct lenses each surrounded with short hairs, which are confirmed to be eyes, both from Swammerdam, and his own experiments to determine it; and that, notwithstanding these lenses are lined with a dark opaque substance, yet they assist so much their vision, that when darkened by paint laid over them, the bees could not find their way to their hive, though at a small distance, but soared directly upwards; nor could they find their way when the 3 smooth eyes were darkened.

But there is one observation, which I don't find he has made, which may have determined the garden bees to make almost all their cells imperfect hexagons. The observation is this; that these opaque eyes on each side of the head, consist of many lenses, each of which is a perfect hexagon; and the whole eye, when viewed in a microscope, appears exactly like a honeycomb: now as the eyes, composed of these hexagonal lenses, are in full view to the other bees, does it not seem that Providence has directed them so as to be a pattern set before them, for the bees to follow in forming their combs? It is not also reasonable to believe, from the disproportion of the convexity between the 3 smooth transparent eyes, and the lenses of the dark rough eyes, that they are appointed for different purposes? Why may it not be thought that the lenses are great magnifiers to view things near at hand, and by many reflections to convey light into the dark of hives, where light is still necessary; and that the 3 other eyes are to observe objects at a great distance, so as to conduct them abroad to fields at a distance, and back again to their hives?

I agree with M. Reaumur in the form and use of the fang or tromp of the working bee, and of the use of the mouth within the teeth of the bee; so that it does not suck, but laps or licks with its rough fang or tromp, like a dog. But I have never observed the bee nipping or breaking open the apices of flowers, to let out the farina, when it is not fully blown or open; but have often with pleasure observed the bee gathering the farina on its fang, by licking it off the apices, and laying it on the first pair of legs, which convey it to the second pair, and these lodge it on the pallet of the third pair, with surprizing briskness; so that, by the time the second pair has lodged it on the third pair, the bee has gathered more, and lodged it on the fore legs; so that all are in constant motion.

From the curious observations made by M. Reaumur, on the structure and behaviour of the queen or mother bee, the drone or male bee, and the working or mule bee, which is of neither sex; from the queen bee's being so exceedingly prolific, as to lay from 30 to 40000 eggs of working bees in a season; besides the eggs of 800 male bees, and of 8 or 10 queen or mother bees; and from the coldness of the male bee who so long resists the caresses of the queen or female bee; and also from the indefatigable labour and economy of the working bee, to nourish the young bees, make up the combs, and lay in stores of farina and honey for winter; I think very good reasons may be given why the queen should have a seraglio of some hundreds of male bees; and why the working bee should destroy the males, when no longer necessary to impregnate the eggs of the mother bee.

It is evident, from the economy of the garden bee, that Providence has appointed that they should share their store with mankind, by making them so industrious in every climate, as to provide, in tolerable seasons, a store of honey and wax, double of what is necessary for their subsistence during the winter, and of combs for the queen's laying her eggs in spring, before new work can be made.

From the vast number of eggs which the queen lays in a season, it is absolutely necessary that she should have a great store of male sperm, to impregnate her eggs; and as the eggs are not sensibly large in her body for 6 months after her coition with the males, who die, or are killed, in August, and she does not begin to lay from that time till February or March; it is therefore necessary that she should have a great store of male sperm within her, to impregnate all the eggs she lays from that time, till June or July, when young drones or males are hatched, who are not designed for her use, but for the young queens, who go off with the swarms, or for the young queen who succeeds the old one in the old hive; since the drones are great feeders, and no workers; and are of no use, but to give a sufficient store of sperm to the mother bee; as the working bees have so many enemies to deprive them of their store, they cannot be maintained during the winter, even if their life should last so long; and as it is probable that each male has but one act of coition with the queen, as they are so cold, and take so much caressing before they act, and, by M. Reaumur's observation, die soon after the act is over, when probably their whole store of sperm is exhausted in that act, as soon as the queen has got as much sperm lodged in the proper reservoir, as is sufficient to impregnate all her future eggs, the males are no longer of use; and if those who have acted die, those who have not, being of no further use, are killed by the working bee, out of economy to save their winter store, when probably by nature they could live but few days more; as we find the silk-worm moth dies soon after the eggs are laid, as well males as females. It seems therefore necessary that the queen should breed so many males as, by one act of coition from each, may impregnate all her eggs, and that the working bee should dispatch them, as soon as that is over, and a store is lodged.

There are 2 vessels described by Swammerdam in the mother bee; one of which is placed between the two lobes of the ovarium, which he supposes to be a bladder to contain air; the other is a spherical vessel, seated close by the common duct, in which the eggs fall from the lobes of the ovarium, which he supposes is to ooze out a juice to moisten the eggs in their passage. I take one of these, but most probably the last, to be the reservoir and repository of the male sperm, wherein it is lodged from the act of coition, till the eggs are enlarged, and pass through the adjoining duct from the 2 lobes of the ovarium.

Since the preservation and increase of bees are evidently beneficial to the public, I approve very much of M. Reaumur's instructions in driving bees from a full hive into an empty one, in case it can be done time enough to have new work, sufficient for the queen to lay her eggs in in spring; since they can be fed at very little expence, if care be taken to keep them in a middle state of stupefaction, neither too hot nor cold, during the winter: but I approve much more of his castrating or sharing the combs with the bees, by taking the combs best stored with honey, and leaving those having the nymphæ and bee-bread; but think, in taking the combs, a safer and easier way may be taken, than he directs: his method is to stupefy the bees with smoke, to oblige them to croud together in the crown of the hive, and then turning up the hive, and cutting out the combs filled with honey. Now I think, that turning up the full hive, and setting an empty hive upon it, and driving the bees into it, is preferable to smoking; for then a very few bees will remain in the full hive, and those few may be stupefied, and the bees in the empty hive being put on a table, the combs may be taken out and selected at leisure, without hazard; and afterwards the empty hive may be turned up, and their old hive set over them, so that they will go up without scruple into their former hive, and repair their work, by making new combs: and if the queen had not quitted the old hive, as is often the case, then they would return to their queen, and the society would not be lost, as is sometimes the case, in driving into an empty hive.

Further Experiments on Substances Resisting Putrefaction; with Experiments on the Means of Hastening and Promoting it. By John Pringle, M. D., F. R. S. N° 496, p. 550.

1. Decoctions of wormwood and of the bark, also infusions of camomile-flowers, and of snake-root, these preserved yolks of eggs, not only several days longer than water did alone, but also when a good quantity of sea-salt was added to it. Dr. P. likewise found that salt of hartshorn preserved this substance better than 4 times its weight of sea-salt.

2. Ox's gall was kept some time from putrefaction by small quantities of ley of Tartar, spirit of hartshorn, crude sal ammoniac, and the saline mixture, and still longer by a decoction of wormwood, infusions of camomile-flowers, and of snake-root; by solutions of myrrh, camphor, and salt of amber; all were separately mixed with gall, and found more antiseptic than sea salt; and seemingly in proportion to their effects on flesh. Only nitre failed; which, though 4 times stronger than sea salt in keeping flesh sweet, is inferior to it in preserving gall, and remarkably weaker than crude sal ammoniac: which again is somewhat less powerful than nitre in preserving flesh. The nitre was soon opened by the gall, and emitted a vast quantity of air, which rose as from a fermenting liquor: and when this happened, the gall began to putrify. But the saline mixture generated no air, and opposed the putrefaction of gall more than it did that of flesh.

3. The last trial was with the serum of human blood, which was preserved by a decoction of the bark, and an infusion of snake-root, nor with less efficacy than flesh. But saffron and camphor were not here above a 4th part so antiseptic

as before; whether it be that they are less preservative of this humour, or, as he suspects, that they were not well mixed. Nitre acted nearly with its full force, being about 4 times stronger than sea-salt; it generated some air, but much less than it did with the gall. No other humour was tried; but from these specimens, added to the former experiments, we may conclude, that whatever is preservative of flesh, will be generally antiseptic, though perhaps not always with equal force.

4. Having already shown how putrid flesh might be sweetened, he concludes this part of his subject with a like trial on the yolk of an egg. A portion of this, being diluted with water, stood till it corrupted; when a few drops were put into a phial with 2 ounces of pure water, and about twice as many drops were mixed with a strong infusion of camomile-flowers. At first both phials had some degree of a putrid smell; but being corked, and kept a few days near a fire, the mixture with plain water contracted a strong fetor, while the other smelled only of the flowers.

Thus far he has related the experiments made with antiseptics; by which it appears, that besides spirits, acids, and salts, we are possessed of many powerful resisters of putrefaction, endued with qualities of heating, cooling, volatility, astriction, and the like, which make some more adapted than others to particular indications. In some putrid cases, many proper antiseptics are already known: in others they are wanting. We are yet at a loss how to correct the sanies of a cancerous ulcer; but from such a multitude of antiseptics, it is to be hoped some may be found at last adequate to that intention. It may be further remarked, that as different distempers of the putrid kind require different antiseptics, so the same disease will not always yield to the same medicine. Thus the bark will fail in a gangrene, if the vessels are too full, or the blood sizy; but if the vessels are relaxed, and the blood resolved or disposed to putrefaction, either from a bad habit, or the absorption of putrid matter, then is the bark a good specific. With the same caution are we to use it in wounds, viz. chiefly in cases of absorbed matter, which infects the humours, and induces a hectic fever. But when inflammatory symptoms prevail, the same medicine increasing the tension of the fibres, and siziness of the blood, a state directly opposed to the other has such consequences as might be expected.

By the success of the bark in so many putrid cases, it should appear that astriction had no small share in the cure. And indeed the very nature of putrefaction consists in a separation or disunion of the parts. But as there are other cases, in which astringency is less wanted, we may find in contrayerva-root, snake-root, camphor, and other substances, a highly antiseptic power, with little or none of the other quality. And since several of these medicines are also diaphoretic, their operation is thereby rendered more successful.

Dr. P. comes now to the last thing proposed, which is, to give an account of some observations made on substances hastening or promoting putrefaction; an inquiry not less useful than the former. For, setting aside the offensive idea commonly annexed to the word, we must acknowledge putrefaction to be one of the instruments of nature, by which many great and curious changes are brought about. With regard to medicine, we know that neither animal nor vegetable substances can become aliment, without undergoing some degree of putrefaction. Many distempers proceed from a deficiency of this action. The crises of fevers seem to depend on it; and perhaps even animal heat, according to a late ingenious theory.*

But, in the prosecution of this subject, he had met with very few real septics; and found many substances, commonly accounted such, of a quite opposite nature. The most general means of accelerating putrefaction is by heat, moisture, and stagnating air; which being sufficiently known and ascertained, he passed over, without making any particular experiment on those heads. Lord Bacon, (vide Nat. Hist. cent. iv. exper. 330,) as well as some of the chemists, has hinted at a putrid fermentation, analogous to what is found in vegetables; and this having so near a connection with contagion, Dr. P. made the following experiment, for a further illustration of this matter.

5. In the yolk of an egg, already putrid, a small thread was dipped, and a small bit of this was cut off and put into a phial, with half of the yolk of a new-laid egg diluted with water. The other half, with as much water, was put into another phial, and both being corked, were set by the fire to putrefy. The result was, that the thread infected the fresh yolk, for the putrefaction was sooner perceived in the phial that contained it, than in the other. But this experiment was not repeated.

In this manner the putrefaction of meat advances quicker in a confined than a free air; for as the most putrid parts are also the most fugitive; they incessantly issue from a corruptible substance, and disperse with the wind; but in a stagnation of air, they remain about the body; and by way of ferment excite it to corruption.

6. As for other septics, recited by authors, Dr. P. found none of them answer the purpose. The alkaline salts have been considered as the chief putrefiers. But this is disproved by experiments. Of the volatiles it may be indeed observed, that though they preserve from the common marks of putrefaction, with a force 4 times greater than that of sea-salt; yet, in warm infusions, a small quantity of these salts will soften and resolve the fibres, more than water does by itself. They also hinder the coagulation of blood; and when taken by way of medicine,

* An Essay on the Cause of Animal Heat, by J. Stevenson, M. D. Vide Medical Essays, vol. v. —Orig.

thin and resolve it, but are not therefore septics. For so little do these salts putrefy, or even resolve the fibres, when applied dry, that he had kept, since the beginning of June last, notwithstanding the excessive heats, a small piece of flesh in a phial, preserved only with salt of hartshorn, at present perfectly sound, and firmer than when first salted.

7. From the specimens he had of the antiscorbutic plants, it is likewise probable that none of that tribe will prove septic. Horse-raddish, one of the most acrid, is a very powerful antiseptic. And though carrots, turnips, garlick, onions, celery, cabbage, and colewort, were tried, as alcalescents, they did not hasten, but somewhat retarded the putrefaction.

8. The case was different with such farinaceous vegetables as were examined, viz. white bread in infusion, decoctions of flour, barley, and oatmeal; for these did not at all retard putrefaction: but, after it was somewhat advanced, they checked it by turning sour. By a long digestion the acidity became considerable; which, by conquering the putrescency of the flesh, and generating much air, did not ill represent the state of weak bowels, which convert bread, and the mildest grains, to such an acid, as prevents a due resolution and digestion of animal food.*

9. He examined cantharides, dried vipers, and Russian castor, all animal substances, and therefore most likely to prove septic. The flies were tried both with fresh beef, and with the serum of human blood; the vipers only with the former: but neither of them hastened putrefaction. And as for the castor, so far from promoting this process, that an infusion of 12 grs. opposed it more than the standard salt.

10. After finding no septics where they were most expected, he discovered some which seemed the least likely; viz. chalk, the testacea, and common salt. 20 grs. of crabs'-eyes prepared, were mixed with 6 drs. of ox's gall, and as much water, into another phial was put nothing but gall and water, in the same quantity with the former; and both being placed in the furnace, the putrefaction began much sooner where the powder was than in the other phial. He infused afterwards in the lamp furnace 30 grs. of prepared chalk, with the usual quantity of flesh and water; and observed, that the corruption not only began sooner, but went higher by this mixture; nay, what had never happened before, that in a few days the flesh resolved into a perfect mucus. The experiment was repeated with the same effect; which being so extraordinary, he suspected some corrosive substance had been mixed with the powder; but, for a trial, a lump of chalk

* It is to be remarked that, in making this experiment, Dr. P. did not then attend to a fermentation that ensued, and which was the cause of the acidity. This kind of fermentation between animal and vegetable substances, being hitherto overlooked, is set forth in the next paper.—Orig.

being pounded, 30 grs. of it proved fully as septic as the former. The same powder was compared with an equal quantity of salt of wormwood, and care was taken to shake both the mixtures alike: but after 3 days warm digestion, the salt had neither tainted nor softened the flesh, while the chalk had rotted and consumed that which was joined to it. Nor were the effects less of the testaceous powders of the dispensary. Egg-shells in water resisted putrefaction, and preserved the meat longer firm than plain water.*

11. To try whether the testacea would also dissolve vegetable substances, he infused them with barley and water, and compared this mixture with another of barley and water, without the testacea. After a long maceration by a fire, the plain water swelled the barley, became mucilaginous and sour; but that with the powder kept the grain to its natural size, though it softened it, made no mucilage, and remained sweet.

12. Nothing could be more unexpected than to find sea-salt a hastener of putrefaction. But the fact is thus: 1 dr. of salt preserves 2 drs. of fresh beef, in 2 oz. of water, above 30 hours, uncorrupted, in a heat equal to that of the human body; or, what amounts to the same, this quantity of salt keeps flesh about 20 hours longer sweet than pure water; but $\frac{1}{2}$ dr. of salt does not preserve it above 2 hours longer. This experiment has been already mentioned. Now he afterwards found, that 25 grs. have little or no antiseptic virtue; and that 10, or 15, or even 20 grs. manifestly both hasten and heighten the corruption.† It is moreover to be remarked, that in warm infusions with these smaller quantities, the salt, instead of hardening the flesh, as it does in a dry form, in brine, or even in solutions, such as our standard, it here softens and relaxes the texture of the meat, more than plain water, though much less than water with chalk, or the testaceous powders.

Many inferences might be made from this experiment: but he only mentions one. Salt, the indispensable seasoner of animal food, has been supposed to act by an antiseptic quality, correcting the too great tendency of meats to putrefaction. But, since it is never taken in aliment beyond the proportion of the corrupting quantities in these experiments, it would appear that salt is subservient to digestion, chiefly by a septic virtue; that is, by softening and resolving meats; an action very different from what is commonly believed.‡

* The trial was made with a coarse powder of this substance, but not repeated.—Orig.

† The most putrefying quantity of salt, with this proportion of salt and water, is about 10 grs. —Orig.

‡ According to later physiologists, salt in small quantities proves subservient to digestion, by its stimulant action on the stomach; in the same manner as spices and other kinds of seasoning do.

On the late Dr. Halley's Demonstration of the Analogy of the Logarithmic Tangents to the Meridian Line, or Sum of the Secants. By Mr. John Robertson, F.R.S.* N° 496, p. 559.

Dr. Halley, in this tract, N° 219, Philos. Trans. seems to have had 2 points chiefly in view, first, to prove, that the divisions of the meridian line in a Mercator's chart, were analogous to the logarithmic tangents of the half-complements of the latitudes. 2dly. To find a rule by which the tables of meridional parts might be computed from Brigg's, or the common logarithmic tangents. The former of these the Doctor has clearly and elegantly proved, but he has given rather too few steps to show as clearly the investigation of the latter.

Article 1. If the circumference of a circle be divided into any number of equal parts, by as many radii; and a line be drawn from the circumference cutting those radii, so that their parts intercepted between this line and the centre be in a continued decreasing geometric progression; then will that intersecting line be a curve, called the proportional spiral, and will intersect those radii at equal angles. This will be evident, by supposing the radii so near to each other, that the intercepted parts of the spiral may be taken as right lines; for then there will be a series of similar triangles, each having an equal angle at the centre, and the sides about those angles proportional.

2. The same things still supposed: the parts of the circumference of the circle reckoned from any one point, may be taken as the logarithms of the ratios be-

* John Robertson, F.R.S. the author of this paper, was born in the year 1712; and though he was at first placed out in a trade, yet he must soon have quitted it, as in the title of his first book, a Complete Treatise on Mensuration, in 1739, he is stiled teacher of the mathematics. In this line, as a private teacher, he continued several years, till in 1754 he was appointed master of the Royal Mathematical School in Christ's Hospital; in which year also he published the first edition of his Elements of Navigation. The year following however he left Christ's Hospital, in consequence of an Admiralty appointment to be first master of the Royal Naval Academy at Portsmouth; soon after which, he published his treatise on mathematical instruments. In 1766, through the petty cabals of the second master, he was dismissed from his situation by the first lord of the Admiralty; on which he returned to London, where, in the latter part of that year, or early in the next, he was appointed clerk and librarian to the Royal Society; an employment which he respectably held to the time of his death, in Dec. 1776, at 64 years of age.

Besides the three publications above-mentioned, which were all excellent of their kind, particularly the navigation, and have gone through numerous editions; he had many ingenious papers inserted in the Philos. Trans. from the 46th to the 60th volume. Mr. R. was a person of very honourable character and conduct, being greatly respected by the more learned and best characters among the members of the Royal Society; on most occasions his opinions in the council were much regarded; and he had the honour to be one of the committee chosen to inspect and report on the government's powder magazine at Purfleet, concerning its damage and security from lightning. In his mode of teaching, and arranging the materials in his publications, he was remarkably neat and methodical; a habit which he probably in some measure acquired in imitation of his good friend and master Wm. Jones, Esq. many of whose papers, on his demise, passed into the hands of Mr. Robertson, which were afterwards sold by auction, along with the valuable library of the latter, after his death.

tween the corresponding rays of the spiral.—For those rays are a series of terms in a continued geometric progression; and the parts of the circumference form a series of terms in arithmetic progression. Now the terms of the arithmetic series being taken as the exponents of the corresponding terms in the geometric series, there will be the same relation between each geometric term and its correlative, as between numbers and their logarithms. And hence the proportional spiral is also called the logarithmic spiral.

3. That proportional spiral, which intersects its radii at angles of 45 degrees, produces logarithms that are of Napier's kind.—For, if the difference between the first and second terms in the geometric series was indefinitely small, and the first division of the circumference was of the same magnitude; then may that part of the spiral, intercepted between the first and second radii, be taken as the diagonal of a square, two of whose sides are parts of those radii; therefore the spiral which cuts its rays at angles of 45°, has a kind of logarithms belonging to it, so related to their corresponding numbers, that the smallest variation between the first and second terms in the geometric series, is equal to the logarithm of the second term, a cypher being taken for the logarithm of the first. But of this kind are the hyperbolical logarithms, or those first made by their inventor the Lord Napier: consequently the logarithms to that spiral which cuts its rays at angles of 45°, are of the Napierian kind.

4. The rhumb-lines on the globe are analogous to the logarithmic spiral.—For every oblique rhumb cuts the meridian at equal angles; and it is a property in stereographic projections, that the lines in it intersecting each other, form angles equal to those which they represent on the sphere. Therefore a projection of the sphere being made on the plane of the equator, the meridians will become the radii of the equator, and the rhumbs intersecting them at equal angles, will become the proportional spiral. Hence, the arcs of the equator, or the differences of longitude reckoned from the same meridian, are as the logarithms of those parts of the corresponding meridians, intercepted between the centre and rhumb-line.

5. A sea chart being constructed, in which the meridians are parallel to each other, and the lengths of the degrees of latitude increase in the same proportion as the meridional distances decrease on the globes, will constitute a Mercator's chart, in which, besides the positions of places having the same proportions to each other, as on the globes, the rhumb lines will be represented by right lines.—For none but right lines can cut at equal angles several parallel right lines.

6. The divisions of the meridian line on a Mercator's chart, are the same as a table of the differences of longitude answering to each minute, or small difference of latitude on the rhumb line making angles of 45° with the meridians.—For, in such a chart, the parallels of latitude are equal to the equator, and are at right angles to the meridians; and therefore a rhumb of 45° cuts the meri-

dians and parallels of latitudes at equal angles; consequently between the intersection of any meridian and parallel, and a rhumb cutting them at 45°, there must be equal parts of the meridian and parallel intercepted; now, on the equator, or parallels of latitude, are reckoned all the successive differences of longitudes; and on the meridians the successive meridional differences of latitudes, or the divisions of the nautical meridian; therefore on the rhumb of 45°, the successive differences of longitude are equal to the corresponding divisions of the nautical meridian.

7. The tangents of the angles which different rhumbs make with the meridians, are directly proportional to the differences of longitudes made on those rhumbs, when the meridional differences of latitudes are equal; or, are reciprocally proportional to unequal meridional differences of latitudes on those rhumbs, when the differences of longitudes are equal.—For the meridional difference of latitude, is to the difference of longitude, as radius is to the tangent of the angle of the course, or of the angle which the rhumb makes with the meridian. Therefore, when the meridional differences of latitudes are equal, the differences of longitudes are as the tangents of the courses; but when the differences of longitudes are equal, the meridional differences of latitudes are reciprocally as the tangents of the courses.

8. The logarithmic tangents of the half-complements of the latitudes, are analogous to the lengthened degrees in the nautical meridian line, in a Mercator's chart.—For, in the stereographic projection of the sphere on the plane of the equator, the latitudes of places are projected by the half-tangents of the complements of those latitudes; which half-tangents are the rays of a proportional spiral. Now, if a series of successive latitudes be taken on any rhumb, the corresponding differences of longitudes will be logarithms to the rays of the spiral, or to the tangents of the half-complements of those latitudes; therefore the differences of longitudes are as the logarithmic tangents of the half-complements of the latitudes; but, art. 6, the lengthened degrees on the nautical meridian are as the differences of longitudes on the rhumb of 45°; consequently the logarithmic tangents of the half-complements of latitudes are as the lengthened degrees on the nautical meridian.

Corol. 1. When the angle between the rhumb line and the meridian is equal to 45°, then the longitudes of places on that rhumb are expressed by logarithms of Napier's kind; whose corresponding numbers are natural tangents of the half-complements of the latitudes to arcs expressed in parts of the radius.

Corol. 2. Hence, to any two places on a rhumb of 45°, the difference of longitude, or the meridional difference of latitude, is equal to the difference of the Napierian logarithmic tangents of the half-complements of the latitudes of those places, estimated in parts of the radius.

Corol. 3. As there may be an indefinite variety of rhumbs, and therefore as many different kinds of logarithms, consequently every species of logarithms has its peculiar rhumb, distinguishable by the angle it makes with the meridian: therefore, among these there are 2 kinds, to which the differences of longitudes are the differences of the logarithmic tangents of the half-complements of latitudes, estimated in minutes of a degree; one of them belonging to Napier's form of logarithmic tangents, and the other to Briggs's, or the common logarithmic tangents.

9. The common logarithmic tangents are a table of the differences of longitudes to every minute of latitude, on the rhumb line making angles with the meridians of 51° 38′ 9″.—For, let z represent the meridional difference of latitude between 2 places on the rhumb of 45°; or its equal, the difference between the logarithmic tangents of the half-complements of the latitudes of those places, estimated either in parts of the radius, or in minutes of a degree. Then, As the circumference in parts of the radius = 62831,853 &c : To the circumference in minutes of a degree = 21600 :: So is a meridional difference of latitude in parts of the radius = z : To a meridional difference of latitude in minutes of a degree, = 0,34377468 &c. × z.

Whose corresponding rhumb is different from that which z belonged to; and the angle which this rhumb makes with the meridian, will be found by the following analogy from art. 7.—As the meridional difference of latitude on one rhumb = 0,34377468 &c. z : To the meridional difference of latitude on a rhumb of 45°, = z :: So is the natural tangent of the rhumb of 45°, = 10000 : To the natural tangent of the other rhumb, = 29088,821 &c.

Which tangent answers to 71° 1′ 42″; and this is the angle that the rhumb line makes with the meridians, on which the differences of the logarithmic tangents of the half-complements of the latitudes, in Napier's form, are the true differences of longitudes estimated in sexagesimal parts of a degree. Now Napier's logarithms being to Briggs's, as 2,30258 &c. is to 1; therefore, 2,30258 &c. : 1 :: 29088,821 &c. : 12633,114 &c.; which is the tangent of 51° 38′ 9″; and in this angle are the meridians intersected by that rhumb, on which the differences of Briggs's logarithmic tangents of the half-complements of the latitudes are the true differences of longitudes corresponding to those latitudes.

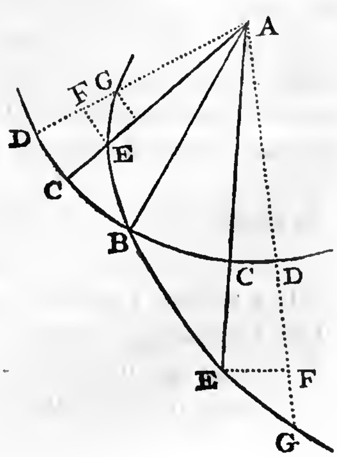
10. The difference between Briggs's logarithmic tangents of the half-complements of the latitudes of any two places, is to the meridional difference of latitude in minutes between those places, in the constant ratio of 1263,3 &c. to 1; or of 1 to 0,0007915704 &c.—For Briggs's logarithmic tangents are as the differences of longitudes on the rhumb (A) of 51° 38′ 9″; whose natural tangent is 1263,3 &c.

The nautical meridian is a scale of longitudes on the rhumb (B) of 45°, by art. 6, whose tangent being equal to the radius, may be expressed by unity. And the differences of longitude to equal differences of latitudes on different rhumbs, being to each other as the tangents of the angles those rhumbs make with the meridians. Therefore, as the tangent of A (51° 38′ 9″) = 1,2633 &c : To the tangent of B (45°) = 1,0000 : So is the difference of longitudes on A, or the difference between the logarithmic tangents of the half co-latitudes of two places: To the difference of longitudes on B, or the meridional difference of latitudes of those places.

And hence arise the rules which are given in nautical works, for finding the meridional parts by a table of common logarithmic tangents.

This curious discovery of Dr. Halley's, joined to that excellent thought of his, of delineating the lines, showing the variation of the compass, on the nautical chart, are some of the very few useful additions made to the art of navigation within the last 150 years; for if, beside these, we except the labours of that ingenious artist Mr. Richard Norwood, who improved the art by adding to it the manner of sailing in a current, and by finding the measure of a degree on a great circle, the theory of navigation will be found nearly in the same state in which it was left by that eminent mathematician Mr. Edward Wright; who, about the year 1600, published the principles on which the true nautical art is founded; and showed, what does not appear to have been known before, how to estimate a ship's true place at sea, as well in longitude as in latitude, by the use of a table of meridional parts, first made by himself, and constructed by the constant addition of the secants, and which differs almost insensibly from such a table made on Dr. Halley's principles, contained in the preceding articles.

Mr. R. concludes this discourse with an article which, though it be somewhat foreign to the preceding subject, yet, as it was discovered while he was contemplating some part of it, and perhaps is not exhibited in the same view by others, it is annexed in this place; which is, to demonstrate this common logarithmic property, that the fluxion of a number divided by that number, is equal to the fluxion of the Napierian logarithm of that number.



Let BEG be a logarithmic spiral, cutting its rays at angles of 45°: then, if AE be taken as a number, BC will be its Napierian or hyperbolic logarithm. Also, let CD express the fluxion of the logarithm BC; then the corresponding fluxion of the number AE, will be represented by FG, or its equal FE; as the angles FEG and FGE are equal. Now, AC : CD :: AE : (EF =) FG. Therefore $CD = \frac{FG}{AE} \times AB$. And if AB be taken as the unit or term from whence the numbers begin: then $CD = \frac{FG}{AE}$. Q. E. D.

Observation of the Total Lunar Eclipse, June 19, 1750, N.S. *at Wittemberg. By G. M. Bose, Prof. of Philos.* N° 496, p. 570.

The end of the shadow, by corrected time, as follows:

11^h	40^m	37^s	End according to Mr. Bose himself.
	45	17	.... according to a friend.
	40	30	.... by the projection of a friend.
	39	38	.... by the corrected calendar of Leipsic.
	39	11	.... by the Connoisance des temps.
	39	46	.... by the Ephemeris of Manfredi.

On the Heat of the Weather at Tooting, in July and September last. [1750] *By the Rev. Henry Miles, F.R.S., D.D.* N° 496, p. 571.

The morning at 4, July 11, had nothing remarkable: at 2 p.m. the heavens mostly clear, and no indications of a storm; the barometer having fallen but $\frac{6}{100}$ inch since 4 a.m. it then stood at 30, 20. the thermometer at 87$\frac{1}{2}$, and before 3 p.m. at 88$\frac{1}{4}$, which is the hottest temperature of the air he ever knew. At 4 p.m. was very distant thunder; soon after it came a little nearer, and was one continued murmur, without any perceivable intermission for great part of an hour: the lightning accompanying it, not much. The wind was nearly S.W. and dark clouds passed by on each side till they united in the N. forming one of the blackest clouds he ever saw, over the city, as near as he could guess. They had not one drop of rain, nor did there fall either rain or hail for near 3 miles to the N. of the place towards London: a few hail-stones it seems fell in some parts of Clapham.

The barometer fell little, and the thermometer no more than usual at that time of the evening. Mr. Canton writes that his thermometer in Spital square (of the same construction, and kept too in the open air) fell no less than 17 degrees.

At 4 a.m. Sept. 2, the wind being easterly, and blowing strong, accompanied with several short showers of rain, the barometer being at 29.97, the thermometer abroad stood at 61: a degree of heat exceeding any he had taken notice of during the whole summer at that time of the morning.

On the Hot Weather in July 1750, *dated Norwich July* 23. *By Mr. Wm. Arderon, F.R.S.* N° 496, p. 573.

For 12 days past, the weather was at Norwich the most excessive ever known. The beginning of this heat was on July 8th; when, though the whole day was cloudy, the ground was so uncommonly hot, that Mr. A. could not bear to walk on it long together without much uneasiness; and many others were sensible of the same inconvenience. On the 11th, which was the hottest day of all, the thermometer in the sun's rays stood 11° above the heat of human blood; and in the shade only 8° below it. The distance between freezing and the heat of human blood being divided into 100 parts. An inch of tallow, $\frac{6}{10}$ of an inch in diameter, liquefied in the sun in less than 30 minutes. A piece of resin, $\frac{5}{10}$ of an inch in diameter, became so soft as to be liable to take any impression in the same time.

About 3 o'clock in the afternoon, when the sky is clear, is the hottest part of the day; but clouds mostly came on about that time on these days.

Several horses dropped down dead under their masters, overcome by this violent heat.

A Total Eclipse of the Moon, observed Dec. 2, 1750, in the Morning in the Strand, London, about 5s of Time West of St. Paul's, and 27s West of the Royal Observatory at Greenwich. By Dr. Bevis and Mr. James Short, F.R.S. N° 496, p. 575.

A sensible penumbra (Dec. 1) at	16^h	32^m	0^s
The eclipse judged to begin at	16	36	50
Total immersion at	17	36	5
The moon begins to emerge	19	14	33

The moon was now got so low, and day-light so far advanced, that no more phases could be observed with any degree of certainty. These observations were made with a reflecting telescope, that magnified 40 times, and a refracting telescope, which magnified 12 times, and the times were the same through these 2 telescopes; for the air was exceedingly clear, and the shadow well defined, the penumbra being scarcely sensible.

Here follows a computation, made from Dr. Halley's tables, by Mr. John Catlin, of Guy's hospital; and sent to Mr. Short the day before the eclipse.

Beginning of the moon's eclipse. (Dec. 1) ...	16^h	44^m	31^s
Immersion at	17	42	45
Emersion at	19	20	37
End at	20	18	51

Hence it appears, that the eclipse began about 8 minutes sooner than the computation from Dr. Halley's tables gave it; but the computation which Mr. Brent made and published some time before the eclipse happened, was within a minute of the time observed; and this exactness he imputes to his leaving out 3 of the 7 equations of the moon, published by Sir Isaac Newton in his theory of the moon.

An Account of some Experiments, made by Benjamin Robins, Esq. F.R.S., Mr. Samuel Da Costa, and several other Gentlemen, in order to discover the Height *to which Rockets may be made to ascend, and to what Distance their Light may be seen. By Mr. John Ellicott, F.R.S.* N° 496, p. 578.

Soon after the exhibition of the fire-works in the Green Park, on occasion of the late peace, Mr. Robins communicated to the Society an account of the height to which several of the rockets there fired were observed to rise. In this account, after having given a short description of the instrument with which the heights were measured, he observes, that the customary height to which the single or honorary rockets, as they are styled, ascended, was about 465 yards: that 3 of them rose to about 550 yards; and the greatest height of any of those fired in the grand girandole was about 600 yards. He further observed, that supposing rockets are made to ascend 600 yards, or more than a third of a mile, it follows, that if their light be sufficiently strong, and the air not hazy, they may be seen in a level country at above 50 miles distance; and that, from the nature of the composition, and the usual imperfect manner of forming them, he was of opinion that rockets were capable of being greatly improved, and made to reach much greater distances.

Mr. Robins not having been able to obtain any certain account to what distance any of these rockets were actually seen, and considering the great use that might be made of rockets in determining the position of distant places, and in giving signals for naval and military purposes, he resolved to order some rockets to be fired at an appointed time, and to desire some of his friends to look out for them at several very distant places. The places fixed on for this purpose, were, Godmarsham in Kent, about 50 miles distant from London; Beacon-hill on Tiptery-heath in Essex, at about 40 miles; and Barkway, on the borders of Hertfordshire, about 38 miles from London.

Mr. Robins accordingly ordered some rockets to be made by a person many years employed in the royal Laboratory at Woolwich; to which some gentlemen, who had been informed of Mr. Robins's intentions, added some others of their own making. The 27th of September, 1749, at 8 in the evening, was the time appointed for the firing of them; but, through the negligence of the engineer, they were not let off till above half an hour after the time agreed on. There were in all a dozen rockets fired from London-field at Hackney; and the heights were measured by Mr. Canton, Mr. Robins being present, at the distance of about 1200 yards from the post from whence the rockets were fired. The greatest part of them did not rise to above 400 yards; one to about 500, and one to 600 yards nearly.

A letter received the next day from the Rev. Dr. Mason, of Trinity college, Cambridge, who had undertaken to look out for them from Barkway on the borders of Hertfordshire, informed, that he plainly saw 4 rise, turn, and spread;

He judged they rose about one degree above the horizon, and that their lights were strong enough to have been seen much farther.

From Essex Mr. R. was informed, that the persons on Tiptery-heath saw 8 or 9 rockets very distinctly, at about half an hour past 8; and greatly to the eastward of these 5 or 6 more. The gentlemen from Godmarsham in Kent having waited till above half an hour past 8, without being able to discern any rockets, they fired half a dozen; which, from the bearings of the places, were most probably those seen to the eastward by the persons on Tiptery-heath; and if the situations, as laid down in the common maps are to be depended on, at about 35 miles distance.

The engineer being of opinion that he could make some rockets, of the same size as the former, that should rise much higher, Mr. Robins directed him to make half a dozen. These last were fired the 12th of October following, from the same place, and in general they rose nearly to the same heights with the foregoing; excepting one which was observed to rise 690 yards. The evening proved very hazy, which rendered it impossible for them to be seen to any considerable distance.

Among some rockets fired in the last spring, there were two made by Mr. da Costa of about 3½ inches diameter, which were observed to rise, the one to about 833, the other to 915 yards. At a second trial, made some time after, there was one made by Mr. da Costa, of 4 inches diameter, which rose to 1190 yards. The last trial was made the latter end of April 1750, when 28 rockets were fired in all, made by different persons, and of different sizes, from 1½ inch diameter to 4 inches; the most remarkable of each size were as follows: one of 1½ inch rose to 743 yards; one of 2 inches to 659; one of 2½ inches to 880; another of the same size, which rose to 1071; one of 3 inches to 1254; one of 3½ inches to 1109; and one of 2 inches, which, after having risen to near 700 yards, turned, and fell very near the ground before it went out. These were all made by Mr. da Costa. Besides these, there was one of the rockets of 2½ inches in diameter, which rose to 784 yards, and another made by Mr. Banks of the same size to 833.

After allowing for possible errors, it still appears certain that several of these rockets rose to 1000 yards, one to 1100, and another to 1200 yards, or double of any of those fired in the Green Park.

Several Papers concerning a new Semi-metal, called Platina. Communicated to the R.S. by Mr. Wm. Watson, F.R.S.* N° 496, p. 584.

* As this appears to have been the first printed account of this new metal, it has been judged to be due to the memory of those who communicated it to reprint it nearly in the original form.

Extract of a Letter from William Brownrigg, M.D., F. R. S. to Wm. Watson, F.R.S. Dated Whitehaven, Dec. 5, 1750. p. 584.

Dr. B. here communicates an account of a semi-metal called platina di Pinto; which, so far as he knew, had not been taken notice of by any writer on minerals. Presuming therefore that the subject was new, he requested the favour to have this account laid before the R.S. The experiments related were several of them made by a friend, whose exactness in performing them, and veracity in relating them, he could rely on: however, for greater certainty, he should himself repeat them.

Memoirs of a Semi-metal called Platina di Pinto, found in the Spanish West Indies, p. 585.

Though the history of minerals, and other fossil substances, has been diligently cultivated, especially by the moderns; yet it must be acknowledged, that among the vast variety of bodies which are the objects of that science, there still remains room for new inquiries.

Gold is usually esteemed the most ponderous of bodies; and yet he had seen, in the possession of the late professor Gravesande, a metalline substance, brought from the East Indies, that was specifically heavier than gold, by at least a 20th part.* Mercury, next to gold, is commonly said to be the heaviest body; yet mercury was greatly exceeded in specific gravity by a semi-metal † brought from the West Indies, of which he had presented specimens to the R.S. This semi-metal seems more particularly to deserve attention, as it is endued with some very singular qualities, which plainly demonstrate that certain general theorems, though long established, and universally received by the metallurgist, yet do not hold true in all cases, and ought not to be admitted into their arts, without proper limitations and restrictions. For instance, that gold and silver may be purified from all heterogeneous substances by coppellation, is a proposition that all assayers and refiners have long thought true and undeniable; yet this proposition ought not to be received by those artificers, without an exception to the semi-metal here treated of; since, like those nobler metals, it resists the power of fire, and the destructive force of lead in that operation.

This semi-metal was first presented to him (Dr. Brownrigg) about 9 years pre-

* This metalline substance, in the possession of Gravesande, though brought from the East Indies, (indirectly from commercial intercourse by the Spaniards with South America) was probably the very metal here treated of, viz. platina (now called platinum), of which the specific gravity in its purest state is 23.000, while that of gold is only 19.3.

† Wrongly termed a semi-metal, but at that time taken for such, as the means of reducing it to a reguline and malleable state were then unknown.

ceding the above date, by Mr. Charles Wood, a skilful and inquisitive metallurgist, who met with it in Jamaica, whither it had been brought from Carthagena in New Spain. And the same gentleman had gratified Dr. B.'s curiosity, by making further inquiries concerning this body. It is found in considerable quantities in the Spanish West Indies, and is there known by the name of platina di Pinto. The Spaniards probably call it platina, from the resemblance in colour that it bears to silver. It is bright and shining, and of a uniform texture; it takes a fine polish, and is not subject to tarnish or rust; it is extremely hard and compact; but like Bath-metal, or cast iron, brittle, and cannot be extended under the hammer.*

The Spaniards do not dig it in the form of ore, but find it in dust, or small grains, as were herewith presented to the R.S. Whether they gather it in a pretty pure state, as brought to us, or wash it, like gold-dust, from among sand, and other lighter substances, was to him unknown: however, it is seldom collected perfectly pure; since, among several parcels of it that he had seen, he constantly observed a large mixture of a shining black sand, such as is found on the shores of Virginia and Jamaica, which is a rich iron ore, and answers to the magnet. It has also usually mixed with it some few shining particles of a golden colour, which seem to be a substance of a different nature.

It is very probable that there is great plenty of this semi-metal in the Spanish West Indies; since trinkets made of it are there very common. A gentleman of Jamaica bought 5 lb. of it at Carthagena for less than its weight of silver; and it was formerly sold for a much lower price.

When exposed by itself to the fire, either in grains, or in larger pieces, it is of extreme difficult fusion; and has been kept for 2 hours in an air-furnace, in a heat that would run down cast iron in 15 minutes: which great heat it endured without being melted or wasted; neither could it be brought to fuse in this heat, by adding to it borax, and other saline fluxes. But the Spaniards have a way of melting it down, either alone, or by means of some flux; and cast it into sword-hilts, buckles, snuff-boxes, and other utensils.

When exposed to a proper degree of fire, with lead, silver, gold, copper, or tin, it readily melts, and incorporates with these metals; rendering the mixture, like itself, extremely hard and brittle.

Having been melted in an assay-furnace, on a test with lead, and with it exposed to a great fire for 3 hours, till all the lead was wrought off, the platina was afterwards found remaining at the bottom of the test, without having suffered any alteration or diminution by this operation.

A piece of platina was put into strong and pure aqua fortis, and with it placed

* Not in its native or crude state; but when properly purified, it may be extended under the hammer.

in a sand-heat for 12 hours: the platina, when taken out of the aqua fortis, was found of the same weight as when put into it; being in nowise dissolved or corroded by that menstruum.

It had been reported, that this semi-metal was specifically heavier than gold; * but having weighed several pieces of it hydrostatically in a nice assay-balance, he found one of these pieces was to that of water exactly as 15 to 1. Another piece, that seemed to be cast very open and porous, he found in gravity to water only as 13.91 to 1: though this last-mentioned piece, could it have endured the hammer as well as gold, might probably have been reduced to a considerably greater degree of solidity than that of the first-mentioned specimen. For the purest gold is seldom found, after fusion, to come up to its true specific weight, till it has been brought up to its greatest degree of solidity under the hammer.

He also weighed an equal mixture of gold and platina, which he found nearly as ponderous as gold itself; the specific weight of this mixture being to that of water as 19 to 1.

It had been reported, that the Spaniards had sometimes been tempted to adulterate gold with platina, as the mixture could not be distinguished from true gold by all the ordinary trials: but the gold thus adulterated was, on a nicer examination, found hard and brittle, and could not be separated from the platina, and rendered ductile and pure, either by cementation, or by the more ordinary operations with lead and antimony. In order therefore to prevent this fraud, the king of Spain commanded that the mines of platina should be stopped up; so that this semi-metal is now much scarcer than formerly.

From the foregoing account it appears, that no known body approaches nearer to the nature of gold, in its most essential properties of fixedness and solidity, than the semi-metal here treated of; and that it also bears a great resemblance to gold in other particulars. Some alchemists have thought that gold differed from other metals in nothing so much as in its specific gravity; and that, if they could obtain a body that had the specific weight of gold, they could easily give it all the other qualities of that metal. Let them try their art on this body; which, if it can be made as ductile as gold, will not easily be distinguished from gold itself.

On the whole, this semi-metal seems a very singular body, that merits an exacter inquiry into its nature than has yet been made; since it is not altogether improbable that, like the magnet, iron, antimony, mercury, and other metallic substances, it may be endowed with some peculiar qualities, that may render it of singular use and importance to mankind.

* When thoroughly purified, it is specifically heavier than gold, weighing 23.000; whereas the specific gravity of gold is only 19.3.

The 3d communication on this interesting subject is from Mr. Watson, who says, that this platina di Pinto is likewise called in America, Juan Blanco. It is not mentioned in any author he has met with, except Don Antonio d'Ulloa, who, in the History of his Voyage to South America, vol. ii. b. 6, ch. 10, which he has here extracted, and translated from the Spanish, when giving an account of the gold and silver mines in the province of Quito, and of the various methods of separating these metals from other substances, with which they are combined, says, that "in the territory of Choco —— there are gold mines, in which that metal is so disguised and enveloped with other mineral substances, juices, and stones, that, for their separation from the gold, they are obliged to use quicksilver. Sometimes they find mineral substances, which, from their being mixed with platina, they chuse to neglect. This platina is a stone (piedra) of such resistance, that it is not easily broken by a blow on an anvil. It is not subdued by calcination; and it is very difficult to extract the metal it contains even with much labour and expence."

In the before-mentioned work, ch. 11, the same author, when speaking of the remaining works of the Indians of old, says, "the specula wrought out of stones, which are found in the places of worship of the Indians, are of 2 kinds, in regard to the matter of which they are made: one of these is called piedra de Inga, the other piedra de Gallinazo. The first of these is smooth, of a leaden colour, and not transparent; they are usually found wrought of a circular figure: one of the surfaces is plain, and as smooth as though it were made of a kind of crystal; the other surface is oval, or rather somewhat spherical, and not so much burnished as the plain one. Though they vary in their size, they are commonly from 3 to 4 inches in diameter; but he has seen one that was a foot and a half in diameter. Its principal surface was concave, and much augmented the size of objects, for its polish was in as great perfection as though it had been worked by a dextrous artist in these times."

"This stone has certain veins, or hair-like appearances, on its surface; by which it is rendered less fit for a speculum, and is apt to break in these veins in receiving any blow. Many are persuaded, or at least suspect, that the matter of these is a cast composition; and though there are some appearances of this being so, they are not sufficiently convincing. In this country there are gullies (quebradas) where the mineral of them is found rough, and from whence some are always taken; but these are not now wrought for those purposes for which heretofore they were employed by the Indians: but this is no reason but that some of them may have been cast, as with the same material taken out of the mine, they may have been made artificially, and thereby have received a greater degree of perfection, as well in their quality as in their figure." He says further, "that though at present these, as well as several other things found there, are but of small value, yet they are extremely curious, and worthy to be esteemed, as well for their great antiquity, as for their being the performances of those barbarous people."

Some of these piedras de Inga Mr. W. laid before the Society, both in their rough and in their polished state. They were brought hither with several other curiosities from America, by Don Pedro Maldonado, and were presented by him to the president, who was pleased to put them into his hands. They are doubtless of a metalline substance, and have, in his opinion, evident marks of having been fused and cast. They very much resemble, as will be seen by comparing them, the platina before mentioned: and though they are called (piedras) stones by Don Antonio d'Ulloa, he likewise gives the same appellation to the platina. He cannot therefore help recommending to some curious metallurgist of the Society to make the experiment, whether, when the piedras de Inga are, by a proper process, divested of their stony and other heterogeneous parts, the metalline residuum will not resemble, as well in specific gravity, as in other properties, the purified platina.

The fourth communication on this subject is from M. da Costa, who states that in Jan. 1742-3, there were brought from Jamaica, in a Man of War, several bars (as thought) of gold, consigned from different merchants of that island, to their different correspondents here, as bars of gold. These bars had the same specific gravity, or rather more than gold, and were exactly like that metal in colour, grain, &c. A piece of one of these counterfeit bars was sent to the mint to be tested, and it was found to be 21 carats 3 grs. worse than standard.

The 5th communication is an extract of a letter from Wm. Brownrigg, M.D., F.R.S. to Wm. Watson, F.R.S., containing some further experiments on the platina. Dated Whitehaven, Feb. 13, 1750. Wherein he thanks Mr. Watson for his trouble in presenting his specimen of platina to the R.S., together with his memoir relating to it; and he further thanks him for the addition made to it of the extract from Don d'Ulloa's Voyage.

The gentleman, whose experiments on platina Dr. B. mentioned to the R.S., was Mr. Charles Wood, who permitted him to make what use of them he pleased; and he did not pretend to have made any new discovery, nor to know so much of that body, as had long been known to the Spaniards.

The chief thing about which he had any difficulty, was what had been asserted of the platina's resisting the force of lead in coppellation. This experiment he had tried, therefore, by adding to 26 grs. of platina, 16 times its weight of pure lead, that he had reduced from litharge. To the lead put into a coppel, and placed in a proper furnace, as soon as it was melted he added the platina, which in a short time was dissolved in the lead. After the lead was all wrought off, there remained at the bottom of the coppel a pellet of platina, which he found to weigh only 21 grs.; so that, in this operation, the platina had lost near a 5th part of its weight.

According therefore to this experiment, the platina does not wholly resist the force of lead in coppellation; but, by repeated operations of that kind with larger quantities of lead, may probably all be destroyed: and by such repeated coppellations, gold and silver may very likely be refined from it; though what was before asserted may hold pretty true, with regard to the common coppellations of the assayers and refiners.

Mr. Wood said, that, in his experiment, he thought the platina rather gained than lost in weight by coppellation. This might happen from some small mixture of lead, or other metal continuing with it after it remained no longer fused.

From this single experiment Dr. B. would not be quite positive that lead thus consumes some small quantity of platina, since it was possible the platina used might not be pure. Besides, in order to keep it longer in fusion, he urged on the experiment with an uncommon degree of heat, especially towards the end of the operation; though he thought no great error could thence arise; as $\frac{1}{4}$ dr. of silver, which he coppelled at the same time, had lost only 2 grs. in the operation.

He was told that one Mr. Ord, formerly a factor to the South Sea Company, took in payment from some Spaniards, gold to the value of 500l. sterling, which being mixed with platina, was so brittle that he could not dispose of it, neither could he get it refined in London, so that it was quite useless to him; though, if no error has been committed in the above-mentioned experiments, it might probably have been rendered pure by a much larger dose of lead than is usually employed for that purpose.

To his memoir he might have added, that attempting to cleanse a parcel of the native platina from the black sand, with which it was mixed, he found that a great many of its grains were attracted by the magnet he made use of for that purpose. This circumstance he took notice of in a letter to Lord Lonsdale two years before.*

Of a very Large Human Calculus. By William Heberden, M. D.† F. R. S. N° 496, p. 596.

There is preserved in the library of Trinity College, Cambridge, a stone taken

* A most ingenious and complete set of experiments was made on platina between 3 and 4 years afterwards by Dr. Lewis. These experiments are inserted in the 48th and 50th vols. of the Phil. Trans. Since then it has occupied the attention of the first chemists in this and other countries; and lately Dr. Wollaston (Phil. Trans. for 1804 and 1805) has shown that there are 2 or 3 distinct metallic substances contained in the ores of platina.

† This eminent physician, as we are informed in his life prefixed to the Commentaries on the History and Cure of Diseases, was born in 1710, in London, where he received the early part of his

from a human bladder, which for its uncommon size may deserve the notice of this Society. It is of an oval shape, flatted on one side, and its surface is smooth. The specific gravity plainly shews that it is of animal origin, its weight being to that of water, only as 1.75 to 1. It was taken from the wife of Thomas Raisin, locksmith in Bury, after her death, by Mr. Gutteridge, a surgeon of Norwich. She had felt much less pain than might have been expected from so large a stone; and might probably have lived much longer with it, had she not thought herself well enough to attempt a journey on horseback; for while riding she was suddenly seized with violent pains, that obliged her to be taken off the horse

education. "In 1724 he was sent to St. John's College, Cambridge, of which he was afterwards elected a fellow. From that time he directed his attention to the study of medicine, which he pursued partly at Cambridge and partly in London. Having taken his degree of M. D. he practised in the university for about 10 years, and during that time read every year a course of lectures on the Mat. Med. In 1746, he became a fellow of the Royal College of Physicians, and 2 years afterwards leaving Cambridge, he settled in London and was elected F. R. S. He very soon got into great business, which he followed with unremitting attention above 30 years, till it seemed prudent to withdraw a little from the fatigues of his profession. He therefore purchased a house at Windsor, to which he used ever afterwards to retire during some of the summer months; but returned to London in the winter, and still continued to visit the sick for many years. In 1766 he recommended to the College of Physicians the first design of the Medical Transactions, in which he proposed to collect together such observations as might have occurred to any of their body, and were likely to illustrate the history or cure of diseases. The plan was soon adopted, and 3 vols. have successively been laid before the public. In 1778 the Royal Society of Medicine in Paris chose him into the number of their associates." Besides his Commentaries on Diseases published after his death, Dr. H. wrote several papers in the Medical Transactions, and others, in addition to the above, in the Phil. Trans. He was the first who gave a clear and satisfactory account of that painful thoracic disease, called angina pectoris. He died in 1801, when he was in his 91st year.

Dr. H. possessed a liberal and enlightened mind, a sound and accurate judgment, a refined and classical taste, and was endeared to all who knew him by the rectitude of his moral conduct, and an uniform complacency of disposition.

As a medical writer he ranks with the most eminent physicians which this country has ever produced. In his Commentaries on Diseases, written and printed separately both in English and Latin—in Latin which for classical purity may be compared to the latinity of Celsus himself—he has described with a precision and fidelity which have never been surpassed the histories of morbid affections; and with a candour worthy of imitation, has told what modes of treatment he had found, after long experience and diligent observation, to be beneficial and hurtful in each; thus bequeathing to posterity a work replete with practical truths, unmixed with theoretical reflexions. It may, however, be remarked that he seems to have been too much prejudiced against chemical medicines; and that having been disappointed in his trials of some reputed remedies in certain obstinate disorders, he was too much inclined to doubt the possibility of their cure being effected by any kind of medicines; thus throwing a damp on further remedial exertions. We readily admit that medical enthusiasm has often proved disgraceful to the art; on the other hand, we think that medical scepticism may be carried to an improper length. Because there are diseases which have hitherto resisted the action of various and even opposite remedies, we are not therefore to conclude that it is hopeless to make further curative efforts. So long as the organization of parts essential to life remains undestroyed, we ought not to despair. Nature has furnished a copious stock of medicinal agents. It will require ages to exhaust them all.

immediately: after which she could never make water, unless the stone was first moved, and she continued in great agonies till she died. This monstrous stone weighs 33 oz. 3 drs. 36 grs. Troy. There appears to have been at least $\frac{1}{4}$ oz. broken off, to examine its internal structure; not to mention what it must have lost by mere wear in 80 years.

We are told, that they have in the hospitals of Paris, human calculi weighing 34 Paris oz. but this in Trinity library, even at present, weighs 34 Paris oz. wanting 9 grs. and must have weighed considerably more when it was whole. Yet these are perhaps the heaviest that are any where recorded; except that very extraordinary one mentioned by Dr. Lister, in his journey to Paris, p. 232; which he says was taken from a monk, A. D. 1690, and weighs 51 oz.

This history may confirm to us the usefulness of endeavouring to relieve the violence of pain in this distemper, by altering the position of the stone in the bladder, either with the help of the catheter, or by some proper alteration in the posture of the patient; since, with respect to the pain which it occasions, the situation of the stone appears to be of far more consequence than its size.

Of a Nondescript Petrified Insect. By the Rev. Charles Lyttelton, LL. D. and F. R. S., Dean of Exeter.* N° 496, p. 598.

The curious fossil now exhibited to the society is as rare as its figure is elegant; having never been mentioned by any of our own writers who treat on fossils, and but very imperfectly described by foreign lithographists. Dr. L. discovered a single specimen of it fig. 9, 10, 11, pl. 1, last year, in the limestone pits at Dudley in Worcestershire; and very lately a large mass of limestone (plate 2,) full of them in the same place; both which are now submitted to the inspection of this learned body, who are best able to determine to what class of the animal kingdom it properly belongs.

Addenda to the preceding paper. Extract of a letter from the Rev. Dr. Lyttelton to C. Mortimer, Sec. R. S.—The Rev. Dr. Shaw, of Oxford, has procured a specimen of the extended eruca. As the fossilists differed in their opinion of this Dudley fossil, some pronouncing it an eruca, others a bivalve, he thought it best to leave the reader to judge for himself from the engravings; but, as we are now able to add a specimen of this fossil in an extended position, there is a better pretence to call it an eruca. See fig. 12, 13, 14, pl. 1.

* The animal itself is as yet undiscovered in its recent state. It seems to be a species either of Oniscus or Monoculus. The fossil is the *Entomolithus paradoxus* of Linnæus.

Some further Account of the before-mentioned Dudley Fossil. By the Editor of these Transactions, Dr. C. Mortimer. N° 496, p. 600.

The Rev. Dr. Pocock, F. R. S. sent several specimens of this fossil to the president; who put them into Dr. M.'s hands, and desired him to draw up an account of them to be annexed to the preceding paper.

The first specimen is a mass of stone containing the face and eyes, with some rudiments of legs on the sides; but the back is entirely broken away. Another specimen contains the head only: a third, the head, and part of the back, but greatly distorted. But the most beautiful and complete are the 2 which are exhibited in pl. 1, at fig. 15, 16, 17, 18.

At fig. 9, is one of these insects completely extended at its whole length; where it appears, that the head is covered with a shell or crust consisting of 3 parts; the middle part a, is broad and round; which he therefore calls the nose: the 2 side pieces are of a triangular form, bb, in each of which is situated a large protuberant eye, cc. The anterior part of the whole is encompassed by a round border, ddd, which looks like an upper lip; though he does not take it to be so; but that the mouth is situated lower down, as in the crab-kind, and does not appear in any of the specimens yet seen. On each side the crown of the head, towards the back part of it, are two small knobs, ee. At ff, in fig. 16, appear some traces of feet, which seem to lie under the belly: but as the belly, or under side, was not distinct, not being cleared from its stony and earthy matter, he could not discern any other legs.

Most likely the whole back of this creature, when alive, was covered with a case, or undivided elytrum, as is the scolopendra aquatica scutata, described in these transactions, n. 447, by M. Klein, of Dantzic; and afterwards by the Rev. Mr. Littleton Brown, both worthy members of this society. M. Klein says the case was whole; and that he was forced to slit it open to shew the back underneath; when it appears, that the body was trilobated, as in fig. 15. The case, being very thin and tender, may probably have been broken off at the death of the animal, before its being petrified.

Mr. Brown does not mention in his insect the property of rolling itself up, which this certainly had; as appears by several of the figures, as fig. 9, 10, 11, 13, 17, and 18, which are entirely rolled up; and as is more particularly represented by fig. 17 and 18, in which it appears, that the tail is turned up under the belly quite to the mouth; and at fig. 14 the creature seems but half rolled up.

Dr. M. consulted all the books he could meet with, which give figures of insects and crustaceous animals in their natural and petrified states; but finds none resemble this Dudley fossil so near as M. Klein's insect; therefore till more information is got, he calls it, scolopendræ aquaticæ scutatæ affine animal petrifactum.

The Description and Figures of a small flat Spheroidal Stone, having Lines formed on it. By C. Mortimer, M. D., and Sect. R. S. N° 496, p. 602.

Mr. Peter Collinson produced, at a meeting of the R.S. on Nov. 8, 1750, a very curious spheroïdal stone, about 4 inches diameter, of a chocolate colour, marked with 4 white lines, about the breadth of a horse-hair, encompassing the whole stone, like the meridians on a globe; but, instead of crossing one another in a point, as they do, on the globe, these are connected by a short transverse line.

Dr. M. also lately received, by a friend, from the isle of Shepy in Kent, a small stone, with similar lines on it. This is only $\frac{7}{8}$ of an inch in diameter, of a brown colour, and of the consistence of marble. As a description in words does not convey so clear an idea as an exact drawing, he has given representations of this stone in different views; as at tab. I. fig. 19, 20, 21, 22.

Fig. 19 represents the top of the stone, on which the lines are most regular, being depressed into the stone, and of the same colour with it. a, c, d, e, are the 4 principal lines, answering to those on Mr. Collinson's stone, and are connected, as in his, by the transverse line g h. The line b is an irregularity in this stone, and so is f, which are not in the other; these irregular, or supernumerary lines being continued to the other hemisphere.

Fig. 20, or bottom of the stone, make the directions of the other lines very irregular, as may be seen in the figure; only the lines c, d, and e, being connected by the transverse line g h. which here stands at right angles with that in fig. 19.

The following figures represent the section of the stone through its equator, as nearly as possible; only the mill cut away the substance to about the thickness of a shilling. In these sections the ramifications appear quite white. Fig. 21 shews the section of the upper hemisphere, as fig. 22 does that of the lower hemisphere; in both of which the letters of reference answer to those in the other figures, shewing where the outside lines abut on these sections.

An Explanation of the foregoing Figures Collected in Pl. I.

9. The face of the Dudley fossil rolled up; 10, the back of the same; 11, the fore and under part, with the tail folded close under the jaw; 12, Dr. Shaw's fossil half extended; 13, the face of the same; 14, the under side of the same, being folded but half way, leaving a space between the jaw and the tail; 15, Dr. Pocock's extended fossil, the back uppermost; 16, a side view of the same;

17, a side view of another folded up; 18, a front view of the same with the tail folded close under the jaw.

The letters of reference in the description answer to the same parts in fig. 15, 16, 17, and 18; but 19, 20, 21, 22, are the figures of the stone, described in this article, in all which the letters refer to the same lines.

Plate II. represents a large mass of lime-stone dug up at Dudley, in which are embodied many of these fossils, with several other petrified shells.

A Collection of Various Papers presented to the R. S. concerning several Earthquakes, felt in England, and other Countries, in 1750 *and other Years.* N° 496, p. 601, &c.

About the year 1750 some earthquakes were felt in many parts of England, indeed almost all over the country: and though no serious ill consequences attended them, yet they produced a vast number of communications to the R. S. stating the circumstances of them, from many different parts of the country. These accounts the Society collected, and printed all together, at the end of this volume 46, forming the N° 497, being the last of this series of publication in the form of N°s by the Secretaries, on their own account. But, as may be expected, there being a general sameness or uniformity that runs through all these accounts, such as loud rumbling noises, the shaking of the ground, the tottering of houses, the rattling of the windows, and the furniture of houses, &c. which circumstances may be all easily conceived; it would be irksome and disgusting to reprint such a number of tedious, and similar, and uninteresting accounts. Instead of which therefore, we shall here give a summary of the whole in the following table; containing, in the 1st column, the date of the earthquakes; in the 2d, the names of the persons communicating the accounts, with the pages of the original vol. (46) where the accounts stand; and in the 3d, the places where the persons write from, or where the earthquake was felt. After which, we shall advert to any particular circumstances, that may be more particularly deserving of that notice.

List of the Earthquakes in this Number, with the Places and Names of the Writers.

Dates, N. S.	Authors and Pages.	Places.	Dates, N. S.	Authors and Pages.	Places.
Feb. 8, 1750,	Hen. Baker.... p. 601,	London.	Mar. 8, 1750,	Martin Clare... 620,	Kensington
....	Gowin Knight... 603,	....	....	Dr. D. P. Layard, 621,	London.
....	Jo. Freeman.. .. 605,	....	....	R. Pickering.. .. 622,	....
....	Wm. Fauquier.. 505,	Eltham.	....	Ja. Burrow, Esq. 626,	....
....	Dr. Hen. Miles.. 607,	Tooting	....	Dr. H. Miles. ... 628,	Tooting.
....	Dr. John Martyn, 609,	Chelsea.	....	Dr. J. Martyn... 630,	Chelsea.
....	S. Lethicullier... 613,	Aldersbrose.	....	Mic. Russel..... 631,	London.
Mar. 8, 1750,	M. Folkes, Esq. 613,	London.	....	Dr. Ja. Parsons... 633,	....
....	Rev. Tho. Birch. 615,	....	....	Ja. Burrow, Esq. 637,	....
....	Henry Baker. .. 617,	....	....	Dr. C. Mortimer, 638,	....
....	Dr. H. Miles. .. 619,	Tooting.	....	Dr. Miles....... 639,	Tooting.

Dates, N. S.	Authors and Pages.	Places.	Dates, N. S.	Authors and Pages.	Places
Mar. 18, 1750,	J. Ellicott...... 646,	Portsmouth.	Apr. 2, 1750,	Rev. J. Seddon.. 696,	Warrington.
....	Dan. Wray, Esq. 647,	....	Mar. 8, 1750,	Wm. Jackson.... 700,	London.
Mar. 8, 1750,	Rev. Dean Cooper, 647,	Hertford.	Mar. 24, 1750,	M. Mackenzie... 701,	Smyrna.
Mar. 18, 1750,	Rev. Mr. Taylor, 649,	Portsmouth.	Sep. 30, 1750,	Wm. Folkes, Esq 701,	Newtown.
Mar. 19, 1750,	Benj. Cooke.. .. 651,	Isle of Wight	....	Ja. Burrow, Esq. 702,	Suffolk.
Mar. 18, 1750,	Jos. Colebrooke.. 652,	Southampt.	....	....	Leicester.
....	P. Newcome.... 653,	Hackney.	....	Sir Tho. Cave.... 706,	....
....	Ja. Burrow, Esq. 655,	East Sheen.	....	J. Nixon....... 707,	Northampt.
Mar. 8, 1750,	Tho. Burrat.... 681,	Kensington.	....	....	Weston.
Apr. 2, 1750,	Rob. Paul....... 683,	Chester.	....	 710,	Warwick.
Mar. 14, 1750,	W. Bowman.... 684,	Ea. Molesey	....	Dr. Doddridge... 712,	Northampt.
Apr. 2, 1750,	Mr. Pennant.... 687,	Flintshire.	....	Steward of the Earl of Cardigan.... 721,	Stamford.
Mar. 18, 1750,	Nat. Downe.... 688,	Bridport.			
May 4, 1749,	Henry Baker.... 689,	Winbourn.	....	Henry Green... 723,	Leicester.
July 1, 1747,	....	Tannton.	Aug. 23, 1750,	M. Johnson.. .. 725,	Spalding.
Oct. 11, 1749,	M. Reaumur.... 691,	France.	Sep. 30, 1750,	Dr. H. Miles.... 726,	Tooting.
Feb. 9, 1750,	Rev. W. Barlow.. 692,	Plymouth.	....	Wm. Smith..... 727,	Peterborough

It is pretty generally agreed that the shocks lasted only 3 or 4 seconds of time. On occasion of these earthquakes, the Rev. Dr. Wm. Stukely wrote his thoughts on the causes of such phenomena. These are given at 3 different parts of this N° viz. at p. 641, 657, 731; and were also printed and published by the author in a separate pamphlet, in 1750, in 8vo. He rejects, he says, the common notion of struggles between subterraneous winds, or fires, vapours, or waters, heaving up the ground like animal convulsions; but he always thought it was an electrical shock; which he is induced to think is the case, from several circumstances, which he notices.

" We had lately, says Dr. S. a very pretty discourse read here, from Mr. Franklin of Philadelphia, concerning thundergusts, lights, and like meteors. He well solves them by the touch of clouds, raised from the sea, which are non-electrics, and of clouds raised from exhalations of the land, which are electrified: that little snap, which we hear, in our electrical experiments, when produced by a thousand miles compass of clouds, and that re-echoed from cloud to cloud, through the extent of the firmament, makes that thunder, which affrightens us. From the same principle I infer, says the Dr. that if a non-electric cloud discharges its contents on any part of the earth, when in a high electrified state, an earthquake must necessarily ensue. As a shock of the electric tube in the human body, so the shock of many miles compass of solid earth, must needs be an earthquake; and that snap, from the contact, be the horrible uncouch noise of it."

Dr. Ste. Hales also, p. 669, besides noticing the phenomena of the earthquake. ventures on an opinion of the cause of such convulsions. " As to the affairs of earthquakes, says he, particularly that which happened March 8, 1750, about 20 minutes before 6 in the morning; I being then awake in bed, on a ground floor, near the church of St. Martin's in the Fields, very sensibly felt the bed heave, and consequently the earth must heave too. There was a hollow, obscure, rushing noise in the house, which ended in a loud explosion up in the air, like that of a small cannon: the whole duration, from the beginning to the end of the earthquake, seemed to be about 4 seconds of time. The soldiers who were on duty in St. James's Park, and others who were then up, saw a blackish cloud, with considerable lightning, just before the earthquake began; it was also very calm weather.

" In the history of earthquakes it is observed, that they generally begin in calm weather, with a black cloud. And when the air is clear, just before an earthquake, yet there are then often signs of plenty of inflammable sulphureous matter in the air; such as Ignes Fatui or Jack-a-Lanterns, and the meteors called falling stars.

" Now I have shewn many years since, in the appendix to my Statical Essays, experiment 3, page 280, the effect that the mixture of a pure and a sulphureous air have on each other; viz. by turning the mouth downwards, into a pan of water, of a glass vessel of a capacity sufficient to hold about two quarts, with a neck about 20 inches long, and 2 inches wide; then, by putting under it, in a proper glass vessel, with a long narrow neck, a mixture of aqua fortis, and powdered pyrites, viz. the stone with which vitriol is made, there will be a brisk ferment, which will fill the glass with redish sulphureous fumes; which, by generating more air than they destroy, will cause the water, with which the whole neck of the glass vessel was filled, to subside considerably. When the redish sulphureous air in the upper part of the glass is clear, by standing 2 or 3 hours, if then the mouth of the inverted glass be lifted out of the water, so as to let the water in the neck of the glass fall out; which, supposing it to be a pint, then an equal quantity of fresh air will rush in at the mouth of the neck of the vessel, which must immediately be immersed in the water: and on the mixture of the fresh air with the then clear sulphureous air, there will instantly arise a violent agitation between the two airs, and they will become, from transparent and clear, a reddish turbid fume, of the colour of those vapours which were seen several evenings before the late earthquakes: during which effervescence, a quantity of air, nearly equal to what fresh air was let in, will be destroyed; which is evident by the rising up of the water in the neck of the glass, almost as high as before. And if, after the effervescence of the mixed airs is over, and become clear again, fresh air be admitted, as before, they will again grow reddish and turbid, and destroy the new admitted air as before; and this after several repeated admissions of fresh air: but after every readmission of fresh air the quantity destroyed will be less and less, till no more will be destroyed. And it is the same after standing several weeks, provided, in the mean time, too much fresh air had not been admitted. Now, I found the sum total of the fresh air thus destroyed to be nearly equal to the first quantity of sulphureous air in the inverted glass.

"Since we have in this experiment a full proof of the brisk agitation and effervescence which arises from the mixture of fresh air with air that is impregnated with sulphureous vapours, which arise from several mineral substances, especially from the pyrites, which abounds in many parts of the earth; may we not with good reason conclude, that the irksome heat, which we feel in what is called a close sultry temperature of the air, is occasioned by the intestine motion between the air and the sulphureous vapours, which are exhaled from the earth? which effervescence ceases, as soon as the vapours are equably and uniformly mixed in the air; as happens also in the effervescences and ferments of other liquors. The common observation therefore, that lightning cools the air, seems to be founded on good reason; that being the utmost and last effort of this effervescence.

"May we not hence also, with good probability, conclude, that the first kindling of lightning is effected by the sudden mixture of the pure serene air above the clouds, with the sulphureous vapours, which are sometimes raised in plenty, immediately below the clouds? the most dreadful thunders being usually when the air is very black with clouds; it rarely thundering without clouds: clouds serving, in this case, like the above-mentioned inverted glasses, as a partition between the pure and sulphureous airs: which must therefore, on their sudden admixture through the interstices of the clouds, make (like the two airs in the glass) a more violent effervescence, than if those airs had, without the intervention of the clouds, more gradually intermixed, by the constant more gradual ascent of the warmer sulphureous vapours from the earth, and descent of the cold serene air from above. And though there was no luminous flash of light in the glass, yet, when such sudden effervescence arises, among a vast quantity of such vapours in the open expanse of air, it may, not improbably, acquire so rapid a velocity, as to kindle the sulphureous vapours, and thereby become luminous.

"And since, from the effects that lightning is observed to have on the lungs of animals, which it often kills, by destroying the air's elasticity in them, as also from its bursting windows outwards, by destroying the air's elasticity on the outside of those windows: since, I say, it is hence probable, that the sulphureous fumes do destroy a great quantity of elastic air; it should therefore cause great commotions and concussions in the air, when the air rushes into those evacuated places; which it must necessarily do with great velocity.

"Dr. Papin has calculated the velocity with which air rushes into an exhausted receiver, when driven by the whole pressure of the atmosphere, to be at the rate of 1305 feet in a second of time; which is at the rate of 889 miles in an hour: which is near 18 times a greater velocity than that of the strongest storms; which is estimated to be at the rate of 50 miles in an hour.

" Hence, we see that an outrageous hurricane may be caused, by destroying a small proportion of the elasticity of the air of any place, in respect to the whole. No wonder then that such violent commotions of the air should produce hurricanes and thunder showers; especially in the warmer climates; where both the sulphureous and watery vapours, being raised much higher, and in greater plenty, cause more violent effects.

" Monsieur de Buffon in his Natural History, and Theory of the Earth, mentions black dark clouds in the air near the tempestuous Cape of Good Hope, and also in the ocean of Guinea, which are called by the sailors the Ox's Eye; which are often the forerunners of terrible storms and hurricanes. Whence it is to be suspected, that they are large collections of sulphureous vapours; which, by destroying suddenly a great quantity of the elastic air, cause the ambient air to rush with great violence into that vacuity, thereby producing tempests and hurricanes. And off the coast of Guinea they have sometimes 3 or 4 of these hurricanes in a day; the forerunners of which are these black sulphureous clouds, with a serene clear air, and calm sea; which on a sudden turns tempestuous, on the explosion of these sulphureous clouds. And in Jamaica they never have an earthquake when there is a wind to disperse the sulphureous vapours.

" In like manner we find, in the late earthquakes at London, and in the accounts of many other earthquakes, that before they happen there is usually a calm air, with a black sulphureous cloud: which cloud would probably be dispersed like a fog, were there a wind: which dispersion would prevent the earthquake; which is probably caused by the explosive lightning of this sulphureous cloud; being both nearer the earth than common lightnings; and also at a time when sulphureous vapours are rising from the earth in greater quantity than usual; which is often occasioned by a long series of hot and dry weather. In which combined circumstances, the ascending sulphureous vapours in the earth may probably take fire, and thereby cause an earth-lightning; which is at first kindled at the surface, and not at great depths, as has been thought: and the explosion of this lightning is the immediate cause of an earthquake.

" It is in the like manner that those meteors, called falling stars, are supposed to be kindled into a flame at the upper part of a sulphureous train, which is kindled downwards into a flame, in the same manner as a fresh blown-out candle is instantly lighted from another candle held over it at a distance, in the sulphureous inflammable smoke of it.

" I am sensible that it may seem improbable, that the ascending sulphureous vapours in the earth should thus be kindled; but since they are continually ascending through the pores of the earth, more or less, for many good and useful purposes, it is plain there is room for them to pass. Besides, as Mons. de Buffon remarks, naturalists have observed perpendicular and oblique clifts, in all kinds of layers of earth, not only among rocks, but also among all kinds of earth, that have not been removed, as is observable wherever the earth is open to any depth. Now these clifts are caused by the drying of the several horizontal layers of the earth; and will also be considerably the wider in long dry hot seasons, which are usually the preparatory forerunners of earthquakes, and the explosion of the sulphureous vapours may probably widen them more.

" It is very observable, in the opinion of Borelli, and other naturalists, that Volcanos begin first to kindle near the surface or top of the mountains, and not in the caverns in the lower parts of them. Mons. de Buffon says, that earthquakes are most frequent where there are Volcanos; sulphureous matter abounding most there: but that, though they continue burning long, yet they are not very extensive. But that the other sort of earthquakes, which are not caused by a Volcano, extend often to a great distance. These are much longer east and west, than broad north and south; and shake a zone of earth with different degrees of force in different parts of their course; viz. in proportion to the different quantities of explosive sulphureous matter in different places. These kinds of earthquakes are observed to be progressive, and to take time to extend to the great distances sometimes of some thousands of miles. They are an instantaneous explosion in every place, near the surface of the earth; and therefore do not produce mountains and islands, as volcanos sometimes do.

" The earthquake in London, March 2, was thought to move from eastward to westward. M. Buffon mentions an earthquake at Smyrna, in the year 1688, which moved from west to east; viz. because the first kindling probably began on the western side; and in the earthquake at London on the eastern side. And accordingly it was observed, that the reddish bows in the air, which appeared several days before that earthquake, arose in the east, and proceeded westward. It was observed, after the earthquake at Smyrna, that the castle-walls, which run from east to west, were thrown down; but those from north to south stood; and that the houses on rocks stood better than those on the earth.

" M. de Buffon relates, that the vibrations of the earth, in earthquakes, have commonly been from north to south; as appears by the motion of the lamps in churches: which makes it probable, that though the progress of the earthquake at Smyrna was from west to east, yet the vibrations of the earth might be from north to south; and thereby occasion the falling of the castle walls, which run from east to west, but not those which run from north to south. A probable argument that, as the freest passage, so the greatest explosions were made in the clifts of the earth which run east and west; which would make the vibrations north and south.

"It was observed, that the waters turned foul the day before an earthquake at Bologna in Italy: and I was informed, that the water of some wells in London turned foul at the time of the earthquakes. Which was probably occasioned by the ascent of great plenty of sulphureous vapours through the earth.

"As to the hollow rumbling noise, which is usually heard in earthquakes, it seems not improbable, that it may be occasioned by the great agitation that the electrical æthereal fluid is put into by so great a shock of a large mass of earth. For, if the like motion of a small revolving glass globe can excite it to the velocity of lightning, and that with a force sufficient to kill animals, how much greater agitation may it probably be excited to, by the explosive force of an earthquake!

"The explosion of a cannon in St. James's Park is observed to electrify the glass of the windows of the treasury. And what makes it still more probable, is the analogy that there is between them in other respects. For, as the electrical flash rushes, with the velocity of lightning, along the most solid bodies, as iron, &c. and as I have seen it run only on the irregular gilding of leather; so such solid bodies are observed to be the conductors of aereal lightning, which rends oaks in pieces, and has been known to run along and melt an iron bell-wire on two sides of a room, &c. And accordingly it was observed, in the great earthquake in Jamaica, that the most tremendous roaring was in the rocky mountains. And in the late earthquake of March 8 in London, the loudest explosions were thought to be heard near such large stone buildings as churches, with lofty steeples and spires.

"I, who lay in Duke's-Court, near St. Martin's church, and was awake all the time of the earthquake, plainly heard a loud explosion up in the air, like that of a small cannon: which made me conjecture, that the noise was owing to the rushing off, and sudden expansion, of the electrical fluid, at the top of St. Martin's spire; where all the electrical effluvia, which ascended up along the larger body of the tower, being by attraction strongly condensed, and accelerated at the point of the weathercock, as they rushed off, made so much the louder expansive explosion.

"The Rev. John Seddon, says, p. 697, as soon as I felt the shock, I was immediately apprehensive what it was, and went out to see whether there was any thing remarkable in the atmosphere. I then observed a very uncommon appearance; viz. an infinite number of rays, proceeding from all parts of the heavens, converged to one point; no luminous body appeared at all. The rays were at first of a bright yellow; afterwards they became blood-red. This phenomenon was not far from our zenith. It continued about 20 minutes, and then disappeared.

Dr. Doddridge, p. 718, says, the morning on which the phenomenon happened, was remarkably calm; but quickly after the shock, the wind rose; and the clouds, which had covered the heavens for several days, were pretty much dispersed. There was a report that, at near 4 o'clock that morning, (Sunday,) a ball of fire was seen. On Monday night the sky in the east was as red as blood; and on Tuesday night was the finest aurora borealis he ever saw. He says that a Mr. Scawen was confident that he heard that rushing noise, so generally mentioned by all who observed any thing extraordinary, not only before, but after the shock; and that he could trace the direction, from S. W. to N. E. He adds, that a niece of Sir Hans Sloane observed, that just before the shock, her birds drooped remarkably, and hid their heads under their wings: a circumstance often observed in Italy, and other places where these phenomena are frequent.

The last of these papers is by Dr. Stukely, p. 731, on what he calls the philosophy of earthquakes: he recounts the most remarkable circumstances mentioned in the several accounts, and thence deduces a theory to explain the whole.

We have had, says he, many opportunities of reflecting on that most awful, and hitherto unusual appearance. The year 1750, may rather be called the year of earthquakes, than of jubilee. For, since they began with us at London, they have appeared in many parts of Europe, Asia, Africa, and America, and have likewise revisited many counties in our island: at length on 30th of September, have taken their leave (as we hope) with much the most extensive shock we have seen in our days.

We have been acquainted by those that remember it, that in the earthquake of November 1703, which happened in Lincolnshire, the weather was calm, close, gloomy, warm, and dry, in a degree highly unusual at that season: and thus it has been with us all the year: and from the numerous accounts we have received at the Royal Society, in the beginning and end of the year, where any mention is made of the weather, they agree in the like particular: which is consentaneous to what is remarked as the constant forerunner of earthquakes, and what prepares the earth's surface to receive the electrical stroke.

We had a paper read at the Royal Society, concerning the first earthquake felt by us at London on 8th of February. A shepherd belonging to Mr. Secretary Fox at Kensington, the sky being perfectly serene and clear, was much surprized with a very extraordinary noise in the air, rolling over his head, as of cannon close by. This noise passed rushing by him; and instantly he saw the ground, a dry and solid spot, wave under him, like the face of the river. The tall trees of the avenue, where he was, nodded their tops very sensibly, and quavered. The flock of sheep immediately took fright, and ran away all together, as if the dogs had pursued them. A great rookery in the place were equally alarmed; and, after a universal clangor, flew away, as if chased by hawks.

It was likewise mentioned, that in the same earthquake, a great parcel of hens and chickens, kept at that time in Gray's-Inn-Lane, on the shock, ran to the roost affrighted: and the like was observed of pigeons. And in our account of the last earthquake from Northampton, it is remarked, that the birds in cages put their heads under their wings, as to hide themselves.

Mr. Jackson, potter at Lambeth, gave an account of some boats and lighters, in the river at that time; the people in them seemed to feel as if a porpoise, or some great fish, had heaved and thumped at the bottom of the lighters. This is sometimes the case with ships at sea; which seems evidently owing to an electrical impression on the water.

In the Evening Post, June 23, we had a paragraph from Venice, that a terrible earthquake had lately been felt in the Isle of Cerigo; a little rocky isle. It threw down a great number of houses, and above 2000 inhabitants were buried in the ruins. Another earthquake about that time happened in Switzerland, which split a vast rocky mountain, and an old castle wall, of an immense thickness.

But, since then, these wonderful movements have stalked round the globe; (and been lately felt in our own island, though to the terror only of many thousand people) besides those that appeared in the western parts, in the more early time of the year.

In a letter from Maurice Johnson, Esq. the founder and secretary of the Literary Society of Spalding, which has now subsisted these 40 years, he says that, on Thursday the 23d of August last, an earthquake was very sensibly felt there, about 7 o'clock in the morning, throughout the whole town and neighbourhood, and many miles round; but chiefly spread northward and southward. He says, that for a fortnight before the weather had been serene, mild and calm; and one evening there was a deep red aurora australis, covering the cope of heaven, very terrible to behold. This same shock was felt at Grantham, Stamford, and Milton by Peterborough; and generally at all the intermediate places.

But we have had many advices from all hands, at the first and second meetings of the R. S. for the winter season; with further particulars relating to this great concussion: that it was felt at the same time at Rugby in Warwickshire, and reached to Warwick; at Lutterworth in Leicestershire; at Leicester, and round about. They describe it, that the houses tottered, and seemed to heave up and down, though it lasted but a few seconds. It was attended with a rushing noise, as if the houses were falling; and people were universally so affrighted as to run out; imagining that their own, or neighbours' houses, were tumbling on their heads. In the villages around, the people, being at divine service, were much alarmed, both with the noise, which exceeded all the thunder they had ever heard, beyond compare; and with the great shock accompanying, which was like somewhat that rushed against the church-walls and roof; some thinking the pillars cracked; many, that the beams of the roof were disjointed; and all, that the whole was falling; and happy were they that could get out first. A few slates, tiles, and parts of chimneys, fell from some houses; pewter, glasses, and brass, fell from shelves; a clock-bell sometimes struck; windows universally rattled; and the like circumstances of tremor.

The same extended itself to Coventry, Derby, Nottingham, Newark; then came eastward to Harborough, Towcester, Northampton, Rowell, Kettering, Wellingborough, Oundle in Northamptonshire, Uppingham, Oakham in Rutland, Stamford, Bourn, Grantham, Spalding, Boston, and to Lincoln, in Lincolnshire; Holbech, and All-Holland, in that county; Peterborough, Wisbech in the Isle of Ely, together with all the intermediate and adjacent places. Then it passed over the whole breadth of Ely-Fen, and reached to Bury in Suffolk, and the country thereabouts; of which we had notice from Lady Cornwallis: an extent from Warwick to Bury of about 100 miles in length; and, generally speaking, 40 miles in breadth. And this vast space was pervaded by this amazing motion, as far as we can get any satisfaction, in the same instant of time.

In regard to circumstances, they were pretty similar throughout. At Northampton, a gentlewoman, sitting in her chair, relates, that she and her chair were twice sensibly lifted up, and set down again. A stack of chimneys were thrown down in College-lane; a place retaining the memory of a sort of university once beginning at Northampton. The windows of houses rattled throughout the whole town; but no mischief done.

They fancied there the motion of it, as they express it, to be eastward. In streets that run north and south, the houses on the east side of the way were most affected: and Dr. Stonehouse's dwelling, the strongest in the town, was most sensibly shaken. So it was likewise observed, that churches were most subject to its violence. They thought too that the motion seemed rather horizontal, or lateral, than upward. Some counted the pulses distinctly, to the number of 4. That the second and third pulse were stronger than the first and fourth. From all these various accounts, there was no sulphureous smell, or eruption; no fissures in the ground perceived; yet several people were sick upon it.

It was more evidently perceived by people standing; most, by those that were sitting; least, by such as were walking; and in upper stories of houses more than in lower, or in cellars. Some, coming down stairs, were in danger of being thrown forwards: several sitting in chairs, and hearing the hollow thundering noise, and thinking it was a coach passing by, when they attempted to get up, to see what it was, they were thrown back again into their chair. Some heard the wainscot crackle. A lady, sitting by the fire, with her chair leaning forwards, was thrown down on her hands and knees.

It was particularly remarked (as before mentioned), that birds in cages were sensibly affrighted, thrusting their heads under their wings. Mrs. Allicock, of Loddington, a lady in childbed, was so affected that it caused her death. Some people felt such a sudden shortness of breath, that they were forced to go out into the open air, it so affected the pulmonary nerves. Many were taken with head-achs.

These are, in general, the observations made at the time of these earthquakes. Give me leave now to make the following remarks.

1. As far as we can possibly learn, where no one can be prepared at different places by time-keepers, this mighty concussion was felt precisely at the same instant of time, being about half an hour after 12 at noon. This, I presume, cannot be accounted for by any natural power, but that of an electrical vibration; which we know acts instantaneously.

2. Let us reflect on the vast extent of this trembling, 100 miles in length, 40 in breadth, which amounts to 4000 square miles in surface. That this should be put into such an agitation in one moment, is such a prodigy, as we should never believe, or conceive, did we not know it to be a fact, from our own senses. But if we seek for a solution of it, we cannot think any natural power is equal to it, but that of electricity; which acknowledges no sensible transition of time, no bounds.

3. We observe, the vulgar solution of subterraneous eruptions receives no countenance from all that was seen or felt during these earthquakes: it would be very hard to imagine how any such thing could so suddenly and instantaneously operate through this vast space, and that in so similar and tender a manner, over the whole, through so great a variety as well as extent of country, as to do no mischief.

A philosophical inquirer in Northamptonshire, who had his eye particularly on this point, takes notice there were not any fissures in the ground, any sulphureous smells, or eruptions, any where perceived, so as to favour internal convulsions of the earth; yet we learn, from a letter, at Uppingham in Rutland, that a plaster floor became cracked thereby. These kind of floors are frequent in this country: what we call stucco in London; and it gives us a good notion of the undulatory vibration produced by an earthquake; which some have compared to that of a musical string; others, to that of a dog, or a horse, shaking themselves when they come out of the water.

4. The former earthquake, that happened at Grantham, Spalding, Stamford (which towns lie in a triangle) took up a space which may in gross be accounted a circle of 20 miles in diameter; the centre of which is that great morass called Deeping-Fen. This comprehends 14 miles of that 20 in diameter; and where, probably, the electrical impression was first made. Much the major part of

Deeping-Fen is under water in the winter; underneath is a perfect bog: now it is very obvious how little favourable such ground is for subterraneous fires.

In the second earthquake, not only this country was affected again, but likewise a much larger space of the same sort of fenny ground, rather worse than the former: all Donington-Fen, Deeping-Fen, Croyland-Fen, Thorney-Fen, Whitlesea-Fen, Bedford-Level, and the whole extent of Ely-Fen, under various denominations. This country, under the turf, abounds with subterraneous timber of all kinds; fir, oak, and brush-wood; stags' horns: now and then they find a quantity of hazel nuts, crouded together on a heap: I have some of them. This is a matter common to all boggy ground over the whole globe. They are the ruins of the antediluvian world, washed down from the high country, where they grew, here lodged, and by time overgrown with the present turf. They that seek for any other solution of this affair, than the universal Noachian deluge, want to account for a general effect by a partial cause; and shut their eyes, both to the plain history of this matter, and to the infinite notorious demonstrations of it from fossil appearances.

5. All this country, though underneath it is a watery bog, yet, through this whole summer, and autumnal season (as they can have no natural springs in such a level) the drought has been so great on the superficies, that the inhabitants were obliged every day to drive their cattle several miles, for watering. This shows how fit the dry surface was for an electrical vibration; and we learn from hence this important particular, that it reaches but very little below the earth's surface.

Mr. Johnson, in another letter which he wrote concerning the second earthquake, observed at Spalding, says, on this occasion, he was obliged to scour his canal, and deepen it; that they came to a white quicksand, which afforded to all the neighbourhood excellent water in plenty.

In the gravelly soil of London, and where the 2 shocks were felt by us, in the beginning of the year, we know there is not a house in the whole extent of this vast city, and all around it, but a spring of water is ready, on digging a well: whence we have much reason to believe, that the internal parts of the earth are like a sponge soaked in water; so that the only dry part of it is the superficies; which is the object, and the subject, of that electric vibration, wherein it seems an earthquake consists. This shows the mistake of the ancients; who, fancying that earthquakes proceeded from subterraneous eruptions, built their prodigious temple of Diana of Ephesus on a boggy ground, to prevent such a disaster.

6. Earthquakes are truly most violent in a rocky country; because the shock is proportionate to the solidity of the matter electrified: so that rocks, old castle-walls, and strong buildings, are most obnoxious to the concussion. The Isle of

Cerigo was more liable, and more rudely treated by the late earthquake; both because it was an isle, and because it was rocky. So we must say of the late earthquake in Switzerland, that split the mountain and the old castle-wall. Whence Mr. Johnson, in his second letter, says, it cracked a very strong brick house in Gosberton by Spalding. Dr. Doddridge observes, from Northampton, that Dr. Stonehouse's dwelling, being a very strong one, was most sensibly shaken. And, throughout the whole extent of this great earthquake, we find both the noise, the shock, and the terror, was greatest at the churches, whose walls and bulk made more resistance than houses: and generally speaking, the churches throughout this whole extent have very fair and large towers, and many remarkable spires of good stone.

This same vibration, impressed on the water, meeting with the solid of the bottom of ships and lighters, gives that thump felt there. Yet, of the millions of ordinary houses, over which it passed, not one fell: a consideration which sufficiently points out to us what sort of a motion this was not; what sort of a motion it was; and whence derived: not a convulsion of the bowels of the earth, but a uniform vibration of its surface, aptly thought like that of a musical string; or what we put a drinking-glass into, by rubbing one's finger over the edge; which yet, brought to a certain pitch, breaks the glass; undoubtedly an electric repulsion of parts.

7. We find, from all accounts ancient and modern, that the weather preceding these shocks was mild, warm, dry, serene, clear, frosty: what notoriously favours all our electrical experiments. We very well know, that generally all last winter, spring, summer, and autumn, have been remarkably of this kind of weather; more so than has been observed in our memory; and have had all those requisites, appearances, and preparations, that notoriously cause electricity, that promote it, or that are the effects of it.

8. We find the blood-red australis aurora preceding at Spalding, as with us at London. This year has been more remarkable than any for fire-balls, thunder, lightning, and coruscations, almost throughout all England. Fire-balls more than one were seen in Rutland and Lincolnshire, and particularly observed. All these kinds of meteors are rightly judged to proceed from a state of electricity in the earth and atmosphere.

Mr. Johnson, in both his letters on the first and second earthquakes at Spalding, remarks particularly of their effects being mostly spread to the north and south, and especially felt on the sea-coast. We may observe that such is the direction of Spalding river, which both conducts and strengthens the electric vibration; conveying it along the sea-shore, thence up Boston channel, and so up Boston river to Lincoln; as we discern, by casting our eye upon a map.

We observe further, that the main of this second earthquake displayed its effects along and between the 2 rivers Welland and Avon; and that from their very origins down to their fall into the sea. It likewise reached the river Witham, which directed the electric stream that way too to Lincoln: for which reason, as there meeting the same coming from Boston, the shock was most sensibly felt. It reached likewise to the Trent at Nottingham, which conveyed it to Newark.

The first electrical stroke seems to have been made on the high ground above Daventry in Northamptonshire, where are the Roman camps, made by P. Ostorius the proprætor. Thence it descended chiefly eastward, and along the river Welland, from Harborough to Stamford, Spalding, and the sea; and along the river Avon, or Nen, to Northampton, Peterborough, and Wisbech, to the sea. It spread all over the vast level of the Isle of Ely, assisted by many canals and rivers, natural and artificial, made for drainage. It was still conducted eastward, up Mildenhall river in Suffolk, to Bury, and the parts adjacent. All this affair, duly considered, is a confirmation of the doctrine I advanced on this subject.

10. I apprehend it was not the noise in the air, as of many cannon let off at once, preceding the earthquake, that so much affrighted people, or affected the sheep, the rookery at Kensington, the hen and chickens in Gray's-Inn-Lane, and the pigeons: it could not be barely the superficial movement of the earth that disturbed them all at once: I judge it to be the effect of electricity, somewhat like what causes sea-sickness; such a sort of motion as we are not accustomed to. So the earthquake affects all those of weak nerves, or that have nervous complaints, obnoxious to hysterics, colics, rheumatic pains in their joints. Several women were seized with violent head-achs, before both the shocks we felt in London. It was this that affected the people with a shortness of breath. This made the dog run whining about the room, seeking to get out: This made the fishes leap up in the pond at Southwark; like the experiment of electrifying the fishes; it makes them sick: and this causes the birds in cages to hide their heads under their wings, because they cannot fly away: which is commonly observed of them in Italy, and countries where earthquakes are more frequent.

11. I observe, the shepherd of Kensington thought the motion of the earthquake, and the sound, were from N.W. to S.E. On the contrary, Mr. Byfield, the scarlet dyer in Southwark, thought the noise came from the river below-bridge, and went toward Westminster; where it rattled so, that he did not doubt but that the abbey-church was beaten down. Dr. Parsons took pains to find out the way of the motion of the earthquake, from the different position of the beds; but from the contradictory answers given, he could obtain no satisfac-

tion, as to that point. All this, and what was observed from Northampton, of the motion being thought by some to be upward and downward, by others, rather horizontal or lateral, the counting the pulses, and the like, only points out to us the prodigious celerity, and the vibratory species of the motion of an earthquake, but far, very far, is this from being owing to the tumultuous ebullition, the irregular hurry of subterraneous explosions.

12. How the atmosphere and earth are put into that electric and vibratory state, which prepares them to give or receive the snap, and the shock, which we call an earthquake, what it is that immediately produces it, we cannot say; any more than we can define what is the cause of magnetism, or of gravitation, or how muscular motion is performed, or a thousand other secrets in nature.

We seem to know, that the Author of Nature has disseminated ethereal fire through all matter; by which these great operations are brought about. This is the subtil fluid of Sir Isaac Newton, pervading all things; the occult fire diffused through the universe, according to Marcilius Ficinus, the Platonic philosopher, in the Timæus of his master. And the Platonists insist on an occult fire passing through and agitating all substance by its vigorous and expansive motion.

Before them, Hippocrates writes in the same sense, 1. de victus ratione, that this fire moves all in all. This ethereal fire is one of the 4 elements of the ancients: it lies latent, and dispersed through all the other 3, and quiescent; till collected in a quantity, that overbalances the circumjacent; like the air crouded into a tempest; or till it is excited by any proper motion.

This fire gives elasticity, and elasticity, or vibration, is the mother of electricity. This fire is in water, and betrays itself to our senses in salt water. Many a time, when I have passed the Lincolnshire washes in the night-time, the horse has seemed to tread in liquid flames. The same appearance is often at the keel of a ship.

The operation of the ethereal fire is various, nay infinite, according to its quantity, and degree of incitement, progress, hindrance, or furtherance. One degree keeps water fluid, says the learned bishop of Cloyne: another turns it into elastic air: and air itself seems nothing else but vapours and exhalations rendered elastic by this fire.

This same fire permeates and dwells in all bodies, even diamond, flint, and steel. Its particles attract with the greatest force, when approximated. Again, when united, they fly asunder with the greatest celerity. All this is according to the laws prescribed by the Sovereign Architect. This is the life and soul of action and re-action, in the universe. Thus has the Great Author provided against the native sluggishness of matter! light, or fire, in animals, is what we call the animal spirits; and is the author of life and motion. But we know not the immediate mode of muscular motion, any more than how, in inanimate matter, it causes the vibrations of an earthquake.

13. The great question then with us is, how the surface of the earth is put into that vibratory and electric state by heat and dryness? We must needs acquit the internal of the earth from the charge of these superficial concussions. How is the ethereal fire crouded together, or excited, so as to cause them; seeing, in our ordinary electrical experiments, we make use of friction?

But that friction alone does not excite electricity, we know, from the obvious experiment of flint and steel; where the suddenness of the stroke, and hardness of the matter does it. Another method of exciting it, is the letting off a number of great guns; which so crouds the ethereal fire together, as to electrify glass windows; observed by Dr. Stephen Hales. The aurora borealis, australis, all kind of coruscation, meteors, lightning, thunder, fireballs, are the effects, and may reciprocally be the cause, of electricity; but how, in particular, we know not.

Come we to the animal world, we must needs assert, that all motion, voluntary and involuntary, generation, even life itself, all the operations of the vegetable kingdom, and an infinity more of nature's works, are owing to the activity of this electric fire; the very soul of the material world. And, in my opinion, it is this alone that solves the famous question, so much agitated with the writers in medicine, about the heat of the blood. How these, how earthquakes, are begun and propagated, we are yet to seek.

We may readily enough presume, that the contact between the electric and the non-electric, which gives the snap, and the shock, must come from without, from the atmosphere; perhaps by some meteor, that crouds the ethereal fire together, causes an accension in the air, in the point of contact, on the earth's surface; perhaps another time by a shower of rain. We may as readily conclude, that, though the original stroke comes from the atmosphere, yet the atmosphere has no further concern in it: no aerial power, or change therein, can propagate itself so instantaneously over so vast a surface as 4000 miles square: Therefore the impetuous rushing noise in the air, accompanying the shock, is the effect, not the cause. But surely there is not a heart of flesh that is not affected with so stupendous a concussion. Let a man estimate his own power with that which causes an earthquake, and he will be persuaded that somewhat more than ordinary is intended by so rare and wonderful a motion.

That great genius Hippocrates makes the whole of the animal economy to be administered by what we call nature, and nature alone, says he, suffices for all things to animals: she knows herself, and what is necessary for them. Can we deny then that he here means a conscious and intelligent nature, that presides over, and directs all things; moves the ethereal spirit or fire, that moves all things; a divine necessity, but a voluntary agent, who gives the commanding nod to what we commonly call nature; the chief instrument in the most important operations of the vast machine, as well as in the ordinary ones? And this leads us,

14. Lastly, in regard to the spiritual use we ought to make of these extraordinary phenomena, or of our inquiries about them; I shall first observe, that we find abroad, that several of these earthquakes this year have been very fatal. In the last we read of at Philippoli in Thrace, the whole city was destroyed, and above 4000 inhabitants killed. At home, where above half a score separate concussions have been felt, there has not been one house thrown down, one life lost. This ought to inspire us with a very serious reflection about them. We may observe, that if we did but read the works of Hippocrates, Plato, and his followers, of Tully, Galen, and the like ethic writers of antiquity, whilst we study and try the affections of matter, we should improve in philosophy, properly speaking; we should lift up our minds from these earthly wonders, and discern the celestial monitions they present to us.

The original meaning of the word philosophy was rightly applied to moral wisdom; we, who have improved both, should join them both together. By this means we gather the truth of the highest and most excellent philosophy, to be found in those volumes of first antiquity, which we call sacred; and we should adore that divine light which they hold forth to us; especially in a country where the principles of true religion are open and undisguised; where the established profession of it is rational, noble, and lovely; worthy of the moral governor of the world."

END OF THE FORTY-SIXTH VOLUME OF THE ORIGINAL.

Art. I. Of a Fire-ball seen in the Air July 22, 1750. *By Mr. Wm. Smith, of Peterborough. Vol. XLVII, Anno* 1751, p. 1.*

On Sunday, July 22, 1750, 20 minutes before 9, in the evening, was seen near Peterborough, a ball of light, seemingly about the height of the sun when

* As this volume of the original is the first of a new series, when the Royal Society began to publish the Philos. Trans. themselves under the direction of their committee; on which occasion they prefaced the volume with the following advertisement, and which has been since, with little variation, continued to the present time in every volume; it has therefore been thought proper to print this advertisement once for all. It may also be here remarked that another alteration was introduced in the mode of publishing the Transactions, and which has also continued ever since, viz. that in-

about 2 hours high, and larger than a star of the first magnitude; the colour like that of a rocket, when thrown, and in its full glory. It drew a tail of light, to our view about 3½ feet long, which was broadest and brightest next the ball, and grew taper in form, and languid in colour, to its termination. Its course was about north-west to south-west. It moved in a direct line horizontally, and its motion through the air was little swifter than the passage of a pigeon in its flight. It was seen about ¾ of a minute. It was also seen at Bourn, which is

stead of the former way in small numbers, of a few sheets each, the last of which was N° 496, they have been ever since published in whole volumes once a-year, or else in half volumes each half year; having the papers numbered in a series of articles from the beginning to the end of each volume. The annual advertisement, here first given, is as follows:

Advertisement.—The committee appointed by the Royal Society to direct the publication of the Philos. Trans. take this opportunity to acquaint the public, that it fully appears, as well from the council books and journals of the Society, as from the repeated declarations which have been made in several former Transactions, that the printing of them was always, from time to time, the single act of the respective secretaries, till this present 47th volume. And this information was thought the more necessary, not only as it has been the common opinion that they were published by the authority, and under the direction of the Society itself, but also because several authors, both at home and abroad, have in their writings called them the Transactions of the Royal Society. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the Transactions had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the royal charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought adviseable, that a committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future Transactions; which was accordingly done on the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, on any subject, either of nature or art, that comes before them. And therefore the thanks which are frequently proposed from the chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

north-west 12 miles off, in the same manner. It must consequently be at a great height, though it did not seem to be so, as the people in Borough-Fen, which lies north-east of the place where he was when he saw it, saw the same on the same hand as he did, and its form and course in the same manner.

II. Of the same Meteor. By Mr. Henry Baker, F.R.S. p. 3.

Mr. William Arderon, F.R.S. wrote, that the same meteor was seen at Norwich by thousands of people, on Sunday the 22d of July, at 9 in the evening; the appearance of which is exhibited in fig. 1, pl. 3. Its direction was, as near as he could guess, from north to south, moving with great velocity. When due east of him, its altitude was about 30 degrees; at which time the great distinctness of its figure made him imagine it was not above 2 or 3 miles from him. The splendour and beauty of its nucleus, particularly on the fore part, surpassed all the fires he ever saw, being of a bright silver colour; its tail was of the colour of a burning coal, though something fainter. Its head, or nucleus, appeared to him under an angle of somewhat more than 2°, and its tail of about 21°. He lost sight of it in a cloud, not above 20° above the southern part of the horizon, into the middle of which it entered; but a friend of his, being about 4 miles more southward, saw it again, after it came out of this cloud, till it entered into another.

III. Thermometrical Tables and Observations. By John Stedman, M.D. p. 4.

This journal of the heat was kept during the encampment in Dutch Brabant, in the last year of the war, viz. 1748, and is chiefly remarkable in showing the difference between the heat in the tents and in the open air.

Dr. S. observed, 1. That in tents the heat frequently varies 20, 25, and sometimes 30°, in 24 hours; reckoning by Fahrenheit's scale.

2. That the uneasiness, felt on great changes of heat and cold, depends more on the sudden change from the one to the other, than on the excess of either; having often seen, in a long course of sultry weather, men sitting unconcernedly in their tents, when the air they breathed in was raised to about 90°; and the same men in winter standing in the open air with no warmer clothes, and yet without any complaint, though the cold was some degrees below the freezing point. Whence it appears, that, if such a change of air be gradual, the same person can, without any uneasy sensation, bear the difference of 60, 62, or 64° of heat.

3. That we are able to endure a greater degree of heat, than what has been hitherto thought enough to kill animals, as will appear from the following example. A soldier being confined to a tent called the standard-guard, while the weather was so extremely hot, that the thermometer rose within the tent 103 or

104°; on the second day his pulse was quick and full, his mouth foul, and he complained of thirst, a nausea, and head-ach. A thermometer being then kept for some time in his arm-pit, rose to 106°. On the third day all the symptoms increased, though the thermometer applied to his body, rose no higher than the day before; but on Dr. S. representing the danger from the heat, he was enlarged, and immediately recovered. The heat in this instance was several degrees beyond what the learned professor Boerhaave thought sufficient to coagulate the blood.

4. That a damp air (cæteris paribus) gives a sensation of greater heat or cold than a dry air, viz. a sensation of greater heat, when the mercury is about 70° or upwards; and of cold, when about 50°, or below that point.

5. That we are able to endure the open air, when heated to a degree considerably greater than the air of a room, that is heated by a fire; and, since one may stay some hours in a bagnio, where the heat is at 100°, we may conclude, that the open air, heated to that degree, will be suffered with less uneasiness, than when it is so confined.

6. That medicines, for whose operation a pretty high degree of heat is necessary, cannot be taken safely, where the heat is very variable, though it should not be less than the degree requisite for the working of such medicines. Thus a mercurial salivation may be carried on safely, where the heat is kept from 66 to 72°; but, were the heat suddenly to vary 15 or 20°, the change would be dangerous, though the heat was not to fall below 66°.

7. That the body is sometimes differently affected, according to the different constitutions of the air; though the air remains the same, so far as we can judge, with regard to heat, humidity, and gravity.

8. That, when the thermometer is high, our bodies are very sensible of a small addition of heat: but it is uncertain, whether this proceeds from the heat being near the greatest degree we can bear; or, that a greater proportion of heat is requisite to raise the thermometer the same number of degrees after it is high, than when it is low. If this be the case, then, in graduating the thermometers, the degrees ought to be marked shorter, proportionally to the height of the mercury; but in what proportion, is not yet discovered.

IV. A General Method for exhibiting the Value of an Algebraic Expression involving several Radical Quantities in an Infinite Series: wherein Sir Isaac Newton's Theorem for involving a Binomial, with another of the same Author, relating to the Roots of Equations, are demonstrated. By T. Simpson, F.R.S. p. 20.

Among all the great improvements, which the art of computation has in these last ages received, the method of series may be justly deemed one of the most considerable; since not only the doctrine of chances and annuities, with some other branches of the mathematics, depend almost entirely on it; but even the business of fluents, of such extensive use, would, without its aid and concurrence, be quite at a stand in a multitude of cases, as is well known to mathematicians.

It is for this reason, that the celebrated binomial theorem, for converting radical quantities into serieses, is ranked by many among the principal discoveries of its illustrious author; seeing, by it, a vast number of fluents are found, that would otherwise be impracticable: nor is there any case, however complex, to which it may not be extended.

It is true, when 2 or more compound radical quantities are involved together, the operation, by having two or more serieses to multiply into each other, becomes very troublesome and laborious; and, what is worse, the law of continuation, by which a part of the labour might be avoided, is exceedingly hard, if not impossible, this way to be discovered. In the following paper something is attempted towards obviating the said inconveniencies.

Prob. 1.—To find a series exhibiting the value of $(1+\frac{x}{a})^m \times (1+\frac{x}{b})^n \times (1+\frac{x}{c})^p \times (1+\frac{x}{d})^q$, &c. in simple terms; x being indeterminate, and a, b, c, d, m, n, p, &c. any given numbers, whole or broken, positive or negative.

Put $u = (1+\frac{x}{a})^m$, $w = (1+\frac{x}{b})^n$, $y = (1+\frac{x}{c})$, $z = (1+\frac{x}{d})$, &c.

Also let $\triangle = uwyz$, &c. (= the quantity proposed)

Then, in fluxions, $\dot{\triangle} = \dot{u}wyz$ &c. $+ u\dot{w}yz$ &c. $+ uw\dot{y}z$ &c. $+ uwy\dot{z}$ &c. &c. Which equation, divided by the preceding one, gives

$$\frac{\dot{\triangle}}{\triangle} = \frac{\dot{u}}{u} + \frac{\dot{w}}{w} + \frac{\dot{y}}{y} + \frac{\dot{z}}{z} \text{ \&c.}$$

But since $u = (1+\frac{x}{a})^m$, we have $\dot{u} = m\dot{x} \times (1+\frac{x}{a})^{m-1}$; and therefore $\frac{\dot{u}}{u} = \frac{m\dot{x}}{a} \times (1+\frac{x}{a})^{-1} = \frac{m\dot{x}}{a} \times (1 - \frac{x}{a} + \frac{x^2}{a^2} - \frac{x^3}{a^3} + \frac{x^4}{a^4}$ &c.) by division. And in the same manner it appears, that $\frac{\dot{w}}{w} = \frac{n\dot{x}}{b} \times 1 - \frac{x}{b} + \frac{x^2}{b^2}$ &c. &c.

Hence, our equation, by substituting these values, becomes

$$\frac{\dot{\triangle}}{\triangle} = \dot{x} \times \begin{Bmatrix} \frac{m}{a} - \frac{mx}{a^2} + \frac{mx^2}{a^3} - \frac{mx^3}{a^4} \text{ \&c.} \\ \frac{n}{b} - \frac{nx}{b^2} + \frac{nx^2}{b^3} - \frac{nx^3}{b^4} \text{ \&c.} \\ c \quad - \frac{px}{c^2} + \frac{px^2}{c^3} - \frac{px^3}{c^4} \text{ \&c.} \\ \text{\&c. \quad \&c. \quad \&c. \quad \&c.} \end{Bmatrix}$$

Put $P = \frac{m}{a} + \frac{n}{b} + \frac{p}{c} + \frac{q}{d}$ &c.

$Q = \frac{m}{a^2} + \frac{n}{b^2} + \frac{p}{c^2} + \frac{q}{d^2}$ &c.

$R = \frac{m}{a^3} + \frac{n}{b^3} + \frac{p}{c^3} + \frac{q}{d^3}$ &c.

&c. &c.

Then it will be

$$\frac{\dot{\Delta}}{\Delta} = \dot{x} \times (P - Qx + Rx^2 - Sx^3 + Tz^4 - Vx^5 \text{ \&c.})$$

Assume $\Delta = A + Bx + Cx^2 + Dx^3 + Ex^4$ &c. let this value, with that of $\dot{\Delta}$, be substituted in the last equation: whence, by comparing the homologous terms, there will come out

$$B = PA$$
$$C = \frac{PB - QA}{2}$$
$$D = \frac{PC - QB - RA}{3}$$
$$E = \frac{PD - QC + RB - SA}{4}$$
$$F = \frac{PE - QD + RC - SB + TA}{5}$$
$$G = \frac{PF - QE + RD - SC + TB - VA}{6}$$

&c.

Where the law of continuation is manifest, and where it is also evident, that the value of A, the first term of the required series, must be a unit, because, when $x = 0$, then the given expression becomes $1^m \times 1^n \times 1^p = 1$. Q. E. I.

Corol. 1.—If a be taken $= 1$, and n, p, q, &c. each $= 0$; then will $P = m$, $Q = m$, $R = m$, &c. And therefore

$A = 1$; $B = m$; $2C = mB - mA$;

$3D = mC - mB + mA = mC - 2C$;

$4E = mD - mC + mB - mA = mD - 3D$

&c.

Consequently $C = \frac{m.m-1}{2}$, $D = \frac{C \times m-2}{3} = \frac{m.m-1.m-2}{2.3}$, $E = \frac{E \times m-3}{4} = \frac{m.m-1.m-2.m-3}{2.3.4}$ &c.

Hence, in this case, $1 + mx + \frac{m.m-1}{2}x^2 + \frac{m.m-1.m-2}{2.3}x^3$ &c. $(= A + Bx + Cx^2$ &c.$) = (1 + x)^m$: which series is the same with that given by Sir Isaac Newton.

Corol. 2.—If a be taken $= \frac{1}{a}$, $\beta = \frac{1}{b}$, $\gamma = \frac{1}{c}$, &c. and $z = \frac{1}{x}$; then will the proposed expression be transformed to

$$(1 + \tfrac{\alpha}{z})^m \times (1 + \tfrac{\beta}{z})^n \times (1 + \tfrac{\gamma}{z})^p \times (1 + \tfrac{\delta}{z})^q \text{ \&c.}$$

Also $P = m\alpha + n\beta + p\gamma +$ &c.
$Q = m\alpha^2 + n\beta^2 + p\gamma^2 +$ &c.
$R = m\alpha^3 + n\beta^3 + p\gamma^3 +$ &c.
&c.

And consequently $(1 + \frac{\alpha}{z})^m \times (1 + \frac{\beta}{z})^n \times (1 + \frac{\gamma}{z})^p \times (1 + \frac{\delta}{z})^q$ &c. $= A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3}$ &c. where $A = 1$, $B = PA$, $C = \frac{PB - QA}{2}$ &c. as before. Which equation or theorem answers in case of a descending series.

Corol. 3.—Hence, if each of the quantities m, n, p, &c. be taken equal to unity, and their number be denoted by v; then will

$(1 + \frac{\alpha}{z}) \times (1 + \frac{\beta}{z}) \times (1 + \frac{\gamma}{z}) \times (1 + \frac{\delta}{z})$ &c. be $= A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3}$ &c.

Which equation, multiplied by z^v, gives $(z + \alpha) \times (z + \beta) \times (z + \gamma) \times (z + \delta)$ &c. $= Az^v + Bz^{v-1} + Cz^{v-2} + Dz^{v-3}$ &c.

Whence it appears, that $(z - \alpha) \times (z - \beta) \times (z - \gamma) \times (z - \delta)$ &c. is $= Az - Bz^{v-1} + Cz^{v-2} - Dz^{v-3}$ &c. Where $A = 1$, $B = PA$, $C = \frac{PB - QA}{2}$, $D = \frac{PC - QB + RA}{3}$, &c. as before; P being in this case $=$ sum of all the quantities α, β, γ, δ, &c. $Q =$ the sum of all their squares; $R =$ the sum of their cubes, &c. &c.

Corol. 4.—Since α, β, γ, δ, &c. are the roots of the equation, $z^v - Bz^{v-1} + Cx^{v-2} - Dz^{v-3}$, &c. $= 0$; it follows, that, if B, C, D, E, &c. be given, the sum of those roots (P); the sum of their squares (Q), and the sum of their cubes (R) &c. will also be given from the foregoing equations: whence will be had

$P = B$
$Q = + PB - 2C$
$R = - PC + QB + 3D$
$S = + PD - QC + RB - 4E$
$T = - PE + QD - RC + SB + 5F$
&c. &c.

where the law of continuation is obvious.

These values are the same with those given (without demonstration) by Sir Isaac Newton, in his Universal Arithmetic, for finding when some of the roots of an equation are impossible.

Prob. 2.—To find a series expressing the value of

$(1 + \frac{x}{a})^m \times (1 + \frac{x^2}{b})^n \times (1 + \frac{x^3}{c})^p \times (1 + \frac{x^4}{d})^q$, &c.

By putting $u = (1 + \frac{x}{a})^m$, $w = (1 + \frac{x^2}{b})^n$, &c.; and proceeding as in the last problem; there will be had

$\frac{\dot{u}}{u} = \frac{m\dot{x}}{a} \times (1 - \frac{x}{a} + \frac{x^2}{a^2} - \frac{x^3}{a^3}$ &c.)

$\frac{\dot{w}}{w} = \frac{2ux\dot{x}}{b} \times (1 - \frac{x^2}{b} + \frac{x^4}{b^2} - \frac{x^6}{b^3}$ &c.)

&c. &c.

Whence, making

$P = \frac{m}{a}$, $Q = \frac{m}{a^2} - \frac{2n}{b}$, $R = \frac{m}{a^3} + \frac{3p}{c}$, $S = \frac{m}{a^4} + \frac{2n}{b^2} - \frac{4q}{d}$, $T = \frac{m}{a^5} + \frac{5r}{e}$, &c. and assuming $A + Bx + Cx^2 + Dx^3 + Ex^4$, &c. to express the series sought, the several values of A, B, C, D, &c. will be exhibited by the very equations brought out in the resolution of the preceding problem.

V. On the Use of the Bark in the Small-pox. By Geo. Bayly, M. D., p. 27.

An account is here given of a gentlewoman of a fat, corpulent habit, who at the age of 73 had the small-pox in the natural way. During the first 3 days of the eruption the patient went on well; but on the 8th day the pustules were at a stand; they sunk in, and her life was despaired of. At this period Dr. B. directed blisters to be applied to the legs, and prescribed ʒss. of the powder of Peruvian bark, and a few grains of serpent. Virginiana every 3 hours; by the use of which the pustules were made to rise and brought to a maturation. The bark and serpentaria were afterwards given in decoction. By a long continuance of these medicines (with the use of the lancet once, and with the help of occasional purges) the patient was restored to health.

To this is subjoined the case of a healthy young man, who, in July 1746, had the small-pox by inoculation. The eruption came on at the right time; but, 3 or 4 days after, in dressing the incisions, 3 or 4 purple spots were observed about them, which occasioned Dr. B.'s being called in. The pustules, which were very numerous, were here and there livid, and in the arms and thighs of a dark colour, tending towards a mortification. He immediately prescribed ʒss. of bark to be given, and repeated once in 3 hours; which was accordingly done for 11 days successively; during which time he took 47 doses of bark, viz. in all, 3 oz. wanting ½ a dr.

VI. A Method of making Artificial Magnets without the Use of, and yet far superior to, any Natural ones. By John Canton, M. A., and F. R. S.* p. 31.

Procure a dozen bars; 6 of soft steel, each 3 inches long, one quarter of an

* Mr. Canton, a very ingenious natural philosopher, was born at Stroud, in Gloucestershire, 1718; where his father, a broadcloth weaver, at a proper age put him to learn his own business. But young C. having at school acquired some knowledge in the elementary branches of mathematics, and a taste for philosophical subjects, he spent his time by stealth in reading books of that kind. His singular habits and early acquirements procured him the notice of several learned men, and among others the Rev. Dr. Henry Miles of Tooting, who prevailed on the father to permit the young man to come up to London to try his fortune there; which he accordingly did in 1737, when

inch broad, and $\frac{1}{20}$ of an inch thick, with two pieces of iron, each half the length of one of the bars, but of the same breadth and thickness; and 6 of hard steel, each $5\frac{1}{2}$ inches long, half an inch broad, and $\frac{3}{20}$ of an inch thick, with 2 pieces of iron of half the length, but the whole breadth and thickness of one of the hard bars: and let all the bars be marked with a line quite round them at one end.

Then take an iron poker and tongs, or 2 bars of iron, fig. 2, pl. 3, the larger they are, and the longer they have been used, the better; and fixing the poker upright between the knees, hold to it near the top one of the soft bars, having its marked end downward, by a piece of sewing silk, which must be pulled tight with the left hand, that the bar may not slide: then grasping the tongs with the right hand a little below the middle, and holding them nearly in a vertical position, let the bar be stroked by the lower end, from the bottom to the top, about 10 times on each side, which will give it a magnetic power sufficient to lift a small key at the marked end: which end, if the bar was suspended on a point, would turn toward the north, and is therefore called the north pole, and the unmarked end is, for the same reason, called the south pole of the bar.

Four of the soft bars being impregnated after this manner, lay the other two, (fig. 3) parallel to each other, at the distance of about one-fourth of an inch, between the two pieces of iron belonging to them, a north and a south pole against each piece of iron: then take 2 of the 4 bars already made magnetical,

he articled himself for 5 years as an assistant to Mr. Watkins, master of the academy in Spital-square: on the expiration of which period, in 1742, he was taken into partnership with that gentleman, whom he afterwards succeeded in the school, and there continued till the time of his death, in 1772, in the 54th year of his age.

Mr. C. being a man of very genteel and modest behaviour, he gained the respect and acquaintance of the most eminent philosophers of his time; with whom he ardently entered on the pursuit and improvements of the then fashionable topics in philosophy; as electricity, magnetism, lightning, &c. in many branches of which he made considerable improvements and discoveries. In 1749 he was engaged, with his friend Mr. Robins, and Mr. Ellicot, in making experiments on the height to which rockets can ascend, and the distance at which their light can be seen. His paper above printed, on making artificial magnets, procured his election as a fellow of the R. S. and the present of their gold medal: and the same year he was complimented with the degree of M. A. by the university of Aberdeen. And in 1751 he was chosen one of the council of the R. S. an honour which was twice repeated afterwards. In 1752 he, first of any person in England, verified Dr. Franklin's hypothesis of the similarity of electricity and lightning, by drawing electric fire from the clouds during a thunder storm. Next year also he communicated to the R. S. another discovery of this kind, viz. the negative and positive states of electricity among the clouds; a discovery also just made in America by Dr. Franklin. In 1762 Mr. C. communicated his curious experiments on the compressibility of water; for which he was a 2d time honoured with the R. S.'s gold medal. Numberless other ingenious papers were written by Mr. C. and published in the Philos. Trans. as well as several other periodical works: a particular account of which, and many other circumstances in Mr. Canton's life, may be seen in Dr. Hutton's Dictionary of Philosophy and Mathematics.

and place them together, so as to make a double bar in thickness, the north pole of one even with the south pole of the other; and the remaining 2 being put to these, one on each side, so as to have 2 north and 2 south poles together, separate the north from the south poles at one end by a large pin, and place them perpendicularly with that end downward, on the middle of one of the parallel bars, the 2 north poles towards its south, and the 2 south poles towards its north end: slide them backward and forward 3 or 4 times the whole length of the bar, and removing them from the middle of this, place them on the middle of the other bar as before directed, and go over that in the same manner; then turn both the bars the other side upward, and repeat the former operation: this being done, take the 2 from between the pieces of iron, and placing the 2 outermost of the touching bars in their stead, let the other 2 be the outermost of the 4 to touch these with: and this process being repeated till each pair of bars have been touched 3 or 4 times over, which will give them a considerable magnetic power, put the half dozen together after the manner of the 4 (fig. 4,) and touch with them 2 pair of the hard bars, placed between their irons at the distance of about half an inch from each other: then lay the soft bars aside; and with the 4 hard ones let the other 2 be impregnated (fig. 5) holding the touching bars apart at the lower end near $\frac{3}{10}$ of an inch, to which distance let them be separated after they are set on the parallel bar, and brought together again before they are taken off: this being observed, proceed according to the method described above, till each pair has been touched 2 or 3 times over. But as this vertical way of touching a bar will not give it quite so much of the magnetic virtue as it will receive, let each pair be now touched once or twice over, in their parallel position between the irons (fig. 6) with 2 of the bars held horizontally, or nearly so, by drawing at the same time the north of one from the middle over the south end, and the south of the other from the middle over the north end of a parallel bar; then bringing them to the middle again without touching the parallel bar, give 3 or 4 of these horizontal strokes to each side. The horizontal touch, after the vertical, will make the bars as strong as they can possibly be made: as appears by their not receiving any additional strength, when the vertical touch is given by a greater number of bars, and the horizontal by those of a superior magnetic power. This whole process may be gone through in about half an hour, and each of the larger bars, if well hardened,* may be

* The smith's manner of hardening steel, whom Mr. C. chiefly employed, and whose bars have constantly proved better than any he could meet with beside, is as follows: having cut a sufficient quantity of the leather of old shoes into very small pieces, he provides an iron pan, a little exceeding the length of a bar, wide enough to lay two side by side without touching each other or the pan, and at least an inch deep. This pan he nearly half fills with the bits of leather, on which he lays the two bars, having fastened to the end of each a small wire to take them out by: he then quite fills

made to lift 28 Troy ounces, and sometimes more. And when these bars are thus impregnated, they will give to a hard bar of the same size, its full virtue in less than 2 minutes: and therefore will answer all the purposes of magnetism in navigation and experimental philosophy, much better than the loadstone, which is well known not to have sufficient power to impregnate hard bars. The half dozen being put into a case (fig. 7) in such a manner, as that 2 poles of the same denomination may not be together, and their irons with them as one bar, they will retain the virtue they have received. but if their power should, by making experiments, be ever so far impaired, it may be restored without any foreign assistance in a few minutes. And if, out of curiosity, a much larger set of bars should be required, these will communicate to them a sufficient power to proceed with, and they may in a short time, by the same method, be brought to their full strength.

VII. An Aurora Borealis observed at the Hague, Feb. 27, N. S. 1750. By Peter Gabré, J. V. D. Phys. Astron. et Math. From the Latin, p. 39.

This very luminous aurora borealis was in the form of an iris, the extremities of which extended from the eastern horizon to the west, and its top towards the south near the zenith, rising near 80° above the horizon. Its breadth at the vertex was about 2°, but narrowed to cusps at the two extremities. The middle of the arc emitted a strong white light; but weaker towards the sides.

VIII. Further Observations on the Cancer Major. By Mr. Peter Collinson, F. R. S., p. 40.

That the cancer major, and all species of crabs, cast their shells, is certain; but at what season of the year, or how frequently, is not exactly to be determined; but it is believed to be annually at the beginning of the summer, sooner or later, according to the greater or less strength of the crab.

There is in the under part of the shell a suture in the form of a crescent, which retains a part of the shell of the same figure. At the time of casting the old shell, this suture opens, and leaves a space sufficient for drawing out the whole body; after which the thorax drops its breast-plate, and then the legs quit their crustaceous coverings. The carcase now is left inveloped with a soft skin like wet parchment. In this helpless state the crab is incapable of moving, but it lies at the bottom of the sea, between the rocks, till its new shell acquires a sufficient hardness and consistence, fit for its defence, and its limbs grow strong enough to bear its weight, and carry it about, to perform its ne-

the pan with the leather, and places it on a gentle flat fire, covering and surrounding it with charcoal. The pan being brought to somewhat more than a red heat, he keeps it so about half an hour, and then suddenly quenches the bars in a large quantity of cold water.—Orig.

cessary functions; while the old shell is left in two parts, that which covered the body in one part, and that which covered the breast and legs, in another.

It happens sometimes that the shell hardens prematurely. In this case the poor animal is made a prisoner, being so cramped, that he cannot disengage himself from his hiding-place, till found by the fishermen, and set at liberty by moving the stones from about him. It is surprizing to consider how a creature can live long confined without any aliment, and yet increase in its dimensions But that the crab will subsist without a sensible decay in the fishermen's pen-pots,* for the space of some months, is very certain. The more healthy and thriving a crab is, the more frequently he casts his shell. But, if he becomes sickly and wasting, the old shell remains on him, till such time as he recovers strength and vigour to cast it.

When the fishermen take a crab, that is not in a good condition, they return it into the sea, and often mark it on the back with a sharp-pointed iron, or top of a knife; and this mark not only remains on the old shell, as long as it continues on, but is found in the same manner impressed or serrated on the new shell; a very strange and surprizing phenomenon, but I am assured it is fact.

If a crab receives a small wound in the very extremity of the claw, he generally bleeds to death, or pines away by slow and insensible leaking of the vital moisture. But if he receives any considerable wound or hurt, that gives him pain, he instantly throws off the offending member, and all is safe, and a new limb soon succeeds to make it again perfect. The leg is always thrown off at the same joint; the blood is stopped by the membrane, that lines that articulation, contracting itself in the form of a purse. If a crab be brought near the fire, he throws off the legs, which feel a painful heat. In like manner if a crab be thrown into hot water, he casts off all his legs together. For which reason, when they are to be boiled, they put them into the pot in cold water, and let it warm very slowly, till the creature gradually die.

The lobster casts its shell much in the same manner as the river crayfish, which are a species of fresh water lobsters.

IX. An Account of the Right Hon. Horace Walpole,† *drawn up by Himself. Dated April* 1750. p. 43.

Mr. W. here states, that Lord Barrington having heard of his complaint,

* These are cages in the sea, made with willow-twigs to keep the crabs in.—Orig.

† Brother to the celebrated statesman Sir Robert Walpole, and uncle to the late Horace Walpole (Lord Orford). He died in 1757. From his earliest years (observes Mr. Coxe in his Memoirs of

sent him the 5th vol. of the Edinb. Medical Essays, containing Dr. Whytt's account of the good effect which the taking of soap and limewater had had in cases similar to his; with ingenious reflections and directions relating to that cruel disease, and the remedy for it. He read them with great satisfaction, and would have immediately fallen into that method, but his relations, touched with the fatal effects, which Dr. Jurin's lixivium* had had on the late Lord Orford, would not suffer him to follow his own inclinations. But while he had a severe fit upon him, he was visited by the Earl of Morton, who, on hearing what was his disorder, gave him an account of the powerful benefit and entire cure, which Mr. Summers had found in voiding the stone, that had tormented him for many years, by adding lime-water to the soap, which he had taken for some time without any success.

This example, by the encouragement of Mr. Graham, his apothecary, fixed his resolution to follow that method; and accordingly before he left the town, he often perused Dr. Whytt's Essay relating to the stone. In March 1747-8, he began at first with taking every day $\frac{1}{2}$ oz. of Alicant soap, made up into pills with the syrup of marshmallows, and drank upon it about a pint of lime-water made of oyster-shells; mixing a spoonful of milk with it, and drinking a spoonful after it, to take away the nauseousness of the tastes.

On the road as he went into the country in May 1748, he had a most severe fit at Newport, making bloody water, with frequent interruptions at short intervals, attended with violent pains, which continued on him to such a degree, that he could not endure the horses to go more than a foot-pace for about 70 miles, till he came home. After his arrival there he was tolerably well for some days; but the least motion in a coach, or even in walking, brought the disorder upon him. He was always entirely easy when he lay in bed, but was obliged, when he got up, to take his couch; and could not venture to move from thence but on necessary occasions. In the mean time he continued to take the soap and lime-water, which by degrees he increased so far, as to take at different

Sir Robert Walpole, vol. i. p. 184) he had been trained to business, under Stanhope, in Spain; under Carleton, when chancellor of the exchequer, and secretary of state; under Townshend, at the congress of Gertruydenberg, and during the negociation for the Barrier Treaty in 1710. At the accession of George I., he was appointed secretary to Lord Townshend, and afterwards secretary to the treasury; and, as envoy to the states general, had conducted with great skill and ability the complicated negociations which took place at the Hague in 1715 and 1716. In 1722 he was deputed as envoy to the Hague, which post he filled with great credit and dignity, and was particularly noticed by George I. as a man of business and address.

* In the medical practice of the present day neither the lixiviom here mentioned, nor lime-water (both which, but particularly the first of the two, possess a causticity which proves hurtful to the stomach,) are prescribed in calculous affections; but in their stead the so called soda-water, in which the alkaline salt is rendered mild by super-saturation with the carbonic acid, (fixed air,) in which state it does not injure the stomach, and may be safely continued a great length of time.

times 1 oz, of soap, and 3 pints of lime-water, every day, observing a very regular diet. After some months he found himself extremely easy in his ordinary motions; but he never ventured to walk far, nor go at all in a wheel-carriage, keeping himself as quiet as he could, till he should be obliged to go to parliament.

Just before he left the country, Mr. Ranby made him a visit; and though he had felt no pain nor symptom of his disease for some time, he advised him not to hazard going to town by any means, unless it were in a litter. However, having caused an easy voiture to be made, he undertook the journey in it the 20th of November 1748, which was regulated by the horses going no faster than a gentle walk, and only 20 miles a day. The cold weather, and the tediousness of creeping so slow, made the coachman sometimes fall into a trot, which he perceived, but finding no inconvenience, did not check his pace. The set stages were observed only the last 2 days, and particularly the last day the coachman drove from Harlow to Whitechapel as full a trot as the horses could well go at any time; and he felt not the least disorder. He took a chair at Whitechapel, and all that winter made use of nothing else, and continued extremely well; but, about 2 months after his coming to town, he found some small uneasiness in making water, and in 2 or 3 days he voided with his urine something of a flat shape about the size of a silver penny, covered with a soft white mucus, which, when it was dry, was plainly of a stony substance; and after that had never been troubled with the least symptom of that cruel disorder: And he found himself so well in the country last year, that, contrary to the advice of all his friends, he undertook in his coach a journey to Chatsworth in Derbyshire from his house in the country, at least 160 miles, to pay a visit to the Duke of Devonshire, the horses going as round a trot as they could conveniently, according to the road; and the last 10 or rather 15 miles, from Hardwicke to Chatsworth, a most rugged and rocky way, they neither spared themselves nor the horses. The great shocks on the stones broke the springs of the coach, but gave him not the least uneasiness, and he had ever after continued, with respect to his former disorder, as well as ever he was in his life; but he had now and then voided, after he had sat a great while in the House of Commons, some re gravel.

X. Extract of the Observations maae in Italy, by the Abbé Nollet, F. R. S. on the Grotta de Cani. Translated from the French by Thomas Stack, M. D. F. R. S., p. 48.

This celebrated grotto is described in numerous books of travels, &c. Dogs exposed to the gas emitted from this cavern are thrown into a state of asphyxia, from which, however, they soon recover on being brought into the open air.

XI. A Letter from the Rev. Patrick Murdocke, F.R.S. concerning the Mean Motion of the Moon's Apogee, to the Rev. Dr. Robert Smith, Master of Trin. Coll. Camb. p. 62.

A warm dispute arose lately at Paris between M. de Buffon and M. Clairaut; the latter pretending that the Newtonian law of attraction is inconsistent with the motion of the moon's apogee; and that its quantity ought not to be expressed by $\frac{1}{x^2}$ of the distance, but by two, or perhaps more, terms of a series, as $\frac{1}{x^2} + \frac{a}{x^4}$; which new doctrine M. Clairaut had got inserted in the memoirs of the Academy, and M. de Buffon had followed him close with another memoir, confuting it. At first it was impossible to judge of the validity of M. Clairaut's reasoning, because he kept his calculus a profound secret. But an absurd consequence of his new law of attraction occurred as soon as M. de Buffon mentioned the thing, that, "if we should put the attraction, expressed by his two terms, of an assumed quantity G, and resolve the equation, there would necessarily arise 2 different values of the distance x, for the same attractive force."

Suspecting therefore, that some error must have slipt into M. Clairaut's reasonings (as he himself afterwards found there had), Mr. M. tried whether, by an arithmetical calculation from Sir Isaac Newton's propositions only, the motion in question might not be accounted for. By Mr. Walmesley's ingenious treatise on the same subject, it appears that however M. Clairaut's hypothesis is given up, yet a notion still prevails as if Sir Isaac Newton's propositions, concerning the motion of the apsides were mere mathematical fictions, not applicable to nature. The following calculation however of Mr. M. shows the contrary.

Of the mean Motion of the Moon's Apogee, according to Sir Isaac Newton.

The rule given by Sir Isaac Newton, in the 9th section of his first book, is to this purpose:

1. That, supposing the common law of attraction, and that a central body T attracts the body P, fig. 8, pl. 3, revolving round it in an orbit nearly circular, with a force as unity; if to this be added a constant force, whose ratio to the former is expressed by c; then the angular velocity of the body P, in an immoveable plane, will be to its angular velocity, reckoned from the apsis of its orbit, in the subdublicate ratio of $1 + c$ to $1 + 4c$, or as $\sqrt{\frac{1+c}{1+4c}}$ to unity. And therefore, if A represent any arc described by the revolving body in an immoveable plane, then $A \times \sqrt{\frac{1+4c}{1+c}}$ will be the corresponding arc in its orbit, reckoned from the apsis. And their difference $A \times (\sqrt{\frac{1+4c}{1+c}} - 1)$, will be the regress of the apsis. But if the force of the central body T be diminished by some constant

force as c, then the sign of c is changed in these expressions; and the direct motion of the apsis will be A $\times (1 - \sqrt{\frac{1-4c}{1-c}})$.

2. And hence, if some foreign variable force, added to, or subtracted from, the central force of attraction, produce a given motion of the apsis, retrograde or direct; it is easy to find a constant force as c, which should produce the same motion.

3. Let S represent the sun, at an immense distance, T the earth, (supposed, for the present, at rest) P the moon's place in her orbit ADBC, in which C, D, are the quadratures, A, B, the syzygies: then if PK, parallel to AB, and cutting TC in K, be produced till KL is double of PK; and LM parallel to PT meet AB produced in M; LM and MT will represent the disturbing forces of the sun, by which the moon is urged in the directions PT, MT. See Princip. lib. i. prop. 66, and lib. iii. prop. 25, 26. And if TR be made perpendicular to LM, the force MT shall be resolved into two forces as RT and MR; of which the latter, MR, taken from LM, reduces the disturbing force, in the direction PT, to their difference LR.

4. Put now PT (= LM) = 1; PK, the sine of the arc PC = s: and then TM (= PL = $3s$): MR :: 1 : s; that is, MR = $3s^2$, and LR, the disturbing force in the direction PT, is as $1 - 3s^2$. When cp, the moon's distance from the quadrature, is an arc of 35° 15′ 52″, in which case $1 - 33^2 = 0$, l and r coincide; and the disturbing force vanishing, the line of the apses becomes stationary. But if the moon's distance from her quadrature be still greater, as at π, then $\mu\varrho$ exceeds $\mu\lambda$; and their difference $\lambda\varrho$ is a force represented by $-(1 - 3s^2)$, acting in the direction Tπ. This force, at the syzygies, is double of TC.

5. Hence, and from § 1, it follows; that c being the sun's disturbing force in the direction CT, at the quadrature; at any other point, as P, it will be $\pm c \times (1 - 3s^2)$. And that writing for c the variable quantity $c \times (1 - 3s^2)$, and A for the fluxion of the arc CP, the fluent of A $\times \sqrt{\frac{1 + 4c \times (1 - 3s^2)}{1 + c \times (1 - 3s^2)}}$ will give the motions of the apsis.

6. The quantity c being $\frac{1000}{178725}$ of the earth's mean attractive force at the moon; by computing as above, it will be found, that while the moon moves from C to p, through an arc of 35° 15′ 52″, the total regress of the apsis is to the arc Cp, as .005404 (= n) to unity; and that the sum of its direct motions, while the moon moves from p to A, is to the arc pA, as .0105707 (= N) to unity. It will be found likewise, by the inverse operation hinted in § 2, that putting $k = .00362552$, and K = .0069611; $+ k$ and $-$ K are forces, which acting constantly, the one from C to p, the other from p to A, would produce the same motions of the apsis.

7. The quantities k and K might have been found, pretty near the truth, only

by summing the ordinates $1 \angle$ R, or $1 - 3s^2$, on the arc A: in which case we should have had $k = c \times .648869 = .00370925$, and $K = c \times 1.24018 = .006939$: and the motions thence computed would not have been much different from their just quantity. This however is mentioned, not as if the method itself were sufficiently exact; but to show that if hereafter, in cases where the limits of the forces are incomparably narrower, we shall, instead of summing the momenta, make use of a mean force determined in a like manner, there is no sensible error to be apprehended.

8. Hitherto we have considered the body T, round which P revolves, as quiescent; and it is thus that authors have always considered it: though the case in nature, to which they meant to apply Sir Isaac Newton's rule, is widely different. The earth and moon revolve about their common centre of gravity; their distances from which being inversely as their masses, and the forces, by which either is attracted by the other, as also the forces of the sun to disturb their motions, being in the same ratio; it follows that the earth, in her motion round the common centre of gravity, will suffer disturbances every way similar to those of the moon. And the whole motion of the apsis of the moon's orbit, resulting from the two disturbing forces, will be nearly the double of what either of them could produce separately, round a fixed centre.

9. To determine this, we may conceive the earth as revolving in an orbit already in motion from the sun's disturbing force on the moon; the retrograde motion of the orbit, while the earth moves from C to p, being $n \times Cp$; and the direct motion, for the rest of the quadrant, being $N \times pA$; hence it will follow, that the disturbing force, $= k$, affects the earth's motion through an arc of her orbit equal to $Cp \times (1 + n)$; and the force $-$ K acts through the arc $pA \times (1 + N)$. And the motions of the apsis being in the same ratios, if r be the regress of the apsis of the moon's orbit (determined as in § 6) and p its progress; the regress of the apsis of the earth's orbit will be $r \times (1 + n)$, and its direct motion, $p \times (1 - N)$. That is, the whole motions of the apsis, resulting from the sun's action on the earth and moon together, will be $(R =) \ r \times (2 + n)$, and $(P =) \ p \times (2 - N)$; and the motions to be ascribed to either arc, $r \times (1 + \frac{1}{2}n)$, and $p \times (1 - \frac{1}{2}N)$.—Now p, found as above, being $2082''.9$ and $N = .0105707$, P is $4143''.8$. And the same way, $R = 1375''.7$: whose difference P $-$ R multiplied by 4, that is, $4 \times 2768'' = 11072'' = 3^\circ\ 4'\ 32''$, is the direct motion of the apsis in a revolution.

First correction for the moon's variation. Fig. 9.—10. In the foregoing calculation, it is supposed, that the moon's orbit is nearly circular, more nearly indeed than it possibly can be, even abstracting from its excentricity. For though the moon had been projected with a direction and force to make her describe a circle round the earth, as EOL, the action of the sun would have changed this orbit into an oval, as OADBC; whose greatest diameter, passing through the quadratures CD, is to the least, as $70\frac{1}{24}$ to $69\frac{1}{24}$. The reason and determination of which we have in Princip. lib. iii, prop. 26, 28.

11. That this action of the sun, and the figure resulting from it, must lessen the mean motion of the apogee, is easily shown. For let P be the moon's place in her orbit, when the apsis is stationary; and EOL the circle of her mean motion, cutting the orbit very near the octant O, and PT in *o*; then the accelerating forces of the earth at P and *o*, being inversely as the squares of PT and *o*T, and the sun's disturbing force at the points P, *o*, being in the simple direct ratio of the same lines; *o*T being given, the ratio of the sun's disturbing force at the point P, to the earth's accelerating force at the same point, that is, the quantity *c* in the theorem, will be as the cube of the distance PT; and, a fortiori, in every point of the orbit, from the quadrature C to P, will exceed the mean force at O, and its effect in producing a retrograde motion of the apsis will be greater.

For the remaining part of the quadrant, where the motion of the apsis is direct, the force *c* is indeed greater than its mean quantity from P to O; but, through the whole octant OA, it is continually decreasing as the cube of the distance from T; hence, on the whole, that force, and its effect, from P to A, fall short of their mean quantities at O. Seeing therefore the direct motion is diminished, and the retrograde increased; their difference, that is, the direct motion in the quadrant CPA will be diminished.

But this mean motion will be diminished somewhat also from the inequable description of the areas (in prop. 26, lib. iii); on which account, the cubes of the distance PT must be every where increased, or diminished, in the duplicate ratio of the moments of time in which a given small angle is described, to the mean moment at the octant.*

12. By computing from these principles, it will be found: 1. That the angle CTP, which was of 35° 15′ 52″ in the circle, will, in the oval orbit, be diminished to 34° 43′ 34″. 2. That the ratio of the mean of the cubes of the moon's distances in the arc CP, to the cube of the mean distance, will be expressed by 1.023916 ($= g$), and the like ratio, in the arc PA, by .9852467 ($= h$). 3. Multiplying therefore the forces *k* and — K, found in § 6, by *g* and

* To express the distance PT by *s* the sine of the angle CTP, in an ellipsis not very eccentric; from any point P draw PK an ordinate to the axis CD, and meeting the circumscribed circle in M; draw likewise M*f* perpendicular to TP produced. Then putting TC = 1, TA = *d*, $\frac{1-d}{d} = t$; by conjoining the ratios of TP to PK, PK to PM, PM to P*f*, it will be TP $= \frac{Tf}{1 + ts^2}$: in which, for the variable numerator T*f*, we might, because of the smallness of the angle PTM, write unity; but taking it rather of its mean quantity *m* (= .999987 in the moon's orbit) the distances, whose cubes are to be summed, will be $\frac{m}{1 + ts^2}$. And the ratio of the moments of time to the mean moment, is that of 110.23 to 109.73 + s^2, by prop. 26, lib. iii.—Orig.

by h, substituting the products for c, in the formula, with the arcs CN, and NG, respectively, and finishing the operation as for the circle, the regress in a periodical month will be 5548″.3, and the progress 16489″.8: whose difference is the direct mean motion sought, 3° 21″ 2½′.

13. But nearly the same conclusion may be obtained, and with much less trouble, as follows: In the circle CGD, take CM = 35° 15′ 52″, and through P, the point where MK, perpendicular to TC, cuts the orbit, draw TPN meeting the circle in N. Then, if R be the regress of the apsis in a circular orbit, $R\sqrt{\frac{CM}{CN}}$ will be the regress in the oval CPA.

In like manner, having inscribed in the orbit the circle Amh, and made a similar construction for the rest of the quadrant, $P\sqrt{\frac{Am}{Ah}}$ will be the direct motion in the oval, P being the direct motion in a circle.

Thus, the angle of variation MTN being (in Dr. Halley's tables) 33′ 9″, the subduplicate ratio of CM to CN will be 1.007927, and that of Am to Ah, or of GM to GN, will be .99499, And therefore R (in § 9) will be augmented to 1386″.6, and P diminished to 4123″; whose difference, multiplied by 4, gives 3° 2′ 25⅔″; exceeding the former only by about 4″.

14. The rule is founded in this, that if, from the centre T, a circular arc Ff be described, including in the angle CTN the sector FTf, equal to the elliptic sector CTP, the cube of TF, the radius of this circle, may be taken for the mean of the cubes of the moon's distances in the arc CP. And because the area CPT is to the sector CMT, as PK to KM, or as TA to TC; and To or TE is a geometrical mean between TA and TC, it will easily appear that $TF^3 : To^3 :: CM^{\frac{3}{2}} : CN^{\frac{3}{2}}$. And that P, found from the tables, being (nearly at least) the stationary point in the oval, if the force k be increased in the sesquiplicate ratio of CM to CN, and the arc CN substituted for A in the formula, we shall, by § 1, find the retrograde motion of the apsis.

Now when the constant force $+ k$ is given, the regress R is as the arc A; and when A is given, and k is but a little augmented, R is proportional to k: in general therefore, if k be but a little augmented, R is as $k \times A$. Write Q for the regress in the oval, R standing for that in the circle, already found; and it will be $Q : R :: k \times (\frac{CM}{CN})^{\frac{3}{2}} \times CN : k \times CM$, or $Q = R \times \sqrt{\frac{CM}{CN}}$, according to the rule. The like reasoning for the direct motion.

Second correction for the excentricity. Fig. 10.—15. This equation is inconsiderable, because, though the ratio of the disturbing force, when the moon is at a greater than her mean distance, is more increased than it is diminished in the opposite points of her orbit; this increase is very nearly compensated by the comparative smallness of the angular velocity. Let ADa represent the moon's elliptic orbit, whose centre is C, its axes Aa, Dd, the mean excentricity CT, and the circle of her mean motion MDmd, cutting Aa in M and m. Then, because it is a mean motion we seek, generated while the axis Aa passes through all its different aspects of the sun; we may conceive the direct motion already found, of 3° 2′ 21⅓″, to be produced by a constant disturbing force — K, acting on the moon as she revolves in her circular orbit MDmd; and we have only to inquire how much this force, and its effects, are to be increased, the moon really moving about the same centre T, in the elliptic arc AD; and how much diminished in the arc Da.

16. For which purpose, the constant force K is to be increased in the ratio of the mean of the cubes of the moon's distances, in the arc AD, to the cube of TD or CA, and diminished as the mean of the cubes of the distances in Da. Let the forces resulting be K × G and K × H; and these being substituted in the formula, with the arcs 2DM, or 2Dm, respectively, the sum of the motions found will be the whole mean motion of the apogee, including the correction for the excentricity.

Now K will be found to be .00557337, and the excentricity TC being .05505, and Q the quadrantal arc to radius 1; the ratio G, or, which is the same, the sesquiplicate of the time in which the elliptic arc AD is described, to the time in the circular arc DM, that is, $(\frac{Q + TC}{DM})^{\frac{3}{2}}$, will be 1.110942; and H $= (\frac{Q - TC}{DM})^{\frac{3}{2}}$ = .9001387; hence the whole motion, found as above directed, will be 10962″ = 3° 2′ 42″; the correction, on account of the excentricity, being only 21″.

Multiply 3° 2′ 42″ by 1.080853, and the product 3° 17′ 28″ is the mean motion of the apogee, in a synodical month; exceeding the quantity marked in the tables by no more than 4″.

17. Of the obliquity of the moon's orbit, to the plane of the ecliptic, we take no notice; because though, absolutely speaking, a force in that plane, referred to the moon's orbit, would thence be diminished by about $\frac{3}{1000}$ parts; yet, in the present case, the effect of the obliquity is included in the first determination of the quantity c, from the periodical times of the earth and moon; all but what belongs to the corrections; and which is only 110″. × . 003 = 0″.33, to be subtracted.

18. The force c is itself the effect of the sun's parallax, and the total effect; excepting only a small difference between his action on the moon, when she is waxing or waning, and when she is in the other half of her orbit; neglected as altogether inconsiderable.

On the whole, we may conclude, that, in this, as in the other phenomena of the celestial motions, the principles and rules of Sir Isaac Newton are fully confirmed and verified.

XII. Experiments made on a great Number of living Animals, with the Poison of Lamas, and of Ticunas. By Mons. Herissant, M. D., and F. R. S. Translated from the French, by Tho. Stack, M. D.* p. 75.

Mons. de la Condamine, on his return from the voyage which he made in the interior parts of South America, from the coast of the South Sea to the coasts of Brasil and Guiana, by going down the river of the Amazons, brought to Paris a small quantity of a very dangerous poison, much in use among the Indians of Lamas,† Ticunas, Pevas, and also among the Yameos, who all extract it by fire from divers plants, especially from certain plants which the French call lianes.

Those savages are very dexterous at making long trunks, which are the most common weapon used by the Indians for hunting. To these they fit little arrows made of palm-tree, on which they put a little roll of cotton, that exactly fills the bore of the tube. They shoot them with their breath, and seldom or never miss the mark. This simple instrument advantageously supplies the defect of fire-arms among all those nations. They dip the points of these little arrows, as well as those of their bows, in this poison; which is so active that in less than a minute, especially when fresh, it kills certain animals, from which the arrow has drawn blood.

Mons. de la Condamine says, in the abridged account of his voyage, that "when he arrived at Cayenne, he had the curiosity to try whether this poison, which he had kept above a year, still retained its activity; and at the same time whether sugar was really as efficacious a counter-poison as he had been assured. Both the experiments were performed, he says, in presence of the commandant of the colony, of several officers of the garrison, and of the king's physician. A hen, slightly wounded with one of these little arrows, the point of which had been dipped in the poison 13 months at least, before the trial, blown through a trunk, lived half a quarter of an hour; another, pricked in the wing with one of these arrows, newly dipped in this poison diluted with water, and immediately

* Other experiments on the poison of Ticunas were afterwards made by the Abbé Fontana (Phil. Trans. vol. 70). He confirms Dr. Herissant's account of the deleterious operation of this poison (the Ticunas), when applied to a bleeding wound, or injected into a vein; but contrary to what is related by Dr. H. he did not find any bad effects to be produced by the vapour which arose from it, in boiling or burning.

† Lamas is a Spanish village, or little town, in Upper Peru, situated in about 7° of south latitude, to the west of the river of Guallaga. The native Indians of this district prepare a famous poison for poisoning arrows, different from that of the Yameos, Pevas, and Ticunas, Indian nations on the borders of the river of the Amazons, towards the mouth of the Napo, in 3° or 4° of south latitude. The poison of Ticunas is the most famous of all for its activity. They say, that that of Lamas sooner loses its force, but that it is more proper for certain animals than that of Ticunas. And it is the common opinion, that that of Lamas, being mixed with that of Ticunas, becomes more violent and active by the mixture.—Orig.

drawn out of the wound, seemed to doze a minute after; convulsions soon came on, and, though we had made her swallow some sugar, she expired. A third, pricked with the same arrow, dipped again into the poison, having been instantly assisted by the same remedy, showed no signs of being indisposed, &c."

Mons. H. was struck with amazement on reading these facts; but his surprise was soon followed by a desire of repeating those experiments himself, and even of trying them on different sorts of animals. Mons. de la Condamine, to whom he imparted his intention, offered to satisfy his curiosity, and for that purpose made him a present of a certain quantity of this poison: and the result of the experiments, which he made with this same poison, forms the subject of this memoir.

He begins the detail of those experiments by that of two accidents, which had like to have disabled him from prosecuting the work he had undertaken; having very narrowly escaped death. The first accident happened thus: M. de la Condamine had forewarned him, that when the Indians designed to use their poison, which in colour, consistence, and even in smell, has a great deal of resemblance to Spanish liquorice, they dissolved it in water, and then evaporated it on a slow fire to the consistence of a soft extract. M. H. made this preliminary preparation in a small closet, in which a young lad was actually at work; and he did not think of making him quit it, because he did not imagine, that the poison, of which he intended to make trial, could produce any bad effects, without being introduced into the blood by the opening of a wound. Nor did he then recollect, what M. de la Condamine had told him; which is, that while they are preparing this poison in the country they oblige some criminal old woman to take care of the boiling of this poison, after shutting her up alone in a separate place; so that when this woman dies, it is a sign that the poison is sufficiently boiled, and that it has all the qualities requisite to make it good. But he was soon made sensible of his imprudence: the door of the closet, where the young lad abovementioned staid, was open; and from the next chamber he saw, that the lad, who had been there about three quarters of an hour, sat still, with his arms across. He began to reprimand him for his laziness, but he excused himself by answering, with a trembling voice, that he was sick at heart, and felt himself very faint. It is easy to imagine the uneasiness which this sight gave M. H.; but luckily it cost him no more than the fright. He made the lad come out of the closet immediately, led him down into the yard, and made him swallow a pint of good wine, in which he had dissolved a quartern of sugar. He recovered his strength by degrees, and was soon able to return to his own home, very merry and happy, without the least notion of the danger he had been in. Some days afterwards he came to M. H. and assured him that he had not felt the least indisposition since the day in question.

The fact above related was shocking enough to have made M. H. abandon his project: however curiosity got the better of his fear, and he even took a strong fancy to repeat the experiment. It would have been inhuman, not to say criminal, to make it on any other person but himself: therefore he resolved to run the risk, or rather persuaded himself, that he should run none, because he should be timely enough to flee from the danger, as soon as the effect of the poison should come to a certain pitch. Besides, he was encouraged by the good success of the foregoing example. Therefore he disposed every thing as at the first time, and he staid in the closet. In about an hour's time he perceived his legs to bend under him, and his arms became so weak, that he could scarcely use them. He had but just time enough to come quickly out of the closet, and get down into the yard; where he ordered wine and sugar to be brought him, as he had before done for the young lad. Such was the first danger, which he incurred in preparing the American poison: the second was not inferior to it.

After having dissolved the poison of Ticunas in water, and reduced it to the consistence of an extract in the manner above described, he put it into a phial, which he stopped very exactly, and locked up in a desk till he should have occasion to use it in the experiments he intended to make. He began these experiments on the 6th of June 1748; which was so hot a day, that he stripped to his shirt, and had his breast and arms exposed to the air. In his left hand he held the phial, the cork of which flew up to the cieling with vast rapidity. At the same instant there issued out of this phial a yellowish vapour, of a very penetrating smell, which was soon followed by the extract itself, that spread itself all over the rim of the neck of the bottle. He was so stupified at this unexpected accident, that he imagined (as it was very possible) that the bottle was broken in pieces; and as soon as he saw his hands, arms, and breast, coloured in several places by the poison, which had besprinkled them in the explosion, he looked on himself as a dead man: which must certainly have been the case, if the bottle had burst, and the pieces of glass had scratched or cut him. But luckily that did not happen; and he soon resumed courage: when, after some minutes, he found himself quite as well as before the explosion of the poison, the effect of which is almost instantaneous; and it gave him no other trouble than to wash and dry himself very carefully.

From this accident he learned that this poison, thus prepared, ought not to be put into glass bottles close stopped, but should rather be kept in a glazed earthen pot, covered with paper only; since it was susceptible of so great an effervescence. Therefore he put it into a gallypot; and the experiments, which he made with this same poison a good while afterward, convinced him, that there is no reason to apprehend, that it would lose any of its activity by evaporation.

These two facts plainly show how much precaution ought to be taken, when this poison is to be used. And we shall be the better convinced of it, when we consider that one single drop, conveyed directly into the blood by a puncture, &c. is sometimes sufficient to kill, or at least to cause great disturbance in the animal economy. It is quite otherwise when taken in at the mouth; for then it does no sort of mischief, as he proves in another place.

He then proceeds to the experiments, which he had repeated a number of times on different species of quadrupeds, birds, fishes, insects, and reptiles. But he first observes, that, of all those animals, none but quadrupeds and birds were killed by this poison, as will more particularly appear by the journal of his experiments: the others, viz. the fishes,* the insects,† and the reptiles,‡ were not killed, though several of them seemed to be disordered by it.

M. H. had verified what M. de la Condamine says, in the account of his voyage, relating to the use that may be made of animals killed by this poison, without apprehending any ill consequences to those who eat of them. In effect he had eaten rabbits, which he had killed with this poison, and afterwards made several other persons eat of them; and no one perceived the least indisposition.

On the 6th of June 1748, M. H. made a small wound, of about 3 lines long, in the left hinder leg of a rabbit of 6 months old: into this wound he put a bit of cotton soaked in the poison of ticunas: the creature died suddenly in his hands, without giving the least indication of having felt pain, and even before he could apply a bandage to the wound. The same day he repeated this experiment on 8 other rabbits, and on 4 dogs: they all died in about a minute.

The 7th of June of the same year he dipped the point of a lancet into the poison: and with this instrument he pricked 4 cats and 2 rabbits, some in the head, and the others in the paw, dipping the lancet each time that he pricked an animal. The rabbits died in as short a time as the preceding day; but the cats held out about 3 minutes.

The same day he made a small wound, about 2 lines long, in the right hinder leg of a rabbit, and put into it a small pledget of cotton soaked in the extract of opium diluted in a little spirit of wine: but this did not cause any disorder in the creature; nor did arsenic, which he applied to another in the same manner. In fine, to a third he made use of the extract of white hellebore, and he perceived, that this animal became restless, nearly as he had observed in the animals that died by the effect of the poison of ticunas. However, this rabbit did not die, but fell into a sudden fit of fury, which went off in about 8 minutes.

* Those which Mr. H. employed, were the carp, the eel, the pike, the gudgeon, the barbel, and the tench.—Orig.

† As caterpillars, bees, different flies of 2 and 4 wings, the grillo-talpa, butterflies, May-flies.—Orig.

‡ For example, earthworms, vipers, snakes.—Orig.

He had likewise made trial of this extract on other rabbits, dogs, and cats; and the effect was the same, more or less. Of all the extracts, which he employed, as for example those of henbane, nightshade, tobacco, &c. he found none but that of white hellebore that seemed to raise some little disorder in the animal economy. The essential oil of the lauro-cerasus did not incommode the animals, into whose mass of blood he conveyed it, instead of the poison.

The 8th of June, with a lancet he made a very small incision between the ears of a cat, and with a pencil he put into it a drop of the poison of ticunas mixed with that of lamas: in an instant the creature died between his hands.

June the 9th, he put some of the same poison into small wounds, which he made in different parts of insects, reptiles, fishes; and not one of them died of it.

The same day he made a wound, that penetrated into the cavity of the abdomen of a large cat, without hurting any of the contained parts; and, with a crotchet holding up the integuments, to keep them from touching the abdominal viscera of this animal, that lay on its back, he introduced the end of a funnel, and through it poured into the cavity of the abdomen about $\frac{1}{2}$ dr. of the poison of lamas mixed with that of ticunas. By this management he intended, that the edges of the wound should not be wetted with the poison, and that it should touch nothing but the surface of the abdominal viscera. He made a suture of one stitch to join the lips of the wound, and he kept the integuments constantly suspended, to prevent their touching the poison: and in this he was certain that he succeeded. At first the creature did not seem to suffer much from this operation; but in an hour's time he died, with such violent convulsions in his throat, that it was almost impossible for him to breathe.

June the 10th, he pricked with a lancet the left fore leg of a large fat cat, and put in a drop of the poison of the ticunas. He let this animal run loose about the room, without dressing the wound. By the time he had made a turn round the room, he seemed very restless and timorous: his legs failed him; he lay flat on his belly; and the skin all over his body trembled considerably; the hair of his tail stood up, and his paws were agitated with a frightful tremor. All this while the animal made no noise: in fine, his head fell all at once between his fore legs, and he died in 4 minutes after the insertion of the poison.

June the 12th, he made the same experiment on 2 other cats, and on 3 dogs; these animals seemed to fall sick almost in an instant: the cats had their hair bristled up, and their bodies gathered into a heap: they scratched the ground with their fore feet. The dogs did the same, and all of them had a languishing look, and their eyes bathed in tears; some of them looked at him stedfastly, and made a mournful noise: they were seized with a shivering, and in fine they became paralytic in their feet only; after which they died, turning their head very quick to the right and left, with their mouth wide open. During this scene, he perceived a spasmodic contraction in all the muscular parts of the neck.

July the 15th he pricked a hawk in the left claw: into the puncture he introduced a small drop of the poison of ticunas mixed with that of lamas, and then set the creature at liberty. From that moment it was impossible for him to fly; the most he could do was to perch on a stick, which was within 6 inches of the ground. There he shook his head several times, as if to get rid of something that seemed troublesome in his throat. His eyes were restless, and his feathers were all bristled up. In fine, after several gapings, his head fell all at once between his legs, and in 3 minutes he died thus with his wings expanded. He repeated this experiment on several sorts of birds,* and they all died with pretty much the same symptoms as those above-mentioned, and in as short a time. He made 6 of these birds swallow a good dose of sugar, before inoculating them with the poison: 3 of them escaped death, but the other 3 died very soon. The moment after inserting the poison into 4 other birds, he made them swallow a good deal of sugar; but that did not prevent their dying, almost as soon as those that had taken none. He made other birds swallow sea-salt instead of sugar; and not one of them recovered, whether they took it before or after the application of the poison.

July the 16th he put a little of the same poison into a small wound he had made in the right fore foot of a young rabbit. The moment this operation was performed, he cut off that foot above the place of insertion of the poison. He dressed the stump, and the animal did not die. Some days afterwards, he repeated this experiment on 2 large dogs, and on a lamb; and not one of them died.

July the 20th, he made a tight ligature on the right hinder leg of a young rabbit, in order to see, if he could thereby prevent the poison from penetrating too quick into the mass of blood. That done, he put a drop of the poison of ticunas and lamas into a small wound, which he made below the ligature: and the animal died in less than 2 minutes.

July the 22d, he poisoned the point of a sword with the same poison; and with this sword he pierced the left thigh of a large cat, which died in a minute, without shewing any signs of suffering.

July the 24th, after having introduced some of the same poison into little wounds, made in the legs, and other parts, of several dogs, cats, foxes, and horses, he immediately applied a red-hot iron, or burning charcoal, on the wounds: not one of these animals died: but this operation must be performed very speedily.

* As pigeons, hens, blackbirds, sparrows, ducks, geese, and magpies.—Orig.

July the 30th, he pricked a great number of rats and mice in the feet, with a lancet, after poisoning its point. They all died in less than a minute, after being tormented with a frightful shivering, which was immediately followed by an almost general palsy. The same thing happened to moles, which he made use of for this experiment.

August the 6th, he made a small wound in the left hinder leg of a pig, of 3 months old; and then he put into it 2 drops of the poison of ticunas: this creature died in 6 minutes. He repeated this experiment on 2 young wolves, which died in the same space of time.

August the 7th, he cut off the tip of the ear of 6 puppies, and rubbed the part with the poison of ticunas: not one of these animals died of this operation. Two days after, he shaved the hair off of their backs very close, and rubbed the part with the same poison: they all died in less than 3 minutes.

The 10th, 11th, and 12th of the same month, into small wounds made in different parts of the body of several dogs, cats, polecats, Guinea-pigs, &c. he instilled 7 or 8 drops of blood, which he drew from the vena cava of a dog, which he had killed with the poison of ticunas mixed with that of lamas. These animals did not die indeed, but were plainly indisposed; insomuch that they lost their vivacity, and became very sullen. Eight days after this experiment, he repeated it on these same animals; and then they became still weaker and fainter. In fine, the next day he made it a third time on them, when they languished 4 or 5 days, and then died.

August the 15th, after having put some of the same poison into a wound made in the right hinder leg of 6 horses, one of which was a very vigorous stone-horse, he quickly bled them all in the neck ad animi deliquium: 2 of them escaped with life; but those that were the weakest, and most worn out could not stand against this operation. Two days afterwards, he again pricked those horses, that did not die of the last experiment; and then they died in about 8 minutes.

He made the following observations on these animals, from the insertion of the poison to their death. The muscle, wounded by the incision made for insinuating the poison, was contracted and relaxed alternatively, just as it happens in animals fresh killed: this lasted about 2 minutes; after which these animals seemed restless and impatient, endeavouring to scrape the ground with their fore foot, which he had suspended in the air with a cord, to prevent their running away. Sometimes also they made a sudden effort, as if to get away, which lasted the space of 2 minutes; after which they grew quiet, and amused themselves with nipping the grass, but not in a natural manner. Then their respiration became very difficult; and, though the weather was very hot, there visibly came out of their nostrils a vapour, like that which issues in winter in the time of expiration. A minute after, he observed that these horses endeavoured to rest the suspended leg on something: and, in another minute, he perceived the fore leg, that rested on the ground, beginning to grow weak, and bend; which occasioned these animals to fall forward, and rise up again alternately, with more or less difficulty. In 2 minutes more, their hind legs grew weak, and bent under them, like the fore legs; and in fine, these animals fell down like a dead lump, without being able to rise again, though he whipped them heartily. Then their sides began to work, and the whole habit of the body was seized with a dreadful horror. He whipped them, and pricked them with a pin; but in vain; for they gave no sign of feeling. All the muscles of the trunk and extremities were become paralytic; and none retained their action, but those of respiration, and those of the ears and eyes. These creatures continued in this condition about 2 minutes; after which their respiration became so operose, that each inspiration consisted of 3 successive attempts, and then followed a most precipitate expiration, accompanied with so violent a hiccup, that the body bending double, the hind legs were pulled quite to the fore legs. In fine, this manner of taking in and letting out breath lasted one minute; in which time their eyes were darkened, and death ensued.

He opened the dead bodies of these horses, and observed as follows: the blood was of a deep-brown colour, and spouted out in a full stream, which lasted near a minute, both from the arteries and veins, which he cut. This phenomenon surprized him much, as well as the horse-flayer, who attended him, and assured him that he had never seen the like. The muscles were flaccid, blackish and very cold. The heart was so violently contracted, that, in cutting it across, he could not see any appearance of the ventricles, till he pulled their sides asunder by force. The lungs and liver were stuffed with blood.

In making the small wounds, for introducing the poison, great care must be taken, to avoid cutting any trunk of an artery or vein; because, when that happens, the blood that issues out, carries off a good part of the poison; which makes the animal pine more or less without dying; or, if he dies, it is in a longer or shorter time, according to the quantity of the poison that has got into the vessels, and been mixed with the circulating fluid. This thing happened to him in trying the experiment on a mare, which had been condemned to the lay-stall. This beast lived about 4 hours, because the wound bled abundantly, and hindered the success of the experiment, for the reasons alleged above.

November 18, he took a small steel arrow, and poisoned it with the poison of ticunas mixed with that of lamas. He caused this arrow to be shot into the right hinder leg of a bear, belonging to M. de Reaumur, which he wanted to have killed, in order to put it into his cabinet of natural history. The creature immediately roared out, from the anguish of the puncture; after which he made a tour round the stable, in which he was, without seeming to be in any pain. Soon afterwards he fell on his side, and died in less than 5 minutes, having his throat squeezed, as if he had been strangled.

M. le Chevalier de Grossée had an eagle, which he had kept a good while in his court-yard, and intended to make a present of it to M. de Reaumur, to adorn his cabinet, but wanted to know how to put it to death without damaging the feathers. M. de Reaumur sent him the same arrow above-described, which had been fresh dipped in the poison; it was struck into the wing of this large bird, which dropped down dead in an instant.

Such are the chief experiments, which M. H. made with the poison of ticunas and lamas: and the following are the results of his observations. 1. In almost all the animals, which he killed with the poison of ticunas and lamas, he observed, that in general they seemed to feel little or no pain before dying, by the action of this poison; 2, That before they die, these animals are seized with a sudden and almost universal palsy; 3, Though the colour of the blood seemed to be altered in certain animals, yet we ought not to draw any inference from thence; because in many others the blood had undergone no sort of alteration, either in colour or consistence. 4. That all the muscles are so vastly contracted in the animals thus poisoned, that there is not a drop of blood to be found in them, whatever way you cut into them. These muscles are clammy to the touch, and seem to approach the condition of flesh beginning to be tainted, which feels clammy; 5, That he did not know a more certain rule for determining that an animal died by the energy of this poison, than this state of the flesh which feels clammy immediately after death: but a person must have handled it more than once, if he would avoid being mistaken; 6, That the whole mass of blood, during the action of the poison, is carried in abundance into the liver and lungs. 7. That neither sugar nor sea-salt ought to be regarded as a specific antidote; because the poison operates so quick, that it does not allow time for these drugs to act, so as to prevent death. He had found nothing but red-hot iron applied in time, that cures with sufficient certainty; 8, That the more the animal is of a lively and sanguine constitution, the more speedily and forcibly the poison acts; 9, The lustier and fatter the animal is, the more poison and time also are required for producing the expected effects.

He remarks, that the poison must be dried on the instrument, before it be struck into the animal, which we intend to kill: for if it be liquid, it remains on the outside of the wound, while the instrument penetrates into the flesh: in which case, either the animal dies not at all, or at least with great difficulty: as it happened with regard to a wolf, which did not die, though the arrow above-mentioned was stuck into one of his thighs; because the poison, which it retained from the dip, continued liquid, and remained on the outside of the wound made by the arrow in piercing the flesh. Therefore time must be allowed for the poison to become hard on the instrument, which is intended to be used; that so, entering into the wound together with the weapon, it may be there diluted, and carried in the course of the circulation to those parts which it must effect, in order to cause death.

XIII. The Case of a Woman, from whom the Bones of a Fetus were extracted. By Mr. Thomas Debenham, Surgeon, at Debenham in Suffolk, p. 92.

On the 25th of April 1749, this woman, aged about 34 years, being pregnant of her 8th child, had all the symptoms of a woman in labour. Accordingly a midwife was sent for; who, from the violence of the pains, expected that she would soon be delivered; but, to her great surprize, nothing ensued but a loss of blood, and the pains were considerably abated. A fever immediately came on, which cast her into an excessive faintness, and loss of strength, accompanied with a nausea.

May 26, Mr. D. was desired by her husband to visit her; and by the account she gave him he much suspected that she must have miscalculated with regard to her time; and he proposed to examine her: but she, out of a mistaken modesty, not complying, he contented himself with cooling injections, mild cathartics, and cordial powders, &c.; by the use of which medicines she got better; and, on the 26th of March following, undertook to walk a journey of 15 miles.

He heard no more of her for some time; but on the 27th of April 1750 the pains returned, very much like those of labour; which obliged her husband to call Mr. D. out of bed. He immediately gave her an anodyne, which abated her pains, and composed her to rest.

On the 14th of May she felt a pricking pain in her navel, with a swelling and redness, which in a few days appeared like a boil; when, being desired to in spect the tumour, he applied an emollient cataplasm. The next morning, on removing the dressings, a fetid matter ensued; then dilating the small sinus with the scissars, the scapula of a fœtus presented itself. On the 25th of July, by the direction of a physician, he undertook, by making a circular incision round the navel, to enlarge the orifice into the cavity of the abdomen, in order to extract the fœtus that way: but the woman being very weak, and much emaciated, he could only take off the scapula.

The next day, he extracted one whole arm, some ribs, part of the vertebræ, &c. and the day following the greatest part of the remaining fœtus, except the cranium, which seemed to adhere to the intestines. This determined him to proceed very cautiously, and not to attempt the removal of it at once, but piecemeal, and by degrees, as opportunity would permit; which he did with his forceps: but, notwithstanding all his care, the sharp edges of the broken pieces of the cranium tore the intestines, so that the fæces issued from the wound at every dressing for several weeks together.

The wound was daily dressed with dry lint, spirituous fomentations, and cataplasms. Injections, made of sack and warm water, were found of great use, thrown in in large quantities; and (what was well worth observation) several parts of the bones, as the tibia, fibula, &c. were discharged by the vagina.

By the means above-mentioned, and proper bandages, the wound was thoroughly deterged, incarned, and, by the use of epulotics, completely cicatrized; and the woman was perfectly recovered, and afterwards grew fat.

After the discharge of the whole fœtus, the patient had milk in her breasts, as on a natural delivery.

XIV. New Discoveries relating to the History of Coral, by Dr. Vitaliano Donati.† Translated from the French, by Tho. Stack. M. D. F. R. S.* p. 95.

Coral is a marine vegetation, in shape nearly resembling a shrub stripped of its leaves. It has no roots, but is supported on a broad foot, or basis, which adapts itself like wax, and sticks to any body in all its parts, so firmly, that it is impossible to disengage it. The shape of this foot is not always the same; but, it mostly approaches to rotundity, as n, n, fig. 1, pl. 4. Its use is to hold the coral fixed, and support it; not to nourish it: since there are found pieces of coral, with their feet broken off, which nevertheless continue to live, to grow, and to propagate, at the bottom of the sea. From this foot arises a trunk, generally single, the greatest thickness of which seldom exceeds an inch.

Out of this trunk the branches shoot, which commonly are few in number; and they afterwards divide into several smaller and slenderer branches. The branches are mostly disjoined, and separate; sometimes two or more branches spring from the foot united and parallel, and as it were clung together so intimately, that the place of their union cannot be distinguished. Frequently two branches adhere and unite in the same manner, in whatever place they happen to touch: and from two branches thus united, there sometimes arises afterwards only a single branch. If a shell happens to stick to the trunk or branches of the coral, it is in time surrounded and covered, either in part, or in the whole, with the same coralline matter to which it stuck.

The greatest height to which coral rises in the Adriatic, is a Paris foot, or a little more. And even this height is very rare in that sea. The trunks, as well as the branches, are commonly round; yet frequently some are flatted and

* Red coral. *Isis nobilis.* Lin. *Gorgonia nobilis.* Lin. Gmel.

† Author of an ingenious work, entitled the Nat. Hist. of the Adriatic Sea, written in Italian and printed at Venice 1750, with numerous plates.

broad. The foot, trunk, and branches of this sea-production, are of one uniform matter: being formed of a substance homogeneous in all its parts, and of a bark or coat. The substance forms the inner part of the coral; and this, even at the bottom of the sea, is nearly as hard as marble. At the ends of the branches it is not so hard as the bark; in some places near the ends it is of equal hardness with it; but in the thick branches and trunk it is harder.

This substance, being observed by a microscope, in corals of one colour, as the red, and those which are not corroded by worms, appears uniform, smooth, without spots of other colours, without holes or pits, being quite even, hard, and capable of a perfect polish. But it is otherwise, in corals of more colours than one; as sometimes in those of a yellowish rose-colour, and those of a rose-colour. For in some of these, the transverse sections exhibit different lines, or annular bands (fig. 2, s, s, s, s,) of which one part is a rose-colour, and the other yellowish, others white, and others more or less charged with colour, which form concentric circles, a, like the coats of an onion. The same sort of annular lines is observable in red coral a little burnt; but they are of a grey colour, and parted asunder by a line of a deep brown grey, (s, s, s.)

When this substance, though very hard, happens to be stripped of its bark, either by age, or accident, it is liable to a sort of teredo, or worm; a small animal, that enters into the body of the coral by very small holes, (fig. 3, a, a,) gnaws its inside, and makes itself roundish cells, (s, s.) These cells have a communication with each other, (a, a,) and are separated by very thin partitions, which weakens the coral extremely, and makes it brittle and improper for any work. There is also another worm, which passes through the coral transversely from side to side, and in right lines, by straight cylindrical holes. Even the hardest marbles, lying in the sea, are liable to be corroded in the same manner. The surface of the substance of coral is furrowed and wrinkled (fig. 4 and 2, e, u.) The wrinkles begin from the foot, and ascend, always nearly parallel, to the trunk and branches. However, these wrinkles are not so deep in the slender branches, and sometimes are not visible there: but they are always more elevated, and more considerable, in the thick branches and trunk: they are not smooth, but uneven, with knobs or bumps on them, and the surface composed of very small hemispheres.

This substance of the coral, being exposed to a strong fire, is reduced to a very fine ash-coloured powder. As common ashes, when taken clean from burning charcoal, and examined by a microscope, exhibit a sort of skeleton, composed of the fibres and vessels of the wood; so the ashes (of the substance) of coral sufficiently show of what sort of parts it is composed. The microscope discovers in it ashes formed of very small white corpuscles, united in clusters; each one of which is nearly spherical. The ashes of the bark of the coral are of the same shape and colour; so that the substance of coral agrees with its bark in the primitive and constituent parts, which seem to be the same in both.

In pieces of coral broken transversely, are often observed some prominent wrinkles, which disengaging themselves from the exterior wrinkles above-mentioned, run towards the centre (fig. 2, u.) Hence it plainly appears, that there is an affinity or connection between the interior and exterior wrinkles. To the exterior wrinkles, and to the whole outer surface of the hard part of the coral (fig. 2, a, s, e, s,) there is closely attached a white or pale pellicle (fig. 2, g, fig. 5, n, n,) which is pretty soft, and composed of vascular and follicular minute membranes, which, by their interlacing, form a reticular body. The whole is accompanied with small vessels, which contain a whitish juice, diffused through all the folliculi or membranulæ; which have also attached to them certain very small red corpuscles, united together by means of other membranulæ.

These corpuscles are nearly of a spherical figure, and in size and shape exactly like those of the ashes of the coralline substance, and of the bark: so that we may properly say, that these little bodies constantly remain entire, even after the action of the fire; having undergone no other change but in their colour. In this pellicle (fig. 5, n, u,) the globular corpuscles are not numerous, but the greatest part of the said pellicle is occupied by very white membranes, from which it takes its colour, and not from the red globular corpuscles. This pellicle, lying immediately on the coral, deposits the red corpuscules, and adapts them to it: and thence it is that the wrinkles are covered as it were, by extremely small hemispheres; and these infallibly show the formation of the coralline substance. If any one should ask, whence can these little spheres derive their origin? Dr. D.'s answer would be, without hesitation, from the polypi of the coral. And the reason is, that if these polypi produce their eggs, as will be shown in the sequel, covered with such corpuscules, we may justly infer that corpuscles of the same nature, wherever they are found, are formed by the same polypi.

To this white pellicle is attached the bark of the coral (fig. 2, t, t, E, s, s,) which is soft, of a vermillion colour, or of a brighter colour than the coralline substance. It is formed of very fine membranulæ, or net-work; to which are annexed, and reciprocally fastened, the red globular corpuscles, which cause its deeper colour. It is along this bark, that cylindrical vessels (fig. 2, t, t, t, t, fig. 5, i, fig. 6, n,) are observed to run lengthways of the coral; which appear by the microscope to be parallel to each other, and out of which issue laterally other vessels infinitely small, (fig. 5, t, t, t,) which have a communication with the above-mentioned membranulæ. The use of these vessels is to give nutriment to the coral, by means of a milky juice contained in them. The surface of this bark is slippery and uneven, when the coral has been just fished out of the sea; somewhat raised in some places, in others more depressed and flatted.

There are observed in several parts of the said bark, small tubercles or prominencies, (fig. 7, s) which may be seen even without a microscope. These tubercles are pretty large at their bottom or basis, and round (fig. 1, n, n), grow somewhat narrower towards their upper part (o), and terminate in a lip of some thickness, regularly divided into 8 parts (fig. 1, s, s, fig. 8, s, s) more or less even; which form the mouth (fig. 1, t, fig. 8, t, fig. 9, a) of each tubercle, or, to speak more properly, of each cellule. The bark of the coral ends at the extremity of these parts: and thus it is, that all the inner part of each cellule of the white pellicle is formed. The white pellicle (fig. 2, g, fig. 5, n, n) is doubled in some places, and forms a little bag (fig. 6, s, c) which lines the inside of each cellule (t), that is, to the beginning of the lip, or to about the middle of the cellule.

The substance of the coral (fig. 6, o) gives way to the cellule by small cavities: yet these are not very visible in the old thick branches, but they are pretty easily seen in the young and slender (fig. 4, a, c). Thus the cellule does not end at the coralline substance; since the white pellicle (fig. 6, s) is between it and the said substance. The hollow of the cellule grows narrow into a sort of cone, with an obtuse apex; the belly of which is greater in diameter than the basis. The bottom of such a cellule faces the foot of the coral, and its mouth the branchy or most distant part from the foot. In this cellule is lodged the polypus, which is visible to the naked eye, (fig. 7, s) but its exact shape is only to be seen by the microscope; and it was by this means, that a drawing has been made of it.

Therefore it is from each cellule (fig. 6, t, c) that a white, soft, and somewhat transparent polypus (fig. 10) comes forth, or extends itself; which in shape resembles a star with 8 equal rays, nearly conical, (fig. 11) and furnished with other conical appendices (fig. 11, a, a, fig. 10, a, a,) which issue out of it on both sides. The two rows of these have their direction nearly on the same plane. The rays are somewhat flatted, (fig. 10, a, a,) and a trough (fig. 12, c, fig. 10, n, o,) rises out of their centre, somewhat widened at its beginning, with an opening or great mouth at top (n). In its sides there are 8 upright ridges, broad and elevated, and as many wrinkles, or furrows; and each ray is inserted between every two wrinkles (a, a). This trough is placed on a smooth part, (fig. 12, g) which we may call the belly of the animal; and this part, while the animal lives, and has not been hurt, is always erect in the cellule; though it be entirely disengaged, and separated on all sides from the said cellule; as may be plainly seen in some positions of the polypus.

All these particularities are to be seen only when the coral is just drawn out of the sea, and suffered to stand in some of the sea-water: for, if you take the coral out of this water; or even if you do but touch it in the water, the polypus immediately retires into its cellule. In retiring, it contracts itself, the trough is closed up (fig. 10, n, o) and each ray, (fig. 13, c) as also each appendix (a, a, a, a) shrinks, and enters into itself, just as snails pull in their horns: eash ray pulls in about half its length, and with their ends they adapt themselves to the edges of the trough (fig. 14, 15).

It is in this position that the polypus is seen the moment the coral is drawn out of the sea. The polypus, in this contracted state, seen without a microscope, resembles a drop of milk; and this is what all the good coral-fishers take for the real milk of the coral; the rather because, by pressing the bark of the coral with the fingers, the polypus is forced out, and in coming forth it always retains the appearance of milk. And this makes it probable, that the accurate Andreas Cæsalpinus, who was the first observer of milk in coral, in reality saw nothing but the polypi in the semblance of milk.

Though the polypi have their belly (fig. 12, g) quite disengaged from the cellule, as said above; yet they always keep it therein, shortening and widening it so, as to make it thicker than the mouth or opening of the cellule (fig. 16, g): and this may be seen very distinctly by separating the cellule and its polypus from the substance of the coral, and then observing it on the back part. In this attitude it is that we see, not only the belly very much shortened (fig. 16, g) but also the position in which the polypus keeps itself in its own habitation.

At the bottom of the belly (fig. 12, g) of some polypi, are observed some roundish hydatides, extremely small and soft, transparent, yellowish, or tending to pale. The situation and figure of these hydatids induced Dr. D. to believe, that they are true eggs of the polypus. Although the size of these eggs is not much above the 40th part of a line, yet by the assistance of a good microscope, are discovered some vestiges of little grains, like those which are common on the bark and substance of coral. These eggs are detached from the polypus, and being soft they adapt themselves, and stick to the hard bodies, on which they all. Afterwards they spread at the foot or bottom, and swell up a little (fig. 17); and in this case we very well discern an inward cavity in them, the upper part of which becomes uneven by 8 wrinkles, (fig. 18) but is not open as yet. Shut up within this cavity the fetus of the polypus remains, contracted within itself, and as it were without form. In due time the polypus grows ripe, and as it were adult; and the upper part (fig. 1, s, t, s) opening, it comes forth properly extended (fig. 12, g), and thus furnishes the coral with nutriment.

While the first cellule is shut up, (fig. 17) or the egg of the coral is in its substance, we do not find any one hard part in it like bone or marble; it is all soft: but afterwards, when the cellule opens, we begin to observe some hard lamellæ; and when it is grown larger, and arrived at the height of about a line and half, it widens at bottom, (fig. 9, n) and at the top, (a) and grows narrower in the middle (o), assuming the proper consistence and hardness of coral. And as this grows, the polypi are multiplied, and new branches of coral are formed. Here then we see the vegetation of a plant, and the propagation of an animal. It is submitted to the learned to decide at present, whether the coral belongs to one of these kingdoms rather than to the other; or whether, with greater justice, it deserves an intermediate place.

Description of the Madrepora. See pl. 5, fig. A. This is entirely like the coral, as to its hardness, which is equal to bone or marble. Its colour is white, when polished. Its surface is lightly wrinkled, and the wrinkles run lengthwise of the branches. Its inside is of a particular organization; having in the centre a sort of cylinder, (fig. D, i) which is often pierced through its whole length by 2 or 3 holes. From this cylinder are detached about 17 laminæ, (fig. D, k, k) which run to the circumference in straight lines (fig. D, m, m, m, m).

These laminæ are transversely intersected by other laminæ, (fig. D, q, q) which form many irregular cavities throughout the whole plant. The branches (fig. A, g, g) are conical; and the basis of the cone is formed by the summit of the branch (fig. A, e, e). Every one of these summits has wrinkles on its outside, which run in the longitudinal direction of the branches (fig. B, c, c); and each wrinkle answers to a lamina, (fig. C, e, u, e, u) and each lamina is of the shape of a prism, (fig. E) the basis of which is warty, and faces the outside, (fig. C. e, u) and its point is cut into teeth, (fig. E, n, n, n) and belongs to the inside. The cellule, (fig. B, a, a, a, c, c, fig. C, e, e, u, u) which is of the shape of a chalice, is composed of these laminæ ranged into a circle.

In every one of these cellules is found a little polypus, represented in fig. F, but considerably magnified; the mechanism of which is this: three different parts, unlike each other, compose this animal; viz. the feet, (fig. F, o, i) a trough, (fig. F, g, H, t) and a head (fig. G, n). Each foot begins by 2 conical appendices. By the union of these appendices a rounded part is formed, which in some degree resembles the belly of a muscle, fig. H, i, fig. I, x) by means of which the foot is shortened and lengthened. To this part (fig. I, x) is annexed a little cylinder, (fig. I, n, fig. H, c) the length of which is indeterminate.

These feet are ranged all around in great number, and annexed to the laminæ, (fig. B, a, a, c, c) and are all united to the trough, (fig. H, c) on the outside of which are seen 10 cavities, with an equal number of prominences (fig. H, t, t, t, s, s, c), and in these is lodged the animal's head, (fig. G) which has prickly rays, the precise number of which could not be determined, on account of the extreme velocity of the continual oscillatory motion of the head from right to left, and from left to right, yet he thought he could perceive the number of these rays to be 8: and the use of them may be for the animal to catch and hold its food. This part is not always to be observed, because it sometimes hides itself, by closing up the trough (fig. H, s, s, t, c) about it; and thus it is safe in its habitation.

As the figure of this animal bears no resemblance to the urtica marina, he cannot see how we could class the polypus of the madrepora with the urtica. This animal is extremely tender, and generally transparent, and very beautiful for its variety of colours. He observed it in spring and autumn in the neighbourhood of Rovigno and Orsera, where it is often fished up.

*A Description of the Miriozoon, or Pseudoforalium album fungosum of Aldrovandus.**—As the size and shape of this polypary is sufficiently seen in fig. K, pl. 6, he describes only what the microscope has enabled him to observe in it; and what Count Marsigli, though peculiarly diligent, has either overlooked, or examined with too little attention. And the rather, as the mechanism of this body appears very wonderful.

Its substance is rather like that of bone than of marble, but brittle: and its brittleness proceeds from the great number of cellules with which it is hollowed. These cellules are ranged all around in the branches, (fig. M) and disposed in the manner of a quincunx; (fig. L, n, o) resembling those cinerary urns, frequently found in Italy (fig. o).

In each of these cellules lodges an oblong polypus (fig. Q), slender at the tail, (fig. Q, t) thick at the belly, (fig. Q, e) and again slender at the neck, (fig. Q, s) to which is attached a little cover, (fig. Q, o, and P, o) round, concavo-convex, and of a bony substance. This cover is attached by its lower part (fig. P, n) to the entry of the cellule.

When the polypus chuses to spread itself out, it opens the cover, and out of its neck thrusts an ample proboscis (fig. R, g), in the shape of a cup; and with this it probably takes its food. There are two little muscles (fig. H, a, a) at the lower part of this proboscis, which are attached to the cover. When the animal returns into its nich, the proboscis sinks into itself; and the animal, by contracting itself, draws back the cover; and thus the cellule is perfectly closed, and the creature secure in its retreat.

However, all the polypi of this plant do not enjoy this conveniency and security, but only the adults; that is those which dwell about the branches. As for the others, that are not as yet adult, and live and lodge on the tops of the branches, fig. L, r, n, n, and N, n, x) they have no covers; and a considerable number of them dwell in imperfect cells, or in such as are finished only in part, (fig. D, t, t) and made of a sort of cartilaginous and membranaceous materials. The imperfection of these cellules, and the weak consistence of the paste which

* This coral is the *Millepora truncata*, Linn.

forms them, afforded a plain proof that the cellules are the work of the polypi, as the niches, where they lodge, are made by some shell-fish.

*XV. On the Class of the Phocæ Marinæ.** *By James Parsons, M.D., F.R.S.* p. 109.

In February 1742-3, Dr. P. gave some account of the sea-calf, which was shown at Charing-cross at that time, which he often saw while alive, and afterwards opened it. It is printed in the 496th number of the Trans. There is also now in town a seal (another species of phoca) alive; which gives occasion to taking further notice of this class of animals, that the Society may have a clear idea of their differences, and great variety.

All the species of phocæ, this being the generical name, have among them a very great likeness to each other, in the shape, not only of their heads, but also of their bodies and extremities. They are webbed nearly alike, are alike reptile, viviparous, bringing forth, suckling, and supporting their young alike; and in fine all have the same title to these appellations, phocæ, vitulus marinus, sea-cow, sea-lion, &c. and these names are vulgarly given to them, as their size happens to be greater or smaller; and the first of these names from φώκη, or, according to Dr. Charleton, from βώκη, signifying a noise, or kind of grunting, which they all at some times make.

The different species of this class, or rather genus, of animals, are distinguishable, by their proportion, their size, as to their full growth, their teeth, webbed feet, and whatever other parts in some may not be proper to others.

As to the first, this species before us is shorter and thicker in proportion than that described before. Dr. Grew, in his excellent book of the Rarities, &c. mentions a difference in the proportions of 2 which he describes, in their thickness; that presented to the Museum by Mr. Haughton being thicker than the other. He also gives an account of another species, which he calls the long-necked seal, in these words: "he is much slenderer than either of the former; but that wherein he principally differs, is the length of his neck; for from his nose-end to his fore-feet, and from thence to his tail, are the same measure; as also in that instead of his fore-feet, he hath rather fins: not having any claws thereon, as have the other kinds." The head and neck of this species are exactly like those of an otter. One of those, which is also now in our museum, taken notice of by the same author, has a head shaped like that of a tortoise; less in proportion than that of every other species, with a narrowness or stric-

* In this paper Dr. Parsons includes not only the seals, strictly so called, but likewise the walruses, manatis, &c. and thus forms a kind of large natural genus under the title of *Phoca*.

ture round the neck: the fore-feet of these are five-fingered, with nails, like the common seal.

Their size, as to the utmost growth of an adult, is also very different. That before described, was 7$\frac{1}{2}$ feet in length; and, being very young, had scarcely any teeth at all. This in town is but about 3 feet long, is very thick in proportion, and has a well-grown set of teeth; which, in a great measure, shows this to be about its full growth. The manati is also a phoca, and is one of those species which grows to a prodigious size. The great skin, in the museum, is that of a manati; which seems to agree with the other species of this family, in every essential part, except broad bifid webs, instead of webbed feet: and Peter Martyr gives an account of one of these, which was 35 feet long, and 12 thick.

The docility of this seal in town is, with reason, much admired, as a thing unusual and strange to us; but it appears, from Dr. Charleton, that in his time it was not uncommon for the seamen and fishers to catch some of these creatures sleeping, on the coasts of Cornwall and the Isle of Wight, and bring them to be so tame, as to get money by showing them, and their performances: and he adds, that the people of the former place call the larger kinds about that coast soils, and the smaller seals. But the story told by the above author Martyr, of that great manati, shows how capable these creatures are of being rendered very familiar; and how susceptible of impressions, though they really seem as unfit for any kind of education as any other whatever. This author describes the manati very fully; and then tells this remarkable story:

"A governor, in the province of Nicaragua, had a young manati, which was brought to him, to be put into the lake Guanaibo, which was near his house; where he was kept during 26 years, and was usually fed with bread, and such-like fragments of victuals, as people often feed fish with in a fish-pond. He became so familiar, by being daily visited and fed by the family, that he was said to excel even the dolphins, so much celebrated by the ancients for their docility and tameness. The domestics of this governor named him Matto; and at whatever time of the day they called him by that time, he came out of the lake, took victuals out of their hands, crawled up to the house to feed, and played with the servants and children; and sometimes 10 persons together would mount upon his back, whom he carried with great ease and safety cross the lake."

All that is here mentioned of the docility of this manati, does not much surpass that of this seal in town. He answers to the call of his keeper, and is observant of his commands; takes meat from his hand, crawls out of the water, and stretches at full length, when he is bid; and when ordered returns into the water; and in short stretches out his neck to kiss his keeper, as often and as long as required. These are marks of a tractableness, which one could hardly expect from animals, whose mien and aspect promise little, and indeed whose place of abode, being for the most part inaccessible, prevent their being familiarized to any commerce with men, except by mere chance.

The teeth are very well preserved in the skin of the manati in the museum: they are 16 in the upper, and 14 in the under jaw; and of these, 4 are between the canine teeth of the upper, and 2 between those of the under jaw. They are all conical from the gums; the canine teeth are 2 in each jaw; being an inch and half long each, and of the same form with the rest; and they all bend a little backwards by a small curve in themselves. Nor have the very back teeth of all the least resemblance to the molares of other animals.

The walrus or mors, is another species of phoca, and differs very little in shape and parts from the other species of this genus; except that the 2 canine teeth of the upper jaw are of a prodigious size, like the great teeth of an elephant.

There are some species of this genus of the phoca, which never grow to above a foot long; and there are of all sizes at full growth from these to the manati and walrus. The skins of every species have short hair, and their colours are variegated from the straw-colour and yellow to the deepest brown and black. They are sometimes regularly brindled, sometimes curiously spotted; sometimes in brown clouds on a yellow ground, like that of a pied horse; and sometimes the brown or black occupies the greater part of the skin, having less of the yellow: and in short even those of the same species are as variously spotted or clouded as the hounds in the same pack; and it is probable, that in unfrequented islands and countries, other species of this tribe are yet undiscovered. But it must be observed, that where no other difference, but the variegation of the colour, appears among them, that is, in their size, proportion, teeth, or extremities, they are no more to be accounted different species, than cows having various changes in the distribution of the clouds or spots on their skins.

In the first chapter of the second book of Lord Anson's Voyage, is described an animal under the name of the sea-lion. This history may be applicable to other species of phocæ; and by this description, as well as the figures exhibited in the book, what are counted sea-lions, are manatis.

Linneus ranks this genus of animals with those of his 2d order of quadrupeds; and indeed with great propriety, however injudicious it may lately have been thought: for though none of this tribe can use the posterior extremities to raise themselves up, or stand upon them, as on legs and feet; yet they swim and guide themselves in the water with them; for which they claim the title of palmipedes, or webbed feet; for they have no similarity with fins.

If it be objected, that these animals would come more naturally under his class of amphibia; we may assert, that he had 2 very good motives for ranking them with quadrupeds. First, he had our great Ray for his director, who has himself done the same thing: and, secondly, he found, that though these creatures are really amphibious, yet the commanding characters, by which he has, with great sagacity, distinguished his classes, prevail here to give them a place rather among the quadrupeds than the amphibia.

This great naturalist divides the animal kingdom into 6 classes, and each class into 6 orders. Each order is again divided into different genera, and each genus again has its different species. The phoca then is the 6th genus under the 2d order of the quadrupedia; which order is that he calls feræ.*

M. de la Condamine, in the account of his voyage down the River of the Amazons, describes an animal, which doubtless is a species of the phoca. See figs. 11, 12, 13, pl. 3.

XVI. Of an Iliac Passion, from a Palsy of the large Intestines. Communicated to Dr. de Castro, F.R.S. Translated from the Latin, by Tho. Stack, M.D., F.R.S. p. 123.

A merchant, aged 70, who had been accustomed to hardships from his infancy, was, for the last 6 years, subject to rheumatic pains; but considering his disorder as the effect of old age, he rejected all medical advice. In these circumstances it happened that he was suddenly set upon by a party of soldiers, who with severe threatenings turned him out of his house, and took possession of it: which so terrified him, that he was seized with a violent belly-ach; and his agony so overpowered him, that he fell on the ground half dead, and at the same time voided blood by the anus.

Afterwards he was much subject to the gripes all the ensuing winter, which he took no care of. During this time he suffered much from costiveness, till March 1747, when he was seized with severe pains about the navel; and though he had clysters of several sorts given him, not one of them could be made to pass. He was feverish and thirsty, with a white moist tongue, and could not sleep. He was blooded as much as he could well bear; and the blood did not appear inflammatory. He was treated with laxative medicines, antiphlogistic fomentations, to ease the gripings, and give a free passage; but nothing took effect for 7 days together.

On the 8th he began to break wind, retain the clysters, discharge some little fæces, and to sleep, though not quietly; and on the 9th to make turbid urine. But these promising appearances were but of short duration; for on the 11th his belly was so bloated, that he seemed tympanitic; and an acute pain, which he

* Dr. P. here refers to the tenth edition of the Systema Naturæ of Linneus.

had in the hypogastric region, darted up towards the midriff on the right side: and now the mucus of the intestines came away with the clysters. He had bad sweats, and made foul urine, without sediment.

On the 15th a consultation was held; and as his thirst and fever were abated, and the medicines hitherto prescribed for opening a passage, and taking down the swelling of the belly, which seemed ready to burst, had proved ineffectual, it was agreed to make him swallow 6 oz. of crude quicksilver, with oil of sweet almonds, and syrup of violets; and soon after to throw in several purging clysters.

In 9 hours a passage was opened, and he voided much black liquid excrement, without the least grain of quicksilver. A little after that, he vomited much; and in what he threw up there plainly appeared excrements, and globules of mercury. This was soon followed by thirst, a little slow fever, very troublesome gripings, no sleep, red high-coloured thick urine, in very small quantities, breaking of wind without any ease, vomiting of every thing he took, great weakness, and partial sweats in the forehead and breast. Under these symptoms he languished to the 20th day, and then died.

The appearances, on dissection, were these: the omentum was consumed; and the colon was inflamed in several places, and so distended with wind, that it nearly filled the whole abdominal cavity. Its ligaments or bands were so thoroughly effaced, that there was not the least sign of them remaining. The cæcum was so stretched, as to occupy the whole capacity of the pelvis; and that part of it, which is touched by the thick gut, was gangrened, and perforated with a small opening. Having cleared it of the excrements, there were no internal rugæ at the insertion of the ileum, nor any traces of the valve of the colon, or of its braces, to be observed. For it was quite smooth on the inside, as well as the colon, by the destruction of the cellules, which it has in a natural state. The quicksilver was dispersed all over the cavity of the abdomen, in such quantities, that it was easy to perceive, that none had been discharged by stool. Every thing else contained within both the cavities, was in its natural condition.

XVII. On the Variation of the Magnetic Needle. By Peter Wargentin, Sec. of the Royal Acad. of Sciences in Sweden. From the Latin.* p. 126.

Dr. Halley suspected that there was some correspondence between the aurora borealis and the magnetic needle. And Celsius and Hiorter found by experiments that the needle was greatly disturbed, and unsteady, whenever the north-

* Peter Wargentin was a Swedish mathematician, but chiefly distinguished as an astronomer, and particularly for his tables for computing the eclipses of Jupiter's satellites, which have been much used by astronomers. He was born in 1717, and died at the observatory at Stockholm in 1783, at 66 years of age.

ern lights rose to the zenith or passed southward, so as that the declination seemed to follow the motion of the light, and in a very few minutes of time would sometimes vary 3 or 4 degrees. M. Wargentin has also, by observations in Feb. 1750, like as Graham, Celsius, and others, observed before, found that there is a diurnal variation of the needle backward and forward: so as that from 7 in the morning till 2 afternoon, the needle declined more and more to the west by $\frac{1}{5}$ or $\frac{1}{4}$ part of a degree; after which it gradually returned again, so as by 8 at night to be nearly the same as it was at 8 in the morning. After this it is nearly at rest during the rest of the night, except some small motion to the west about midnight. And this diurnal variation never fails, but is constant and almost regular, unless when it is impeded by the northern lights. This he observed constantly from the 1st of February to the 15th, on which last day an aurora borealis appeared, and deranged the needle so, as in 10 minutes time, about 10 at night, it shifted 20′ to the west, and in another ten minutes returned thirty-seven minutes to the east. But on the lights disappearing, the needle settled at rest. And thus it continued in its regular diurnal vibrations, till Feb. 28, when it was again disturbed by another appearance of the northern lights, so as to cause the needle to vibrate irregularly between 6° 50′ and 9° 1′ of west variation. And on the 2d of April, from a like cause, it differed from itself little less than 5°, shifting irregularly and frequently backward and forward, between 4° 56′ and 9° 55′.

*XVIII. Abstract of a Letter, dated May 2, 1750, from Mr. Freeman at Naples, relating to the Ruins of Herculaneum.** p. 131.

About 7 or 8 years ago, the discovery of Herculaneum was much spoken of, which was reported to have been swallowed up by a violent eruption of Mount Vesuvius, according to the last accounts, in the first year of the reign of Titus, 79 years after Christ. The situation of this city is at the foot of Vesuvius near the sea, and just at one end of the village of Portici, the summer residence of the king of Naples; and probably a great part of the city is under the said village.

You are first conducted down a narrow passage, scarcely wide enough for 2 persons to pass; and in a gradual slope, to the depth of about 65 feet perpendicular. Here is shown a great part of the ancient theatre, a building in the form of a horse-shoe. That part where the spectators sat, is visible, and consists of 18 rows of broad stone seats, one above another, in a semicircular form. At proper distances within the circuit of the seats, through the whole range, from bottom to top, are little narrow flights of steps, by which the spectators might come to, or go from, their seats commodiously, without crouding. These steps

* See some former accounts of these ruins, vol. viii, p. 435—438, vol. ix, p. 362 of these Abridgments.

or stairs also lead up, in a straight line, to a sort of gallery, several feet wide, which ranges all round the outside of the theatre, and is called the precinct; above which there are other stairs, which lead to a second. By this precinct it is judged, that the theatre, with the orchestra, must be about 52 or 53 feet diameter.

Going round the theatre, are seen several large square pilasters, equally distant from each other; and which supported the whole edifice. These pilasters are of a thin compact red brick, adorned with marble cornices. The pavement of this theatre must have been very beautiful, by the different coloured marble, that has been taken out of it, and some that remain. In short, by the broken pieces of cornices, mouldings, and carved work, and the many fragments of pillars, &c. which have been found within and without the theatre, it appears to have been a most magnificent edifice.

There are 2 principal gates to the theatre, with inscriptions on the architraves, which are taken out, and placed in the king's palace, among the other curiosities.

There is another opening, distant from that which leads to the theatre, by which they have made a way into some houses. Here they seem to have dug infinitely more than about the theatre; for one may ramble, as in a labyrinth, for at least half a mile. Among the things that have been dug out of either of the two places, are many parts of broken horses, with part of a triumphal car or chariot, all of gilt bronze; and which, they say, was placed over one of the gates of the theatre. Two equestrian statues, which were found on each side of one of the said gates, and they suppose fronting a street that led to the theatre. Those, they say, were erected in honour of the 2 Balbis, father and son, who were benefactors to the Herculaneans. One of these statues cannot be repaired; the other, which happened to be better preserved, is well repaired, and is set up under the piazza in the gate-way of the king's palace at Portici.

This is a most beautiful statue, and is considered to be one of the best in the world. Not far from it, at the bottom of the palace stair-case, are fixed a beautiful statue of the emperor Vitellius, very perfect and entire; one of Nero, with a thunderbolt in his hand; one of Vespasian; one of Claudius; one of Germanicus; and 2 beautiful statues, sitting. There are many others, of marble, and of bronze, all larger than life; and even some gigantic, or colossal; many without heads, or arms, and others so destroyed as never to be repaired. Of busts, there are some very beautiful, as that of Jupiter Ammon, Neptune, Mercury, Juno, Ceres, Pallas, &c. In the apartments of the palace is a vast number of little statues, many of which are extremely beautiful: also a great number of little idols, tripods, lachrymatories, and many vases curiously wrought. Among these, is a whole loaf of bread burnt to a coal; which they will not suffer any one to touch. It is covered with a glass bell, through which are perceived letters on the loaf, which possibly were the baker's mark.

There are many other valuable curiosities locked up in the king's closet, and private apartments; such as medals, intaglios, and cameos.

Of the pictures, some were taken out of a temple near the theatre, others from the houses. They have all preserved their colours to admiration, which are very lively. They are painted in fresco, and were sawed out of the walls, with much trouble and care; and are now fixed, with binding mortar, or cement, in shallow wooden cases, to prevent their breaking, and varnished over, to preserve their colours. You must think, that these pictures are not alike valuable, otherwise than from their antiquity; some doubtless have been done by good hands, others by bad, as one sees by the works of those now-a-days. There are two as large as life. One of these pictures, they say, represents Theseus. The figure is naked, and holds a small club in his hand: between his legs lies a Minotaur, the posture of which produces a most admirable foreshortening. There stand about him also three little boys, one of which kisses his right hand, another embraces his left arm, and the third his left hand; all extremely well expressed. The other picture is of the same size as the former, and composed of many figures as large as life. A woman sitting with a wand in her hand, and crowned with flowers: on one side of her stands a basket of pomegranates, grapes, and other fruit: near her is a little satyr or fawn, playing on one of the ancient instruments, of 6 or 8 tubes, joined together in a row. There is a lusty naked man standing by her, with his face turned somewhat towards her, with a short black beard. He has a bow, a quiver of arrows, and a club. In the same piece is another woman, who appears talking to the first; she is crowned with ears of corn. There is also a hind giving suck to a boy, which they say represents the story of the discovery of Telephus. Another picture represents a winged Mercury, with a child sitting across his neck, near whom is a woman sitting, and taking Mercury by the hand. This, we were told, was supposed to be Bacchus carried to nurse. Another piece represented Jupiter embracing Ganymede. In another is a hunt of stags and swans. Three others, in each a Medusa's head. Another, representing two heads of imaginary animals. A beautiful one, representing 2 of the muses, one playing on the lyre, the other with a mask on her head. Another, with a lion, a wood, and distant views. In another, various centaurs, buildings, &c. In another, a stag; over which is a bird flying, and seeming to beak at him. Two other small pictures of a dolphin. Another with architecture, and distant views. One with a peacock. Another with a temple, adorned with various pillars.

There have been also found two large cornucopias of bronze gilt, a large round shield of metal, 2 metal dishes, several lachrymatories of glass, others of earth; 4 large candlesticks of bronze, a large metal vase with a handle; many others of earth, curiously wrought; the foot of a lion most curious, but in marble, and which supported a marble table; a beautiful mascharron of metal, having the face of a cat, with a mouse in her mouth. There is also a very fine medallion, extremely well preserved, with a bas relief on both sides; on one is a woman, near whom is a man naked killing a hog; on the reverse is an old man, naked to his waist, sitting and playing on 2 pipes, which he holds in his hands. There is also another odd piece in bas relief, which represents a green parrot, drawn in a chariot, and driven by a green grasshopper, which sits on the box, as coachman. There are many baskets and cases full of different things, all jumbled together; such as kitchen utensils, locks, bolts, rings, hinges, and all of brass. Things, that were of iron, were totally eaten up with rust. When the workmen came to any thing of that sort, it mouldered to dust as soon as they touched it; occasioned doubtless by the dampness of the earth, and the many ages during which it lay buried. There were found many vases, and crystal bottles full of water; but that might penetrate through the earth, and fall into them, if not close stopped: also a sort of standish, or inkhorn, in which were found many stylets or pens, with which they wrote in those days. When it was first taken out, they say the ink had not only its natural colour, but that it was yet capable of tinging: it is very dry now. There were eggs found quite whole, but empty; also nuts and almonds; grain of several sorts, beans and pease, burnt quite black. Many other sorts of fruit were found burnt quite to a coal, but whole and entire.

Mr. F. declares that he cannot be of the opinion of some, who assert that this city was suddenly swallowed up, which implies that the earth must have opened, and formed a pit to receive it. His opinion is, that it was overwhelmed with the boiling matter issuing from the mountain, at the time of the eruption; because most things were found upright, chiefly the buildings. That it was not a sudden overwhelming, and that the inhabitants had time to escape with their lives, though not with their goods, is proved, by their not finding dead bodies, where they have hitherto dug. It is said that some human bones were found, though few. Very little money or plate has been found, or any other portable thing of great value; which is another proof that the inhabitants were not destroyed. Doubtless before the violent eruption came on, the people for some days might perceive such tokens and signs, as could not but alarm them, and put them on their guard; as at the eruption which happened in 1737, before it burst forth for some days, the inhabitants of Portici, and the adjacent villages, all retired; being by some signs apprised of the event.

The matter (called the lava) it seems is not of the same quality nor substance all the way through the body of it; for in descending to the theatre, the sides of the passage at the entrance were a sort of mold, 8 or 10 feet thick; after which appeared stone of a blackish or dark grey colour, to the thickness of about 3 or 4 feet; then another layer of sandy earth, under which was a layer of the same sort of stone; and thus it continues stratum super stratum, to the bottom. The theatre and the houses seem all to have been filled with earth. In general, this stone is very hard and heavy, and the whole city of Naples is paved with it. Some of it will bear a fine polish, and of which they make snuff-boxes.

XIX. Of the Hermaphrodite shown in London. By J. Parsons, M. D., F. R. S. p. 142.

She was a French girl about 18 years old, and the true description of her pudenda was as follows: What was mistaken for a penis, and had at first sight caused the deception, was the clitoris, grown to an inordinate size. The prepuce of this was continued down on each side, to form the nymphæ; under these the natural urethra was in its proper place, as in all females, and just under this was a natural vagina. This vagina was concealed by a skin growing up from the perinæum, and continued to the labium of each side quite over it; which, if snipped with scissars, would lay the orifice of the vagina bare, and show the person a perfect female, having only this morbid size of the clitoris. This was really the fact, which any one might have been satisfied of, by passing a finger down under this skin to the perinæum, when he would meet the orifice of the vagina, and find it as perfect as that of any other woman of the same age.

The vagina being thus covered, and the clitoris thus large, it was no wonder, that she should at first sight be taken for a male by the vulgar; but it would seem a little too careless in any of the faculty to be so deceived. However if we consider the following observations, we shall find it no such strange affair, as it now seems to the world: nor is it new to find people imagine that, since this mistaken penis is imperforate, the urethra is preternaturally directed to appear under it, without considering it to be a true female urethra, in its natural place.

Dr. P. on the 30th of April, 1741, laid before the Society 7 or 8 female fœtuses, from about 6 to somewhat more than 7 months growth. Each of these had its clitoris larger in proportion than the present girl, or any other he had ever seen; which is the case with all female fœtuses, during the greatest part of the time of gestation. And this is nature's common rule all over the world.

Now it is impossible that so many hermaphrodites should be formed at once, since we have so few instances among the European nations of those so reputed; though they are common enough in Asia and Africa, in all those places especially that are nearest the equinoctial line, where the non-naturals themselves conduce much to the general relaxation of the solids in human bodies, and consequently to this unseemly accretion of that part.

Now as the female fœtus increases in the uterus in a natural way, the neighbouring parts of the pudenda grow more in proportion than the clitoris, drawing away the integuments from it, whereby it becomes by degrees less conspicuous; and at length, as the child grows up, it is shrunk between the labia, and remains always covered, as it is now the common appearance in our women. But when it continues its growth, together with the neighbouring parts in the same proportion, which all female fœtuses have it in, maintaining its first proportional size, the person, when grown up, is called by the vulgar a hermaphrodite; since the natural structure of this part is in a great measure like that of a penis virilis. Nor is its largeness in a fœtus much to be wondered at, since there are other very similar cases in the same body, as the gland thymus and glandulæ renales; both which, as the child grows larger, diminish in their proportion.

These macroclitorideæ are so numerous among many nations of Asia and Africa, that the ancients, Albucasis especially in his 71st chap. informs us of the necessary operation and method of cure, which he terms cura tentiginis, finding the part so called inconvenient from its largeness. Nor was this knowledge confined to men of science alone among the Egyptians and Ethiopians, and Angolans; for all parents know, when the child has these parts longer than ordinary, and they cut or burn them off, while girls are very young, and at the same time never entertain the least notion of the existence of any other nature besides the true female, in those children who are thus deprived of that part. The learned De Graaff was well acquainted with this, and gives his approbation of the operation, as highly necessary, as well as decent: "estque hujus partis chirurgia orientalibus tam necessaria quam decora."

It has been said too, that this girl in town had not the least appearance of breasts; but those who reported this, must surely have never seen the breasts of the women of any other nation but our own. On the contrary, she had as large breasts as any French girl of her age, and as good a nipple. Besides she was a thin girl, and small of her age; and really among our own young women, when they are spare and small in stature, it will be hard to find any with breasts more conspicuous than hers, if so much.

Dr. P. had considered this subject more at large in his Critical Inquiry into the Nature of Hermaphrodites, to which the reader is referred.

XX. Of a very small Monkey. Communicated by James Parsons, M. D. F.R.S.* p. 146.

It is, from the tip of the nose to the root of the tail on the edge of the spine,

* This animal is the *simia iacchus* of Linnæus. *Striated monkey*, Pennant. It is a native of South America.

only $7\frac{1}{2}$ inches; and the tail, from its root to the extremity, is 9 inches; its face about an inch long; and hardly $\frac{3}{4}$ of an inch broad at the eyes, where it is broadest. Its weight is about $4\frac{1}{2}$ oz. The face is naked, and of a flesh-colour; the eyes black, having no white part visible; the ears are thin, large in proportion, and of a dark colour; and are surrounded each with a grove of very white hairs; between which the hairs of the neck are blackish, and so are the 4 extremities; the rest of the body and tail is a mixture of dusky and yellow, so as to compose a dark olive; the hairs of the body are exceedingly soft, and each hair is parti-coloured, dusky at the root, then a little yellowish, then dark, and then yellowish again, somewhat like the soft feathers of partridges. The fingers are slender, each having 3 joints; they are 5 on each extremity, and are pointed by nails rather resembling the claws of birds, than those of human bodies; which is common to most other species of the cercopitheci.

XXI. Abstract of a Letter from Naples, concerning Herculaneum, containing an Account and Description of the Place, and what has been found in it. p. 150.

The entrance into Herculaneum is described to be down a narrow passage, cut with a gradual descent; and towards the bottom into steps, and the city is supposed to lie about 60 feet under the surface of the ground. Those who go down into it, carry each of them a wax taper, and are preceded by a guide. It is supposed that besides the earthquake, which swallowed up this town, it was also at the same time overwhelmed with the burning lava, which ran down from mount Vesuvius, during the eruption. And accordingly all the passages into it are cut through this lava; which is a very hard substance, like stone, of a slate colour, and said to be composed of various kinds of metals and glass; which indeed is manifest in the appearance of it. The streets of Naples are paved with the same lava; but it seems to be of a much more soft and sandy substance in Herculaneum, than in the places where they dig it for use.

The appearance of this city would greatly disappoint such, as should have raised their expectation to see in it spacious streets and fronts of houses; for they would find nothing but long narrow passages, just high enough to walk upright in, with a basket on the head; and wide enough for the workmen, who carry them, to pass each other, with the dirt they dig out. There is a vast number of these passages, cut one out of another; so that one might perhaps walk the space of 2 miles, by going up every turning.

Their method of digging is this: whenever they find a wall, they clear a passage along the side of it. When they come to an angle they turn with it; and when they come to a door or a window, they make their way into it. But when they have so done, they are far from finding themselves in a spacious room, or open area; for all the rooms and places they have yet found, are so filled with lava, that it sticks on to the sides of the walls; and they can advance no farther than as they can make their way by digging; which is such labour, that when they cease to find any thing worth their search, they fill up the place again, and begin to dig elsewhere. By which means no place is quite cleared. Consequently it does not appear how many stories high the houses may be; nor is any thing to be seen over head but lava. In this are vast numbers of burnt beams, that seem to have been joists of floors; though they are now little more than black dust; and where they are quite mouldered away, one may plainly see the grain of the wood imprinted on the lava, so close did it stick.

A skeleton was found in a door-way, in a running attitude; with one arm extended, which appeared to have had a bag of money in the hand of it, for the lava had taken so exact an impression of the man, that there was a hole under the hand of the extended arm; which hole was apparently the impression of the bag, and several pieces of silver coin were found in it. This man therefore must have had notice enough of the danger, to endeavour to secure his treasure; though he must have been instantaneously encompassed with liquid fire, in attempting it. No manuscripts have yet been found; but they have met with some few inscriptions on marble, but none of any consequence, or which serve to give new light on any point of antiquity.

The writer proceeds next to give some account of the paintings, and observes that, much the greatest part of them are little better than what you will see on an alehouse wall. They are all painted on plaster, which has been very carefully separated from the wall, in as large pieces as possible. These pieces are now framed, and there are above 1500 of them, but not above 20 that are tolerable. The best of them are 3 large pieces; one of which is a sort of history piece, containing 4 figures, with some expression in their faces; but even these best, if they were modern performances, would hardly be thought worthy of a place in a garret. There are about a dozen little pieces, of women dancing, centaurs, &c. the attitudes of which are very genteel, and the drawing pretty, but the shading is mere daubing.

The colouring is allowed to be surprisingly fresh and well preserved, considering how long it has been done, but the painters seem to have been masters of only a few simple colours, and those not very good. The red is the brightest and best. The lava was found sticking to all the painting; which, some think, has helped to preserve it. The paint is liable to be rubbed off; to prevent which inconvenience, they have slightly varnished it.

The designs of the greatest part of these paintings are so strange and uncouth, that it is almost impossible to guess what was aimed at. Much of it looks like such Chinese borders and ornaments as we see painted on skreens. There are numbers of little figures dancing on ropes; a few small bad landscapes; and some very odd pieces, either emblematical, or perhaps only the painter's whim. Of which last the writer gives two specimens; one, of a grashopper driving a parrot, the other, of a vast great head, in the midst of what seems to have been intended for a green field encompassed with a hedge.

The rest of this paper is only a repetition of that in art. 18 preceding, on the same subject.

XXII. An Occultation of the Planet Venus by the Moon in the Day time, observed at Mr. Short's, in Surrey-street, London, April 16, 1751, *O. S. By Dr. John Bevis.* p. 159.

The whole matter in this business was to direct a tube so, as to find out Venus a little before her ingress, and to manage the instrument so, as also to have sight of her at the instant of her egress. And knowing that Mr. Short is never unprovided with one or more instruments exceedingly well adapted to this and other purposes, the same that he has described in Phil. Trans. N° 493; which, for its easy removal from place to place, may be considered as a sort of portable observatory, Dr. B. intimated his intention to him the evening before; who was so kind as to set up two of the said instruments, which he found rectified, and ready for observation, when he visited him the next morning. One of these, placed near his clock, he intended for his own use, and the other was for the Doctor.

The air was of itself clear; but the wind, being in the north-east quarter, brought such drifts of smoke, as much impaired the distinctness of Venus, which however looked round. Several minutes before Dr. B. expected it, the figure of the planet was manifestly altered; on which he called out to Mr. Short to hasten to his instrument, which he did, though too late. The total ingress was at $10^h\ 39^m\ 30^s$ by the watch. From his first perceiving the change of the figure, to the entire ingress, could not be a full minute.

He observes, that not a glimpse of the moon, then not 2 days old, could be discerned; so that the business of securing Venus, at the instant of her emersion within the field of the telescope, over which she passed in about $2^m\ 10^s$, depended entirely on a due management of the screw, which gave motion both to the equatorial or horary plate, and to the telescope. A little after 11 he brought the point of the hour circle, answering to Venus, to the index, and might then have seen her near the middle of the field, had she already emerged. Every 2 minutes after he was careful to turn the screw so much, as to be sure of keeping her within the field. At length setting his eye to the instrument immediately after one of these operations, he perceived her quite emerged and round:

this was at $11^h\ 13^m\ 15^s$ by the watch, which still kept exact pace with the clock; and his eye had not been removed more than a minute.

Venus passed the meridian in the transitory at $1^h\ 37^m\ 55^s$ afternoon by the clock: the sun passed this day at $11^h\ 57^m\ 27^s$, and yesterday, the 15th, at $11^h\ 57^m\ 28\frac{1}{2}^s$; whence it is easy to reduce all to apparent time, as follows:

Total ingress of Venus 1751, April	15^d	22^h	42^m	2^s
Her total emersion	15	23	15	47
Her meridian transit	16	1	40	29
Now, supposing the whole disk to have taken up one minute, as it seemed thereabout, both in the ingress and egress, the middle of the occultation must have been	15	22	58	$24\frac{1}{2}$
And the duration, with respect to the centre of Venus			33	45

P. S. Mr. John Canton sent notice that he observed the occultation of Venus by the moon last Tuesday, at his house in Spital-square, and found the immersion at $10^h\ 42^m\ 20^s$ a. m. emersion at $11^h\ 15^m\ 40^s$.

XXIII. Of a remarkable Appearance in the Moon, April 22, 1751. By James Short, F.R.S. p. 164.

In N° 396 of the Phil. Trans. there is an account of an observation made on an uncommon appearance of the lunar spot called Plato in the nomenclature of Riccioli's and Grimaldi's Selenography, and Lacus niger major in that of Hevelius. Signor Bianchini, to whom we owe this communication, says, that it was the 16th of August, 1725, N. S. about an hour after sun-set, when he took his observation with a dioptric telescope, of 110 feet, made by the famous Campani, the air being very serene, and the moon, as he says, speaking of the same phenomenon in his book of Venus, a day past the first quarter: so that the said spot then lay in the common section of light and darkness. The mountainous oval margin, with which it is surrounded, was brightly illumined with the sun's rays; but the plain bottom looked darkish, as having not yet received his light. There was however extended along its area, from end to end, a track of reddish light, as if a beam had been admitted through some perforation in that side of the margin, which was then exposed to the sun. M. Bianchini proposes the solution of this matter in two different ways: first, by supposing an aperture in the margin, as just now mentioned: or secondly, by conceiving the moon to have an atmosphere, and that thereby the rays passing near the summit of the margin might be so refracted as to be thrown on the plain area or bottom.

Mr. S. having lately had an opportunity of observing something of the same nature himself, he here lays it before the Society, with a conjecture concerning its cause. Monday, April 22, 1751, O. S. being at Marlborough house, and having directed the great reflector to the moon, he perceived a single streak of light projected along the flat bottom of the spot Plato; and from what he was then able to recollect of Bianchini's narrative, he doubted not but that it was of the same kind with that he saw, and which he had so often looked for in vain. By the position of the spot on the disk, and the shadow of the mountains on the west side of it, we should not have expected to have seen any light on the bottom. Soon after he discerned another streak of light extended along the bottom, parallel to the first, but somewhat lower, which in a very short time was evidently divided into two. He sought in vain for such a perforation, as that hinted at in the other account; but through the great magnifying power of this instrument, he was able to discover a gap or notch in the mountains to the westward, which abutted against the first streak or stream, and pursuing the object with great attention, he also perceived a similar gap in the direction of the lower streak; but though this streak was divided into two, he was not able at any rate to find out another notch, by which to account satisfactorily for the whole appearance; which he would have considered as solved, could such a one have been discerned in a right situation.

XXIV. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries for the Year 1750, *pursuant to the Direction of Sir Hans Sloane, Bart.* p. 166.

[This is the 29th presentation of this kind, completing the number of 1450 different plants.]

XXV. Observations on the Sex of Flowers. By W. Watson, F. R. S. occasioned by a Letter on the same Subject, by Mr. Mylius of Berlin. p. 169.

Extract of Mr. Mylius's Letter to Mr. Watson, dated at Berlin, Feb. 20, 1750-51.—" The sex of plants is very well confirmed by an experiment which has been made on the palma major foliis flabelliformibus. There is a great tree of this kind in the garden of the royal academy. It has flowered and born fruit these 30 years; but the fruit never ripened, and when planted, it did not vegetate. The palm-tree, as you know, is a planta dioecia; that is, one of those in which the male and female parts of generation are on different plants. We having therefore no male plant, the flowers of our female were never impregnated by the farina of the male. There is a male plant of this kind in a garden at Leipsic, 20 German miles from Berlin. We procured from thence in April 1749 a branch of male flowers, and suspended it over our female ones, and our experiment succeeded so well, that our palm tree produced more than 100 perfectly ripe fruit; from which we have already 11 young palm trees. This experiment was repeated last year, and our palm tree bore above 2000 ripe fruit. As I do not remember a like experiment, I thought convenient to mention it to you; and, if you think proper, be pleased to communicate it to the Royal Society."

In pursuance of his correspondent's desire, Mr. Watson lays this account before the Royal Society, which he thinks very curious; not on account of its novelty, or of its confirming the sex of plants, which is now sufficiently established; but on account of the male and female palm-tree's flourishing so completely in such high latitudes as those of Leipsic and Berlin.

The impregnation of the female palm tree by the male has been known in the most ancient times. Herodotus, when speaking of the palm tree, says, "that the Greeks call some of these trees male, the fruit of which they bind to the other kind, which bears dates: that the small flies, with which the male abounds, may assist in ripening the fruit; for, says this author, the male palm tree produces in its fruit small flies, just as the fig tree does." The very remote age, in which Herodotus wrote, sufficiently apologizes for his believing, that what was really brought about by the farina fœcundans of the male flower, was to be attributed to the insects frequently found in it, and which perhaps very often do carry this farina from the male to the female. They had seen the effects of caprification in fig trees by these insects, and were misled by the analogy. They are here translated small flies; but they had a particular appellation given them by Herodotus, Aristotle, and Theophrastus, who call them ψῆν. Pliny, in his history, when treating of caprification, which is almost a translation from Theophrastus, calls them culices, Linneus ichneumones, and Tournefort moucherons. Theophrastus, in his account of the palm tree, gives the very process mentioned by our correspondent. "They bring together, says he, the males and the females, which causes the fruit to continue and ripen on the trees. Some, from the similitude of this to what happens in fig trees, call it caprification; and it is performed in the following manner: while the male plant is in flower, they cut off a branch of these flowers, and scatter the dust and down in it on the flowers of the female plant. By these means the female does not cast her fruit, but preserves them to maturity." Pliny also mentions the like process. Among more modern authors, Prosper Alpinus, gives at large the manner of the impregnation of the female palm tree by the male, for the purposes before-mentioned. We have also copious accounts of the same process by Tournefort, Kæmpfer, and Ludwig. As Kæmpfer was an eye-witness, his account of this matter is most to be depended on.

Mr. W. observes, that though the ancients distinguished rightly, in determining the true sexes of the palm tree, it is the only plant in which they have not erred. Though they called plants of the same genus, or of others very nearly related to it, male and female, it was on an imaginary, a false principle: and that usually taken from their size, the difference of their leaves, or the figure of their fruit; and what therefore they have denominated male and female, must not with the modern exactness be rigorously considered as such. Thus Aristotle, after having taken notice that there was the distinction of male and female observable in plants, says, "that the male plant is more rough and strong, the female more weak and fruitful." And Theophrastus, when speaking of the male and female pine tree, says, "that the Macedonians have trees nearly related to pines, of which the male is of shorter growth, and has harder leaves; that the female is taller, and has its leaves softer and more fleshy." He says, on his own authority, "that the wood of the male pine is hard, that of the female more soft." Pliny also in his history gives a like reason for his distinguishing the sex of the pine: he says further, in another part of the valuable monument he has left us, "that the most expert naturalists assert, that every tree, and every herb, which the earth produces, has both sexes;" but this is to be understood in the manner just mentioned; and so likewise is the distinction among the more modern botanists in their denominations of several plants, such as veronica, eupatorium, anagallis, tilia, pæonia, balsamita, filix, quercus, orchis, laureola, abrotanum, cornus, polygonum, equisetum, mandragora, and others, which are termed imaginarily male and female; as the discovery of the real sex of plants was reserved for the accuracy of the present age. Besides the before-mentioned erroneous principle, from which the ancients, as well as some more modern authors, determined the sex of plants, there is yet another, and that is, a denomination of plants from their sex, which is absolutely false; and in order to elucidate this position, and to show at the same time in what the sex of plants really consists. Mr. W. premises, that it is in the flowers of vegetables only that the parts subservient to generation are produced. Simple flowers, to use this term in opposition to the compound flowers of the botanists, are either male, female, or hermaphrodite. By male flowers, he means those which are possessed only of those organs of generation analogous to the male parts of animals; and these are what former botanists have denominated stamina and apices, but are since named more properly by Linneus, filamentum and anthera. The female flower is only endowed with parts like those which perform the office of generation in females; and these are the pistillum and its appurtenances, which, by Linneus, with his accustomed accuracy, are divided into three parts, viz. the germen, stylus, and stigma. The hermaphrodite flower, which constitutes the great bulk of the vegetable creation, is possessed of all these parts in itself, and is itself thus capable of propagating its species without any foreign assistance; which, by many incontestible experiments, it has been found neither the male nor female flower simply is able to do. Much the greater number of plants, as just hinted, have hermaphrodite flowers; but there are some which have both the male and female flowers growing from the same root. Such are mayz, or Indian corn, nettles, box, elm, birch, oak, walnut, beech, hazel, hornbeam, the plane tree, pine, fir, cypress, cedar, the larch tree, melons, cucumbers, gourds, and several others. In many of these, though the male and female flowers are at considerable distances, the farina fœcundans, which Providence, on account of its being liable to be spoiled by rain, or dissipated by winds, has provided in great abundance, is conveyed to the female by means of the atmosphere. It is this class of vegetables, and the following, the quantity of the produce of which is much more precarious than those plants which have hermaphrodite flowers; as the impregnation of these last may be performed within their own calyx; whereas the former must necessarily commit their farina to the circumambient air. It is for this reason that if, during the time of the flowering of these plants, the weather is either very wet or stormy, their produce of fruit is very inconsiderable, from the spoiling or hasty dissipation of the male farina. Thus, independent of frosts, the fruit of the nut and filberd tree is most numerous in those years, in which the months of January and February are the least stormy and wet, as at that time their flowers are produced. For the same reasons, a stormy or wet May destroys the chesnuts; and the same weather in July prodigiously lessens the crop of mayz or Indian corn, as its spikes of male flowers stand lofty, and at a considerable distance from the female. In like manner a judgment may be formed of the rest of these. Some of the more skilful modern gardeners put in practice, with regard to melons and cucumbers, the very method mentioned by Theophrastus 2000 years ago, in regard to the palm tree. As these plants, early in the season, are in this climate confined to frames and glasses, the air, in which they grow, is more stagnant than the open air, by which the distribution of the farina fœcundans, so necessary towards the production of the fruit for the propagation of the species, is much hindered; to obviate which, they collect the male flowers when fully blown, and presenting them to the female ones, by a stroke of the finger they scatter the farina fœcundans in them, which prevents the falling of the fruit immaturely.

Besides the vegetables before-mentioned, which bear male and female flowers on the same root, there are others, which produce these organs on different roots: in the number of these are the palm-tree, (the more particular subject of this paper,) hops, the willow-tree, misletoe, spinach, hemp, poplar, French and dog's mercury, the yew-tree, juniper, and several others. Among these, the valisneria of Linneus, as to the manner in which its male flower impregnates the female, is one of the most singular prodigies in nature. The manner of this operation is figured by Micheli, in his Nova Plantarum Genera, and described by Linneus, in the Hortus Cliffortianus. As that elaborate and expensive work is in very few hands, Mr. W. here gives a short account of it,

The valisneria grows in rivulets, ditches, and ponds, in many parts of Europe.

The male plant, which is continually covered with water, has a short stalk, on the top of which its flowers are produced. As this top never reaches the surface of the water, the flowers are thrown off from it, and come unopened to the surface of the water; where, as soon as they arrive, by the action of the air, they expand themselves, and swim round the female flowers, which are blown at the same time. These last have a long spiral foot-stalk, by which they attain the surface of the water, and remaining there in flower a few days, are impregnated by the male flowers detached from the stalk at the bottom. This operation seems to be thus directed, as the farina fœcundans could not exert its effects in so dense a medium as water; and we find that even the hermaphrodite flowers of water-plants, such as those of potamogiton, renunculus aquaticus, hottonia, and nymphæa, never expand themselves till they reach the surface of the water.

But to return: it was not possible for Mr. W. without premising these things, to make evident what he just now mentioned, in regard to the falsely denominating the sexes of plants; as it is to this last class that the wrong application has been made by botanical writers. This error seems to have been first introduced as early as Dioscorides, and has been continued through a great variety of writers, even to our own time. It is most certain, that those plants, which produce the seed, ought to be considered as females; but it happens that in the French and dog's mercury, the seeds are produced in the female plants by pairs; and these are contained in a capsule, which was thought to resemble the scrotum of animals; and from this testiculated appearance they called these plants males, and the others females. Thus, for example, Dioscorides, when treating of mercurialis, or what we here call French mercury, says, "the seed of the female is produced in bunches, and is copious; that of the male grows near the leaves; it is small and round, and disposed in pairs like testicles." Dodonæus, Lobel, Delechamp, John and Caspar Bauhin, Morrison, Tournefort, and Boerhaave, in their several works, have followed Dioscorides, and have denominated the seed-bearing plant of this kind, the male; and the other, the female. Fuchsius and John Bauhin likewise call the cynocrambe or dog's mercury, which bears fruit, the male; and the spiked one with male flowers only, the female. This mistake is observable in hemp, hops, and spinach.

We observe that the operations of nature are carried on most usually by certain general laws, from which however she sometimes deviates. Thus almost all plants have either hermaphrodite flowers, or male and female flowers, growing from the same root, or male and female flowers from different roots: but there are a few of another class, which from the same root furnish either male and hermaphrodite flowers, or female and hermaphrodite flowers. Of this kind are the mulberry-tree, the musa or plantain-tree, white hellebore, pellitory, arrach, the ash-tree, and a few others. But of this class the empetrum or berry-bearing heath is the most extraordinary; as of this are found some plants with male flowers only, others with both male and female flowers separately, and still others with hermaphrodite flowers. What Pere Labat mentions in his Voyage à l' Afrique Occidentale should likewise be taken notice of here. This author, after having laid down the different methods of impregnating the female palm-tree by the male, says, that this process is not absolutely necessary for the production of dates; for being at Martinico, he there saw growing by an old convent near the place, where they anchored, a palm-tree bearing dates, though the only one of its kind which was thereabouts. Whether it was male or female, he did not pretend to determine, but was certain, that there then was none, nor had been any, within 2 leagues of the place where it grew. He doubts indeed whether this tree bearing fruit did not proceed from the farina fœcundans of the male cocoa tree, which is a species of palm, and which grew in abundance near the tree that bore dates: but he observes, that the stones of these dates did not vegetate, and that those who were desirous of propagating date-trees, were obliged to plant the Barbary dates; as he believed the others had not the germ proper to produce the tree. From this account it is very obvious, that the palm-tree here mentioned, was a female, in which though the fruit ripened, it was in such a state of imperfection, as not to be able to propagate its species. In this manner we have eggs furnished by hens without a cock; but these eggs produce no chickens. What this father says of the female palm-tree's bearing fruit without the assistance of the male, Mr. Miller says, has been fully confirmed to him by several persons: and John Bauhin, an author of great credit, describes and figures the whole fructification of a palm-tree, which he saw growing at Montpelier, and which not only produced branches of male flowers, but also female ones bearing dates. Mr. Ray many years after tells us in his history of plants, that at Montpelier he saw this very remarkable tree mentioned by John Bauhin. This variety in the fructification of the palm-tree, singular as it may seem, has been likewise observed in some few others. The learned Jungius, in his Doxoscopia, mentioning that class of trees which are male and female in different parts of the same tree, says, " that trees of this kind, when they have for many years produced flowers without fruit, afterwards produce fruit without flowers. This, he thinks, should be further inquired into." This, since Jungius's time, has been done, and it has been found, that sometimes some of the trees of this class are wholly male, while young; but as they advance in age, they have flowers of both sexes, and afterwards become entirely female. This fact Mr. Miller has frequently himself observed in the mulberry-tree; and the Chevalier Rathgeb, a gentleman excellently well versed in whatever relates to vegetation, has observed, that a large lentiscus, or mastich-tree, near his garden, had for 30 years produced only male flowers, but that for 3 years past it had produced plenty of fruit.

The foundation of the discovery of the real sex of plants, which is of no less importance in natural history, than that of the circulation of the blood in the animal economy, was laid by the members of this learned Society; though much of the honour due to them is attributed by foreigners to the late ingenious Mons. Vaillant of Paris: and this may have arisen from our language not being generally understood on the continent. Sir Thomas Millington, sometime Sedleian lecturer of natural philosophy at Oxford, as we see by our worthy member Dr. Grew's anatomy of plants, seems first to have assigned a more noble purpose to the stamina and apices of flowers, than that which had been attributed by preceding writers, and by Mons. Tournefort afterwards; viz. that of secreting some excrementitious juices, which were supposed hurtful to the embryos of the fruit. Sir Thomas conjectured, and rightly, " that the stamina and apices served as the male for the generation of seed." This hint, which was afterwards adopted by Mr. Ray, in the preface to his Sylloge Stirpium Exterarum, Dr. Grew carried farther, as we find by his works; and it was followed by Camerarius, professor at Tubingen: but our member Mr. Morland, afterwards pursued this inquiry much higher, as we see by his memoir published in the Phil. Trans. N° 287. After these, Messrs. Vaillant and Geoffroy illustrated and strengthened these discoveries by very curious experiments; so that now nothing seems wanting for the confirmation of the truth of this doctrine.

So much for the discovery of the sex of plants in géneral, on which Linneus has founded his system of botany, at present so much and so well received. Whoever therefore would consider minutely the structure of flowers, and the almost infinite variety of the number and disposition of their parts, may consult Linneus's Philosophia Botanica lately published, where this subject is treated in a very copious and instructive manner.

XXVI. On a small Species of Wasps. By Mr. John Harrison of Cambridge, in New England,* p. 184.

About the 28th of May, Mr. H. discovered hanging to the roof on the inside of a green-house (which was of wood) something about the size of a child's farthing ball, in shape like a Provence rose full grown, before it opens, that is, a round bottom, ending in a blunt point, at which point was a round hole, large enough for insects (something less than a wasp) to go in and out at. He soon perceived that it was the work of insects, a small species of wasps. They have 6 legs,

* See a pretty good representation of nests of this kind in the 6th vol. of Reaumur's Hist. of Insects, pl. 19.

black near the body, then yellow, ending in cinnamon colour. Some have 6 or 7 rings, of a bright yellow colour round the tail part, with small hollows or indents on the upper parts. The divisions between the rings are of a bright jet colour; the face is yellow; on the head are 2 horns. These insects are very industrious in making their nest. The top is fastened to the ceiling, and formed of many round coverings, one within another, yet not touching each other, by the 8th of an inch. Probably this space is left to make the cells, in which they lay their eggs.

Their manner of working is curious, and as it is principally performed externally there is an opportunity of seeing every circumstance of the operation, which is carried on with as much application, and perhaps more skill and contrivance than the honey-bees, who are beholden to a hive or hollow tree, &c. to fabricate their combs in; whereas these little animals are the sole builders of the outer walls, as well as the interior parts of their dwellings. They range about for the materials, but with all his endeavours Mr. H. could never observe from whence they were collected; only that they bring a little lump of dark-coloured paste between their fore legs, about the size of a radish seed. This they carry first to the inside of the covering, which they are about to finish, and stay near half a minute, probably to work some of it on that side; then they return with the greatest part, to enlarge it on the outside, which they execute in a most dextrous manner, by taking the paste from between their legs with their mouths, (which open cross ways to their body) and fixing it on the edge of the covering, working backwards, for about an inch at a time in length, and then spread and smooth it with their horns. This is all performed in about 2 minutes, and they are seldom more than 5 days in finishing a whole cover. Their number is only between 20 and 30. They seem not at all hurtful; and are so intent on their business, that if 3 or 4 people at a time are looking within so many inches of their nest, they neither attack them, nor forbear to carry on the public work, which comes to be about 5 inches diameter, and about 4 deep.

They continued their work till they had finished 15 coverings one over another, and began 3 more, which they never completed. About the 16th of August there was a cessation of their usual industry. There was only one or two in a day at work, which continued to the 26th, when they quite gave over adding any more to their nest. Since that, he could only see one or two going in and out once or twice a day, for about a fortnight after. In that time he observed 2 of these insects come out of their nest, of an extraordinary size, at least one-third larger than those that built the nest. These seem, and doubtless are, the parents or queens appointed by the all-wise Creator for continuing their species, as their sluggishness has a near analogy to the queen-bees, that are sometimes seen to come to the mouth of the hive, without any other seeming business than to take the air, and show themselves, and then return into the hive again. About the 6th or 7th of September, he saw the last; none were afterwards seen (Dec. 22). If these insects may be compared to hornets, which they most resemble, in their making and hanging up of their nest, the queens will only survive, and each in the next spring be the founder of a new colony. The common wasps are under the same regulation. The males die at the approach of winter, and leave but few females to survive them. This is wonderfully contrived to prevent the increase of such noxious animals; whereas the bees, so beneficial to mankind, survive the winter, unless robbed of their honey, which is their support during that season.

In the spring, finding none of the insects appeared, Mr. H. took down the nest, which he found had been quite deserted.

XXVII. Concerning Mr. Bright, the Fat Man at Malden in Essex. By T. Cole, M.D. Dated Chelmsford, April 16, 1751, p. 188.

Mr. Edward Bright, grocer, of Malden in Essex, died there the 10th of November 1750, in the 30th year of his age. He was a man so extremely fat, and of such an uncommon bulk and weight, that there are very few, if any, such instances to be found in any country, or on record in any books. He was descended from families greatly inclined to corpulency, both on his father's and his mother's side. He was always fat from a child, yet strong and active, and used much exercise, not only when a boy, but till within the last 2 or 3 years of his life, when he became too unwieldy. He could walk nimbly, having great strength of muscles, and could not only ride on horseback, but would sometimes gallop after he became between 30 and 40 stone weight. He used to go to London about his business, till the journey (40 miles) became too great a fatigue to him; so that he left it off some years before he died. In the last year or two he could walk but a little way, being soon tired, and out of breath. At 12½ years old he weighed 144 pounds; and before he was 20 he weighed 24 stone or 336 pounds. The last time he was weighed, about 13 months before he died, his weight, exclusive of his clothes, was 41 stones and 10 pounds, or 584 pounds. What it exactly was at the time of his death, cannot be told; but as it was manifestly increased since the last weighing, if we take the same proportion by which it had increased for many years on an average; viz. about 2 stone a year, and only allow 4 pounds addition for the last year, on account of his moving about but little, while he continued to eat and drink as before, this will bring him to 44 stone or 616 pounds neat weight.

As to his measure, he was 5 feet 9½ inches high. His body round the chest just under the arms measured 5 feet 6 inches, and round the belly 6 feet 11 inches. His arm in the middle of it was 2 feet 2 inches about, and his leg 2 feet 8 inches.

He had always a good appetite, and when a youth used to eat somewhat remarkably; but toward the end of his life, though he continued to eat heartily, and with a good relish, yet he did not eat more in quantity than many other men of good appetite. Though he did not take any liquor to an intoxicating degree, yet perhaps on the whole he drank more than might have been advisable to a man of his very corpulent disposition. When he was a very young man, he was fond of ale and old strong beer; but afterwards his chief liquor was small beer, of which he commonly drank about a gallon in a day. In other liquors he was extremely moderate, when by himself, sometimes drinking half a pint of wine after dinner, or a little punch, and seldom exceeding his quantity; but when he was in company, he did not confine himself to so small an allowance.

He enjoyed for the most part as good health as any man, except that in the last 3 years, he was 2 or 3 times seized with an inflammation in his leg, attended with a little fever; and every time with such a tendency to mortification, as to make it necessary to scarify the part. But by the help of scarifications and fomentations, bleeding largely once or twice in the arm, and purging, he was always soon relieved.

He married when 22 or 23 years old, and lived a little more than 7 years in that state; in which time he had 5 children born, and left his wife with child of the 6th, near her time.

His last illness, which continued about 14 days, was a miliary fever. It began with pretty strong inflammatory symptoms, a very troublesome cough, great difficulty of breathing, &c. and the eruption was extremely violent. His body began to putrify very soon after he was dead; so that notwithstanding the weather was cool, it became very offensive the next day before a coffin could be made. The coffin was 3 feet 6 inches broad at the shoulders, 2 feet 3½ inches at the head, 22 inches at the feet, and 3 feet 1¼ inch deep.

XXVIII. The Effects of the Hyoscyamus Albus, or White Henbane. By Dr. J. Stedman, late Surgeon Major to the Regiment of the Royal Grey Dragoons.* p. 194.

In the month of August 1748, while the Greys were cantoned in the village of Vucht near Boisleduc in Dutch Brabant, 5 men and 2 women of that regiment having eaten of the leaves of the hyoscyamus albus, shred and boiled in broth, were soon after seized with a giddiness and stupor, as if drunk. Dr. S. saw them

* The plant here mentioned was, as Mr. Watson afterwards remarks, the *hyoscyamus niger*, Linn. or common henbane.

about 3 hours after eating of it; and then 3 of the men were become quite insensible, did not know their comrades, talked incoherently, and were in as high a delirium as people in the rage of a fever. They all had low irregular pulses, slavered, and frequently changed colour: their eyes looked fiery, and they catched at whatever lay next them, calling out that it was going to fall. They complained of their legs being powerless. He mixed what ipecacuanha he had with him in warm water, and made them drink it; and afterwards threw in as much warm water and oil, as he could prevail with them to swallow. Those who were not insensible vomited freely, and were relieved by it. Two of the 3 affected with delirium, though they drank great quantities, did not vomit, but had profuse sweats, and passed plenty of urine, by which they were likewise somewhat relieved. The 3d of these was obstinate, and could not be prevailed on to do any thing. The symptoms with him continued longer, and were more violent. He was so restless, that though he could not walk, 2 of his comrades were not able to keep him in a chair. Next morning they had no other complaint than people commonly have after great drinking; but afterwards (though the danger seemed over) some of them complained of feebleness and a weight at their stomachs; others, of gripes, stitches, headach; and all of them were vertiginous at times. These complaints continued above a month after the accident. One of the women had her hands stiff and swelled; whether from the action of the vomit, or the force of the poison, he knew not. The man who gathered these leaves in mistake for another plant, said, that from the nearest conjecture he could make, there might be from 15 to 20 leaves, boiled in about 10 quarts of water. They did not eat half of that quantity, and the poison began to discover itself with some of them in half an hour. This seemed to be the hyoscyamus major albus of Caspar Bauhin. It is easily known by its large duskish bell-flower; but if not in the flower, the remarkably noisome smell of the leaf, somewhat narcotic, if once known, will ever after discover it.

Some time before this accident, some of the horses had been put into an orchard, where they cropped the branches of these trees, and in about 4 hours, without any previous symptom of disorder, dropped down, and after a struggle of a minute or two died. This was probably about the time that the juice entered the blood.

Remarks by Mr. Wm. Watson, F.R.S.—On reading the above paper, Mr. Watson observed, that the effects could not arise from the hyoscyamus albus, or white henbane, as Dr. Stedman imagines; that plant, from the concurrent testimony of the best botanical writers, not being found so far north as Brabant: but the mischief was done by the hyoscyamus niger, or black henbane, which grows plentifully there, as well as almost all over Europe in uncultivated places, and by the sides of roads. The white on the contrary is sown in gardens, and not found spontaneous in higher latitudes than the southern parts of France. Dr. Stedman's description demonstrates likewise that the above plant was the hyoscyamus niger, as he says, that 'it is known by its duskish bell-flower.' The flower of black henbane is of that hue, being of a yellow colour interspersed with veins of purple; whereas the flower of the white henbane is of a pale yellow colour. This error arises from the improper denomination imposed on many plants by the ancients, and which has been preserved even since the revival of letters; which, to one not very well acquainted with botany, is liable to mislead. Thus, in the case before us, the leaves of the black henbane are very little less white than those of the white; but this denomination took its rise from the different colour of their seeds. In such cases therefore, without being well acquainted with the specific difference of each plant, before it ripens its seed, it is not a little difficult to distinguish them one from the other. This specific difference will be best furnished by the leaves. Thus in the henbane, the leaves of the white are placed on long footstalks; those of the black have none, but the lower extremity of the leaf surrounds the stalk.

XXIX. The best Proportions for Steam-engine Cylinders, of a Given Content, considered. By Francis Blake, Esq. F.R.S.* p. 197.

The steam-engine, for draining of mines, is a master-piece of machinery, a very capital contrivance in the works of art, and meriting our attention for further improvements. The prodigious vessel of water to be kept always boiling, when only an inconsiderable part of it is employed in the work, savours too little of the frugality of nature, which we ought ever to imitate. But waving that now, what Mr. B. inquires into here, and endeavours to regulate, is the proportion of the cylinder's altitude and base; which has not been hitherto noticed.

It is evident, in the first place, from a general law of mechanics, that the content of the cylinder remaining the same, the quantity of water discharged at each lift will in all cases be equal, by only changing the distance of the centre of the piston from the fulcrum of the balance. It will be granted also that the excess of the column of atmosphere, above that of the water, is a weight on the piston, driving it to a depth of about 5 feet, by the present construction, within the cylinder; acceleratedly till friction and an impediment from the steam, which remains in the cylinder even after the jet d'eau, and is increased in elasticity while its bounds are diminished, shall equal the accelerative force; and that then again the piston is retarded the rest of the way. It may be convenient to re-

* Francis Blake, Esq. a gentleman of great fortune, and a very learned man, was the father of the present Sir Francis Blake, Bart. of Twizel Castle in the county of Northumberland, also a learned and very respectable character.

mark too, that if the rarefaction be so complete, that the descent would be greater than the construction admits of, the retardation is augmented by a brachium of the balance pressing on springs. But to say nothing of friction here, we can, notwithstanding this diminution of force by the remainder of steam within the cavity of the cylinder, demonstrate the ratio of the velocities, and the times of descent of the pistons, in cylinders of unequal altitudes, to be exactly the same as if the resistance was nothing; whence we shall without difficulty arrive at some conclusion in this matter.

MN is the working part of a steam engine cylinder, of the usual height, equal in diameter to a shorter one m n, fig. 1, pl. 6; and the rarefaction in both of them being supposed the same, AQ=aq, RQ=rq, and AR=ar, may represent the excess of the atmosphere's weight above the column of water, the resistance to the pistons from the remainder of steam, and the effective force, respectively, e. g. at the beginning of the descent. Take then every where ak: AK:: an: AN, and at all similar positions the resistance bc of mn and force kc on its piston, will be equal to the resistance BC of MN and force KC on its piston; and by what Sir Isaac Newton has demonstrated (Book 1, Prop. 39,) of the descent of bodies, we have $\sqrt{}$akcr: $\sqrt{}$AKCR:: celerity in k: celerity in K. But these areas being evidently as the corresponding parallelograms kq and KQ, and these again as their heights, the celerities generated are in the subduplicate ratio of ak: AK, as if the resistance had been nothing; and by an obvious enough reasoning from the said proposition, the times also appear to be in the above-mentioned ratio; which ratio is not any way varied, though the resistance prevails from the intersecting points o.

Now, to apply what has been said to the business in hand; if TW be a cylinder of equal content with the cylinder MN, the quantity of water delivered by both will, as a consequence of the fundamental law of mechanics observed above, be the same at each lift: but the cylinder TW is no higher than nm, and ex hypoth. their rarefactions are equal; therefore by what has been proved with regard to the times, the time of the piston's descent in TW, will be to that of the piston's descent in MN:: $\sqrt{}$EW: $\sqrt{}$AN; whence in any given time the broad cylinder TW will perform more than the longer one MN of equal content, and that in the ratio of their diameters; for $FE^2 \times EW = MA^2 \times AN$ ex hypoth. and EW: AN:: MA^2: ET^2, consequently $\sqrt{}$EW: $\sqrt{}$AN:: MA: TE. The friction too is diminished with the slowness of the motion, and because the periphery increases in a less ratio than does the area of a circle.

The result of the whole then is in favour of the broad cylinder; and still the broader the better; for unless some mechanical considerations should limit the problem, it is evident in a geometrical sense, that there is no limitation. A disadvantage might arise perhaps to the effect of the jet d'eau from thus increasing the breadth; which however would probably be remedied by a number of these jets: but be that as it may, it is certain, that to augment the diameters, and diminish the lengths of the smaller kind of cylinders, now used, could have no such inconvenience, nor fail of being attended with an augmentation of force.

XXX. Mr. John Bradley's Observation of the Occultation of Venus by the Moon. Communicated by Mr. James Short, F.R.S. p. 201.

Mr. Gael Morris having favoured Mr. Short with the observation of the late occultation of Venus by the moon, taken at Greenwich with great exactness by Mr. John Bradley, he laid the same before the Royal Society, in order to show its very near agreement with those phases, which Dr. Bevis observed at his house in Surry-street, allowing for the difference of meridians.

Apparent time.				
1751 April 15,	22^h	41^m	45^s	The first contact; doubtful to 1 second.
		42	18	Quite immerged.
	23	15	$36\frac{1}{2}$	Began to emerge.
		16	$8\frac{1}{2}$	Wholly emerged.
16,	1	39	12	Venus passed the meridian.

XXXI. An Account of Mr. Benjamin Franklin's Treatise, intitled, Experiments and Observations on Electricity, made at Philadelphia in America. By Wm. Watson, F.R.S.* p. 202.

Mr. Franklin's Treatise, lately presented to the Royal Society, consists of 4 letters to his correspondent in England, and of another part intitled, ' Opinions

* Dr. Benjamin Franklin, one of the most celebrated philosophers and politicians of the 18th century, was born at Boston in North America, in the year 1706. His father was a tallow-chandler there, and young Franklin was taken from school at 10 years of age to assist him in that business. But after two years spent in this situation, he was apprenticed to an elder brother, then a printer in Boston, who in 1721 began to print a newspaper there; the copies of which our author was sent to distribute, after having assisted in composing and printing it. On this occasion, our young philosopher enjoyed the secret and singular pleasure of being the much admired author of many essays in this paper; a circumstance which he had the address to keep a secret, even from his brother himself; and this when he was only 15 years of age.

The frequent ill usage from his brother produced a separation between them, when our author, at 17 years of age, withdrew privately to New York, and thence to Philadelphia, where he worked with a printer a short time. Here he was much noticed by Sir Wm. Keith, governor of the province, who advised him to go to England to purchase printing materials, to commence the business on his own account in Philadelphia, promising to advance him the money, and send him letters of credit to London for that purpose. This promise however was never fulfilled, and Mr. F. was thus thrown upon London at 18 years of age, without either money, friends, or credit. He soon found employment however as a journeyman printer; and after continuing about 18 months in this station; he returned to Philadelphia in 1726, along with a merchant of that town, as his clerk. But his and conjectures concerning the properties and effects of the electrical matter arising from experiments and observations.'

master dying the same year, he again applied to the printing business, and soon after set up a printing house himself. About the same time Mr. F. selected, and assembled together, a few youths like himself, of a literary and philosophical turn of mind, forming a club or society, to meet on certain days to converse on such subjects, to read books, and to write useful essays. Their collection of books gradually increased, and at length advanced to a public library. The other colonies, sensible of its advantages, began to form similar plans; and hence originated the libraries at Boston, New-York, Charlestown, &c.; that of Philadelphia having since become equal to any in Europe.

About 1728 or 1729, young Franklin set up a newspaper in Philadelphia, which proved very profitable, and otherwise useful, as affording an opportunity of making himself known as a political writer. He now became a public man; his talents began to be generally known, and in consequence he was appointed successively to the offices of printer to the House of Assembly, clerk to the General Assembly of Philadelphia, and post-master, and at length a member of the general assembly itself. In 1738 he formed the first fire-company there, to prevent and extinguish fires in houses, &c. also insurances from the same; plans which still exist, and were soon imitated by other persons and in other places. In 1744, during a war between France and England, the French and Indians falling on the back settlements, by Mr. F.'s exertions a body of 10,000 volunteers were raised for their defence and security.

Pursuits of a different nature next occupied his chief attention for some years. Being always much addicted to the study of natural philosophy; and the discovery of the Leyden experiment in electricity having rendered that science an object of general curiosity; Mr. F. applied himself to it, and greatly distinguished himself in it. By his experiments he made a number of important discoveries, and proposed ingenious theories to account for various phenomena; which have since been generally adopted. His observations he communicated, in a series of letters, to his friend Mr. Collinson in England, by whom they were published; the first of which is dated March 28, 1747. In these he makes known the power of points in drawing and throwing off the electric matter, on which he afterwards founded his celebrated method of securing buildings from the stroke and damage of thunder and lightning, having previously proved experimentally the identity of electricity and the matter of lightning: on similar principles too he explained the aurora borealis.

In the year 1749 he proposed a plan of an academy, to be erected in the city of Philadelphia, as a foundation for posterity to found a seminary of learning, more extensive and suitable to future circumstances; and in 1750 three of the schools were opened, viz. the Latin and Greek school, the mathematical school, and the English school. This foundation soon after gave rise to another more extensive college, incorporated by charter in 1755, which is now in a very flourishing condition. In this last year, when he returned to London, he met with the greatest respect from all learned men: he was elected F.R.S., and had the honour of the Society's gold medal for his philosophical discoveries; he had also the degree of doctor of laws conferred on him by different universities. But at this time, by reason of the war which broke out between England and France, he returned to America, and interested himself in the public affairs of that country, with most effectual benefit. In 1757 he was again sent to England as agent for the province of Pennsylvania, Massachusetts, Maryland, and Georgia; after remaining here 5 years, he returned to America in 1762, where he received public thanks for his faithful services. In 1764 he again returned to England as a provincial agent, where he remained many years. In 1766 he was examined before the House of Commons relative to the state of America, particularly as to the stamp act, which was soon after repealed. But the troubles were now beginning, and the British government seemed resolved to accelerate rather than divert the storm. Dr. F. remained in Europe till 1775, and then returned to his native country, The 4 letters, the last of which contains a new hypothesis for explaining the several phenomena of thunder-gusts, have either in the whole or in part been before communicated to the R. S. It remains therefore now only to lay before the Society an account of the latter part of this treatise, as well as that of a letter intended to be added to it by the author, but which arrived too late for publication with it.

This ingenious author, from a variety of well adapted experiments, is of opinion, that the electrical matter consists of particles extremely subtle, since it can permeate common matter, even the densest metals, with such ease and freedom, as not to receive any perceptible resistance. Electrical matter, according to him, differs from common matter in this, that the parts of the latter mutually attract, and those of the former mutually repel each other; hence the divergency in a stream of electrified effluvia: * but that, though the particles of electrical matter do repel each other, they are strongly attracted by all other matter. From these 3 things, viz. the extreme subtilty of the electrical matter, the mutual repulsion of its parts, and the strong attraction between them and other matter, arises this effect, that when a quantity of electrical matter is applied to a mass of common matter of any size or length within our observation (which has not already got

having first endeavoured in vain to dissuade the ministry from their coercive measures. His fame stood as high in the political as it had done in the scientific world. He became an active member of the new legislative assembly, and America is indebted for the formation of its constitution to this virtuous and enlightened philosopher. After this important service he was sent ambassador to France, to negociate an alliance with that country, in which he was completely successful. He also acted as one of the plenipotentiaries for his country in signing the treaty of peace with England in 1783. Two years after, he returned again to America, and received from his grateful countrymen those honours and distinctions which he had so justly merited. At length, after rendering to mankind the most essential benefits as a natural and moral philosopher, the infirmities of age and sedentary employments increasing fast upon him, he became more and more afflicted with the gout and the stone, till the time of his death, which happened the 17th of April 1790, at 84 years of age.

To record Dr. Franklin's numerous discoveries and experiments, with the many useful institutions founded by his means, and the other curious transactions of his long and valuable life, would require an ample volume: and indeed a posthumous volume has been published, drawn up by himself, but containing only about half the term of his life; which leaves a general wish that the remainder of such interesting memoirs may one day see the light.

Dr. F. was author of very numerous tracts and essays on various branches of natural philosophy, as well as on politics and miscellaneous subjects, which have been published in different forms. His diction was easy, natural, and flowing; and his conversation at once amusing and instructive. His temper and manner lively, innocent, playful, interesting. His character leading and persuasive, not commanding. Among his playfellows, while a boy, he was always the captain, leader, and conductor; among men of all descriptions, in maturer age, he was the life and soul of every company.

* As the electric stream is observed to diverge very little, when the experiment is made in vacuo, this appearance is more owing to the resistance of the atmosphere, that to any natural tendency in the electricity itself. W. W.—Orig.

its quantity) it is immediately and equally diffused through the whole. Thus common matter is a kind of sponge to the electrical fluid; and as a sponge would receive no water, if the parts of water were not smaller than the pores of the sponge; and even then but slowly, if there was not a mutual attraction between those parts and the parts of the sponge; and would still imbibe it faster, if the mutual attraction among the parts of the water did not impede, some force being required to separate them; and fastest if, instead of attraction, there were a mutual repulsion among those parts, which would act in conjunction with the attraction of the sponge: so is the case between the electrical and common matter. In common matter indeed there is generally as much of the electrical as it will contain within its substance: if more is added, it lies without upon the surface,* and forms what we call an electrical atmosphere; and then the body is said to be electrified.

It is supposed, that all kinds of common matter do not attract and retain the electrical with equal force, for reasons to be given hereafter; and that those called electrics per se, as glass, &c. attract and retain it the strongest, and contain the greatest quantity. We know that the electrical fluid is in common matter, because we can pump it out by the globe or tube; and that common matter has near as much as it can contain; because, when we add a little more to any portion of it, the additional quantity does not enter, but forms an electrical atmosphere; and we know that common matter has not generally more than it can contain; otherwise all loose portions of it would repel each other, as they constantly do when they have electric atmospheres.

The form of the electrical atmosphere is that of the body which it surrounds. This shape may be rendered visible in a still air, by raising a smoke from dry resin dropped into a hot tea-spoon under the electrized body, which will be attracted and spread itself equally on all sides, covering and concealing the body. And this form it takes, because it is attracted by all parts of the surface of the body, though it cannot enter the substance already replete. Without this attraction it would not remain round the body, but be dissipated in the air. The atmosphere of electrical particles surrounding an electrified sphere is not more disposed to leave it, or more easily drawn off from any one part of the sphere than from another, because it is equally attracted by every part. But that is not the case with bodies of any other figure. From a cube it is more easily drawn at the corners than at the plane sides, and so from the angles of a body of any other form, and still most easily from the angle that is most acute; and for this

* The author of this account is of opinion, that what is here added, lies not only without upon the surface, but penetrates with the same degree of density the whole mass of common matter, upon which it is directed.—Orig.

reason points have a property of drawing on, as well as throwing off the electrical fluid, at greater distances than blunt bodies can.

From various experiments recited in our author's treatise, the preceding observations are deduced. And the following are a few of the other most singular ones. The effects of lightning, and those of electricity, appear very similar. Lightning has often been known to strike people blind. A pigeon, struck dead to appearance by the electrical shock, recovering life, drooped several days, ate nothing, though crumbs were thrown to it, but declined and died. Mr. F. did not think of its being deprived of sight; but afterwards a pullet, struck dead in like manner, being recovered by repeatedly blowing into its lungs, when set down on the floor, ran headlong against the wall, and on examination appeared perfectly blind; hence he concluded that the pigeon also had been absolutely blinded by the shock. From this observation we should be extremely cautious, how in electrizing we draw the strokes, especially in making the experiment of Leyden, from the eyes, or even from the parts near them.

Some time since it was imagined, that deafness had been relieved by electrizing the patient, by drawing the snaps from the ears, and by making him undergo the electrical commotion in the same manner. If hereafter this remedy should be fantastically applied to the eyes in this manner to restore dimness of sight, it will be well if perfect blindness be not the consequence of the experiment.

By a very ingenious experiment our author endeavours to evince the impossibility of success, in the experiments proposed by others of drawing forth the effluvia of non-electrics, cinnamon, for instance, and by mixing them with the electrical fluid, to convey them with that into a person electrified; and our author thinks, that, though the effluvia of cinnamon and the electrical fluid should mix within the globe, they would never come out together through the pores of the glass, and thus be conveyed to the prime conductor; for he thinks, that the electrical fluid itself cannot come through, and that the prime conductor is always supplied from the cushion, and this last from the floor. Besides, when the globe is filled with cinnamon, or other non-electrics, no electricity can be obtained from its outer surface, for the reasons before laid down. He has tried another way, which he thought more likely to obtain a mixture of the electrical and other effluvia together, if such a mixture had been possible. He placed a glass plate under his cushion, to cut off the communication between the cushion and the floor; he then brought a small chain from the cushion into a glass of oil of turpentine, and carried another chain from the oil of turpentine to the floor, taking care that the chain from the cushion to the glass touched no part of the frame of the machine. Another chain was fixed to the prime conductor, and held in the hand of a person to be electrified. The ends of the two chains in the glass were near an inch from each other, the oil of turpentine between.

Now the globe being turned could draw no fire from the floor through the machine, the communication that way being cut off by the thick glass plate under the cushion: it must then draw it through the chains, whose ends were dipped in the oil of turpentine. And as the oil of turpentine, being in some degree an electric per se, would not conduct what came up from the floor, the electricity was obliged to jump from the end of one chain to the end of the other, which he could see in large sparks; and thus it had a fair opportunity of seizing of the finest particles of the oil in its passage, and carrying them off with it; but no such effect followed, nor could he perceive the least difference in the smell of the electrical effluvia thus collected, from what it had when collected otherwise; nor does it otherwise affect the body of the person electrified. He likewise put into a phial, instead of water, a strong purging liquid, and then charged the phial, and took repeated shocks from it; in which case every particle of the electrical fluid must, before it went through his body, have first gone through the liquid, when the phial is charging, and returned through it when discharging; yet no other effect followed than if the phial had been charged with water. He has also smelt the electrical fire, when drawn through gold, silver, copper, lead, iron, wood, and the human body, and could perceive no difference; the odour being always the same, where the spark does not burn what it strikes; and therefore he imagines, that it does not take that smell from any quality of the bodies it passes through.

Mr. Franklin, in a letter to Mr. Collinson some time since, mentioned his intending to try the power of a very strong electrical shock on a turkey. He accordingly has been so obliging as to send an account of it, which is to the following purpose. He made first several experiments on fowls, and found, that 2 large thin glass jars gilt, holding each about 6 gallons, were sufficient, when fully charged, to kill common hens outright; but the turkeys, though thrown into violent convulsions, and then, lying as dead for some minutes, would recover in less than a quarter of an hour. However, having added 3 other such to the former 2, though not fully charged, he killed a turkey of about 10 lb. weight, and believes that they would have killed a much larger. He conceited, that the birds killed in this manner eat uncommonly tender.

In making these experiments, he found that a man could, without great detriment, bear a much greater shock than he imagined; for he inadvertently received the stroke of 2 of these jars through his arms and body, when they were very near fully charged. It seemed to him an universal blow throughout the body from head to foot, and was followed by a violent quick trembling in the trunk, which went gradually off in a few seconds. It was some minutes before he could recollect his thoughts, so as to know what was the matter; for he did not see the flash, though his eye was on the spot of the prime conductor, from whence it struck the back of his hand; nor did he hear the crack, though the bystanders said, it was a loud one; nor did he particularly feel the stroke on his hand, though he afterwards found it had raised a swelling there of the size of half a swan-shot or pistol-bullet. His arms and the back of his neck felt somewhat numbed the remainder of the evening, and his breast was sore for a week after, as if it had been bruised. From this experiment may be seen the danger, even under the greatest caution, to the operator, when making these experiments with large jars; for it is not to be doubted, but that several of these fully charged would as certainly, by increasing them, in proportion to the size, kill a man, as they before did the turkey.

On the whole, Mr. Franklin appears in this work in the light of a very able and ingenious man; that he had a head to conceive, and a hand to carry into execution, whatever he thought might conduce to enlighten the subject of which he was treating; and though there are in this work some few opinions, in which Mr. W. could not perfectly agree with him, he thought scarcely any body was better acquainted with the subject of electricity than Mr. F. was.

XXXII. On Dr. Hales's Ventilators; also the Temperature and Saltness of the Sea, &c. By Captain Henry Ellis, F. R. S. dated Jan.* 7, 1750-51, *at Cape Monte Africa, Ship Earl of Halifax.* p. 211.

The following is a detail of the experiments, which Capt. E. made to prove the utility of the ventilators. 1. He took a wax candle, of 8 to the pound, and drew it through a mold, to make it of one thickness from end to end: then weighed it exactly, and lighted it in the ship's hold: where it wasted 67 grains in 30 minutes; that place not being ventilated during 24 hours, but after 6 hours ventilation it wasted 94½ grains in the same time.

2. He carried into the hold a plate of silver, well polished, and a lantern and candle, all blinded, except a round hole of about 2 inches diameter. He placed the plate at 6 feet distance from it; and with such obliquity, that the rays from the light should fall on its surface at an angle of 45 degrees. He then fixed a white paper screen, at the same distance from the plate, and at the same angle as the lantern, so that the reflected rays might fall on it also. This done, he observed, that the reflection from the plate distinctly was only 17^m 30^s with an unventilated hold; it being turned the colour of tarnished lead; whereas, when the air was replaced by 4 hours ventilation, it continued to reflect light, and retain its brightness 4^h 47^m.

3. The ship's bell, whose diameter is 14 inches, he had brought into the hold, when ventilation had been omitted 12 hours. Having hung it under the lower

* Who published an account of his voyage to Hudson's Bay.

deck, he took out the clapper, and having suspended it also by thread, which, with its own length, made 44 inches; the angle, which the rim of the bell made, with a line let fall perpendicular from the pin, on which the clapper hung, was equal to 34′. He then held the clapper at the same angle, on the other side of the line, in order that the strokes at different times might be with the same force; when, letting it go, it struck the bell. In its return he catched it, and counting the vibrations, he heard them distinctly only 3 times; whereas, when the hold was well ventilated, it vibrated 5 times; but its vibrations were not so quick in the latter as in the former case.

The ship's crew was very healthy, though their number was 130, not one being sick aboard. The hold, which in most ships is very moist, in theirs was quite dry. Their cargo arms, kept there in upright chests, without wrappers, came out as bright as from a recent polish. The ventilator was far from being inconvenient aboard of them, on the contrary, it was good exercise for the slaves, and a means of preserving the cargo and lives. On the passage, Capt. E. made several trials with the bucket sea-gage, in latitude 25′ 13″ north, longitude 25′ 12″ west. He let it down to different depths, from 360 feet to 5346 feet; when he discovered, by a small thermometer of Fahrenheit's, made by Mr. Bird, which went down in it, that the cold increased regularly, in proportion to the depths, till it descended to 3900 feet: whence the mercury in the thermometer came up at 53 degrees; and though he afterwards sunk it to the depth of 5346 feet, that is, a mile and 66 feet, it came up no lower. The warmth of the water on the surface, and that of the air, was at that time by the thermometer 84 degrees. The water might be a degree or two colder when it entered the bucket, at the greatest depth, but in coming up had acquired some warmth; for he found that the water which came up in the bucket, having stood 43 minutes in the air, the time of winding it up, the mercury rose above 5 degrees. When the air had rendered it equally warm with the water on the surface, he tried their weight, by weighing equal quantities very exactly, as also by the hydrometer, and found that from great depths the heaviest, and consequently the saltest water.

This experiment, which seemed at first but mere food for curiosity, became very useful to them. By its means they supplied their cold bath, and cooled their wines or water at pleasure; which was vastly agreeable in that burning climate.

On the preceding account Dr. Hales remarks that the bucket sea-gage, above-mentioned, and which he provided for the Captain to find the different degrees of coolness and saltness of the sea, at different depths, was a common houshold pail or bucket, with 2 heads in it: which heads had each a round hole in the middle, near 4 inches diameter, covered with valves which opened upwards; and that they might both open and shut together, there was a small iron rod fixed to the upper part of the lower valve, and at the other end to the under part of the upper valve: so that, as the bucket descended with its sinking weight into the sea, both the valves opened by the force of the water, which had by that means a free passage through the bucket. But when the bucket was drawn up, then both the valves were shut by the force of the water at the upper part of the bucket: by which means the bucket was brought up full of the lowest sea-water, to which it had descended.

When the bucket was drawn up, the hole at the bottom was stopped with a cork, to keep the water in, when the valves were opened, to come at the mercurial thermometer, which being tied to an upright stick, could readily be unfastened, by pulling out a loose nail, which went into the upper end of the stick, which was fastened at its lower end in the same manner. But great care must be taken to observe the degree the mercury stands at, before the lower part of the thermometer is taken out of the water; else it would immediately be altered by the different temperature of the air. To keep the bucket in a right position, 4 cords are fixed to it, which reach about 3 feet below it, to which the sinking weight is fixed.

XXXIII. Observations on the Roman Colonies and Stations in Cheshire and Lancashire. By Thomas Percival, Esq.; communicated by Hugh Lord Willoughby of Parham, F.R.S., p. 216.

In the second iter of Antonine's Itinerary, we find, after several other stations, mentioned Eboracum, Calcariam M. P. IX, Camulodunum M. P. XX, Mamuci um M. P. XVIII, Condate M. P. XVIII, Devam M. P. XX. Though with various readings of the names.

It is agreed, that Deva is Chester, and that Mamucium or Manucium or Mancunium, is Manchester, by the common consent of all antiquarians. But where Condate is situated, is yet a matter of debate; Mr. P. thinks it must have been what is now Kinderton. Though Mr. Camden and others declare for Congleton; and some also for Norwich. The great question, where Cambodunum is situated, whether according to Mr. Camden, at Almondbury, or, according to Mr. Horsley, at Greatlandmoor, may be so far determined, that is, at neither. Mr. P. rather thinks it was about Kirklees, or near Rastrick on the banks of the Calder.

XXXIV. An Account of Professor Winkler's Experiments relating to Odours passing through electrized Globes and Tubes, being the Extract and Translation from the Latin, of two Letters sent by that Gentleman to Cromwell Mortimer, M. D. Sec. R. S. With an Account of the Result of some Experiments made here with Globes and Tubes, transmitted from Leipsic by Mr.

Winkler to the R. S., in order to verify the Facts before-mentioned. By Mr. W. Watson, F. R. S., p. 231.

Professor Winkler, in his first letter to Dr. Mortimer, dated at Leipsic, March 12, 1748, mentions, among other particulars, that if odoriferous substances were included in glass globes and tubes closely stopped, and if these globes were electrized, the smell of the odoriferous substances would as easily as the magnetical power pass through the glass, and be conveyed with the electrical effluvia to considerable distances, on substances readily conducting electricity: that when a man was electrized with a globe of this sort, the odoriferous matter pervaded his whole body; and that not only his skin and his cloths, but his breath, saliva, and sweat, were impregnated with the smell of the substance included in the glass. That after these globes had been rubbed a few minutes, the flavour of their contents would be strongly perceptible on entering the chamber in which this operation was performing; and that the substances which he had then tried, were sulphur, cinnamon, and balsam of Peru.

Mr. Winkler mentions, that when he made use of sulphur in his globe, in company with his friend Mr. Haubold, and others, the smell of the sulphur was perceived at more than 10 feet distance, and was so prevalent, that his company was driven away by it: but that himself staying in some time longer, his cloths, his body, and his breath, were infected by it; and that this smell even continued on him the next day. Further, on his repeating the experiment, as he had before found, that sulphur had been useful to him, he on the third day found in his mouth manifest indications of an inflamed blood. After this he wanted to transmit a pleasant odour; and for this purpose employed cinnamon, which under the like circumstances sent forth its odour in great abundance; so that it was not only immediately perceptible to any one entering the chamber, but continued there the next day.

Balsam of Peru, under the like treatment, so impregnated the air of the room, that the cloths and the breath of the persons in it smelled of the balsam, after having passed through several streets; and that Mr. Winkler, when drinking his tea next morning, still perceived its flavour. A few days after, when the smell of the chamber was gone off, he conducted a chain on silk lines from it, through the open air into another chamber quite separate from the former. In this second chamber he placed a man on a silk net, who held the chain in his hand, and after having electrized him with the sphere containing balsam of Peru for a quarter of an hour, any person who was perfectly ignorant of what was doing, would immediately smell the balsam in it. The man who was electrized, said that his tea next morning had a finer taste than usual.

As these experiments did not succeed here, though attempted with a due attention to whatever could be imagined necessary; and as they had done so no where on the continent, Italy alone excepted, Dr. Mortimer was desired by the Royal Society to acquaint Mr Winkler of this want of success, and at the same time to desire him to transmit hither, not only a circumstantial account of the manner of making his experiments, but likewise, lest the difference of the result might arise from employing different kinds of glass, some globes and tubes fitted up under his own eye in the most advantageous manner. This Mr. Winkler was so obliging as to comply with; and accordingly the Society has received from him 2 globes and 4 tubes; and at the same time this gentleman sent a letter to Dr. Mortimer, dated at Leipsic, Nov. 23, 1750, minutely describing his manner of using them.

The tubes and globes referred to above, were received by the R. S. about the middle of May 1751, and were presented to that body by the president at their next meeting; and they were put into Mr. W.'s hands, that their effects on trial might be reported at a future meeting. The largest sphere was of crystal glass of about 7 inches diameter, fixed to its wooden spindles by a resinous cement, and contained not more than half an ounce of a terebinthinate fluid, less deep in colour than balsam of Peru, and more so than balm of Gilead. The smaller globe was 5 inches in diameter, mounted nearly as the larger one, and contained about half an ounce of beaten cinnamon. The tube containing the flowers of sulphur was 2 feet in length, and about half an inch in diameter: it, like the globes and the other tubes, was of crystal glass, and in like manner with the rest of the tubes was hermetically sealed. The tube, said to contain balsam of Peru and chalk, was about 20 inches long, and $\frac{3}{8}$ of an inch in diameter: that said to contain opobalsamum was about 16 inches long, and half an inch in diameter: and that with spirit of wine and chalk was about 17 inches long, and about half an inch in diameter. The manner of mounting these globes might be somewhat exceptionable for the purposes intended, as the necks were fitted to their wooden blocks with a resinous cement without glass stoppers; so that when the globes, from their being rubbed, had warmed the cement, if an odour of the matter contained in the glass had been perceptible, it might have been urged, that it came through the cement with more probability than through the glass: but nothing of this kind could be objected to the tubes, as they were hermetically sealed.

June 12, 1751, there met at the house of Mr. W. in order to make trial of the effects of these glasses, Martin Folkes, Esq. P. Nicholas Mann, Esq. V. P. Dr. Mortimer and Peter Daval, Esq. Secs. Mr Canton; and Mr. Schrader, a gentleman of distinction well known to, and corresponding with Mr. Winkler. The presence of this gentleman was fortunate, as he was thus enabled to satisfy both himself and Mr. Winkler of the zeal and address which were exerted in order to verify Mr. Winkler's assertions. The weather was dry, and very fit for electrical experiments. Not the least alteration had been made in Mr. Winkler's globes. The largest globe, said to contain opobalsamum, was first put to the trial: it was first rubbed a considerable time with a dry hand chalked, and the snaps at the prime conductor were but weak; but on rubbing the globe, first with the cushion and afterwards with read leather, the snaps were much stronger; and Mr. Canton, as well as another gentleman present, were electrized by turns with it: but all this while no smell of the balsam could be perceived by any of the company, either on the equator of the globe, or the persons electrized, or the prime conductor, or any of the rubbers made use of; though for this purpose they carefully observed, not only the method suggested by Mr. Winkler, but such others as appeared the most conducive to the present purpose. When the globe was heated, indeed, by applying the nose to the mounting, a smell of resin in it was perceived; but this was all.

They next tried the less globe containing cinnamon, and most punctually observed Mr. Winkler's directions: but all endeavours were to no purpose, for they could never, after many trials, either smell the cinnamon, or make the electricity the least perceptible on the prime conductor.

They then began with the tubes: but not one of the company, after very many trials in different ways, could perceive the least odour of the substances contained, either on the outside of the tubes, or on the substances electrized by them. They thus spent more than 2 hours without success, in endeavours to see the effects proposed by Mr. Winkler; for they were not able to verify them in one single instance.

XXXV. On the Bishop of London's Garden at Fulham. By Mr. William Watson, F. R. S., p. 241.

Mr. W. here gives an account to the R. S. of the remains of that famous botanic garden at Fulham, where Dr. Henry Compton, formerly bishop of London, planted a greater variety of curious exotic plants and trees, than had at that time been collected in any garden in England. This excellent prelate presided over the see of London from the year 1675 to 1713; during which time, by means of a large correspondence with the principal botanists of Europe and America, he introduced into England a great number of plants, but more especially trees, which had never been seen here before, and described by no author: and in the cultivation of these, as we are informed by the late most ingenious Mr. Ray, he agreeably spent such part of his time, as could most conveniently be spared from his other most arduous occupations.

Mr. Ray, in the 2d volume of his history of plants, which was published in the year 1688, gives us a catalogue of the rare and exotic trees and shrubs, which he had just before observed in the bishop's garden, which he at that time called hortus cultissimus, novisque et elegantioribus magno studio nec minore impensa undique conquisitis stirpibus refertissimus. As this prelate's length of life and continuance in the see of London were remarkable, so we find the botanists, who wrote after Mr. Ray, most frequently mentioning in their works the new accessions of treasure to this garden; and of this we meet with a great variety of examples in the treatises of Dr. Pluknet, Herman, and Commelin.

On the death of Bishop Compton, all the green-house plants and more tender exotic trees were, as Mr. W. was informed by Sir Hans Sloane, given to the ancestor of the present Earl Tylney at Wanstead. And the curiosities of this garden were no longer attended to, but left to the management of ignorant persons; so that many of the hardy exotic trees, however valuable, were removed, to make way for the more ordinary productions of the kitchen garden.

Mr. W. then subjoins a catalogue of the exotic trees remaining in the Bishop of London's garden at Fulham, June 25, 1751. These are the remains of that once famous garden; among which are some, that notwithstanding the present great improvements in gardening, are scarcely to be found elsewhere. From the length of time they have stood, several of the trees are by much the largest of their kind he ever has seen, and are probably the largest in Europe. This account of them therefore is not merely a matter of curiosity; but we learn from it, that many of these trees, though produced naturally in climates and latitudes very different from our own, have grown to a very great magnitude with us, and have endured our rude winters, some of them for almost a century: and that in proper soils and situations they may be propagated to advantage, as well as for beauty. For the exemplification of this he recommends to the curious observer the black Virginian walnut-tree, the cluster-pine, the honey locust, the pseudo-acacia, and ash-maple, &c. now remaining at Fulham.*

XXXVI. Of an Inverted Iris, observed on the Grass in September, and another in October, 1751. *By Philip Carteret Webb, Esq. F. R. S.*, p. 248.

Sept. 24, 1751, about 10 in the morning, Mr. W. observed a solar iris on a grass lawn near his house, at Busbridge in Surry. The morning was fair and clear, and the grass of the lawn was the night before almost covered with webs resembling those of spiders, which many persons esteem the forerunners of fair weather; and there had fallen in the night much dew, with which the webs and the grass were thoroughly wetted. The arch or bow appeared inverted, the

* Of the hardy exotics enumerated in the above paper, there were remaining in 1793 the following: acer negundo, cupressus sempervirens, juniperus virginiana, gleditsia triacanthus, juglans nigra, quercus alba, quercus suber, &c. most of them trees of a great height, and of large dimensions in the girth. See Lysons Environs of London, 2nd vol. p. 351.

point being distant about 24 inches from the point of his foot; and where ever he moved on the lawn, it seemed to move at that distance before him. The lawn on which he observed this appearance, is a hanging level, which drops about 6 feet in 100. It extended itself to the end of the lawn, the grass of which was short, and it was not visible on the surface of the adjoining water, or grass fields. It was about 2 feet wide, and the colours were vivid and distinct.

Oct. 3, 1751, at 30^{m} after 9 in the forenoon, he observed about the same spot a like iris. It was a very fair morning: there had fallen much dew in the night, and the lawn was then, and the night before, webbed over as it was the 23d of September.

XXXVII. Extract of several Letters from John Huxham, M. D. of Plymouth, F. R. S. and Mr. Tripe, Surgeon, at Ashburton in Devonshire, concerning a Body found in a Vault in the Church of Staverton in that County: Communicated by Thomas Stack, M. D. F. R. S.

Mr. Tripe to Dr. Huxham, dated Ashburton, June 28, 1750.

According to the register of burials, no person had been deposited in this vault since October 15, 1669, so that a body had lain there upwards of 80 years: yet, when the vault was opened about 4 months before the above date, it was found as perfect in all its parts, as if but just interred. The whole body was plump and full; the skin white, soft, smooth, and elastic; the hair strong, and the limbs nearly as flexible as when living. A winding sheet, which was as firm as if but just applied, inclosed it from head to foot; and 2 coarse linen cloths, dipped in a blackish substance like pitch, infolded the winding sheet. The body thus protected was placed in an oaken coffin, on which, as it was always covered with water, was found a large stone and a log of wood, probably to keep it at the bottom.

Various have been the conjectures as to the cause of its preservation; but the pitch-cloths and water seem to account for it; the former, by defending the body from the external air, and the latter, by preserving the tenacity of the pitch. The left side, from the middle of the forehead to the scrotum, having been for some time exposed to the air, was become black, and mouldered away; but where the pitch-cloths remained, the parts underneath were perfectly fresh and firm. As the coffin was pretty much injured, though entirely sound when the vault was first opened, the body was ordered by Mr. Worth, of Worth near Tiverton, whose ancestor he was, to be speedily removed to another, and then nailed up.

Dr. Huxham to Dr. Stack, May 21, 1751.

Mr. Tripe, on dissecting the corpse found the heart and lungs as sound as if the person had not been dead above 4 days, but much more flat and compressed than usual, the joints very flexible and supple; the knees in particular, the patella, tendons, ligaments, and the whole articulation being as smooth, unctuous, and flexible, as in a body newly dead.

Simon Worth, Esq. whose corpse this was, died at Madrid, and was sent home in the manner described, and so buried. His wife's coffin, who was buried in the same vault 2 years before, and 2 of his children about 11 years after (as appeared by the register) were quite rotten. The oaken coffin, pitch-cloth, and water, seem greatly to have contributed to the preservation of this body. His coffin was found very sound.

Mr. Tripe to Dr. Huxham.

Mr. Tripe here observes that on dissecting the corpse, and examining the internal parts, he found them answerable to the external, most of them nearly in a natural state, but little altered or different from the condition of a living body.

XXXVIII. Extract of a Letter from Professor Euler, of Berlin, to the Rev. Mr. Caspar Wetstein, Chaplain to Her Royal Highness the Princess Dowager of Wales. p. 263.

You have doubtless heard that the Academy at St. Petersburg have fixed a prize of 100 ducats, which they will give every year to him who shall give the best answer to the question that shall be proposed; and for the first time they have proposed this question:

"Whether the theory of Sir Isaac Newton is sufficient to explain all the irregularities which are found in the motion of the moon?"

This question is of the last importance; and I must own, that till now I always believed, that this theory did not agree with the motion of the apogee of the moon. Mr. Clairaut was of the same opinion; but he has publicly retracted it, by declaring that the motion of the apogee is not contrary to the Newtonian theory. On this occasion I have renewed my inquiries on this affair; and, after most tedious calculations, I have at length found to my satisfaction, that Mr. Clairaut was in the right, and that this theory is entirely sufficient to explain the motion of the apogee of the moon. As this inquiry is of the greatest difficulty, and as those who hitherto pretended to have proved this nice agreement of the theory with the truth, have been much deceived, it is to Mr. Clairaut that we are obliged for this important discovery, which gives quite a new lustre to the theory of the great Newton: and it is but now that we can expect good astronomical tables of the moon.

XXXIX. Extract of Two Letters from Dr. Alston, Bot. Prof. at Edinburgh, to Dr. Mortimer, Sec. R. S. The first dated* 17*th March,* 1749; *the second, August* 9, 1750. p. 265.

A property of quick-lime, which Dr. A. believed had not been observed before. In June 1743, for some experiments in vegetation, he infused about 2 lbs. of quick-lime in 24 lbs. of water, resolving to change the lime, as soon as it did not communicate its virtues to the water. He soon made use of the first lime-water, and filled the vessel with fresh water. When that was exhausted, he filled it up a third time; and so on for 20 or 30 times: for he had no reason to change the lime for 3 years; so long it was good lime-water, gathered crusts on its surface, turned syrup of violets green, vegetable infusions yellow, tasted as at first. But at the end of the third year, it gathered no more crusts, was no more lime water.

The quick-lime, which he kept dry, fell soon into a powder; it stood covered for 3 years (the vessel with the lime-water in it was an inverted large bell-glass, never covered) in the green-house. This powder he infused in water, but it communicated no virtue to it whatever. The calx viva that he used, was made of the common limestone. It was also a common observation of our farmers, that the effect of lime on lands lasts only 3 years.

Second Letter, August 9, 1750.

The paradox, which he formerly mentioned, concerning calx viva, which no body would at first believe, he had demonstrated by repeated experiments, by which it appears, that the stone calx viva may afford more than 600 times its own weight of good lime-water; for from $\frac{1}{2}$ dr. of quick-lime he had 40 oz. of lime-water; from 1 lb. of quick-lime 500 lbs. of lime-water; and the lime was not yet exhausted, the water being as good then as at first, by every experiment that he knew. He poured some of it cold (very lately) on some small calculi, in a drinking glass, and in one night's time such phenomena appeared as notably explained, as well as confirmed, the use of lime-water in the stone. He found also, that quick-lime kept dry, in the open air, 14 months, communicated nothing to water, though long infused in it; that lime-water, boiled down to a 4th part, is not weakened, neither sensibly stronger; yet yields a very little of small slender prismatic crystals.

XL. A new Trocart for the Puncture in the Hydrocephalus, and for other Evacuations, which are necessary to be made at different Times. By M. le Cat, F. R. S. Translated from the French by Thomas Stack, M. D., F. R. S., p. 267.

This new trocart is represented by fig. 2, pl. 6, and has this peculiarity, that

* Author of lectures on the Mat. Med. 2 vols. 4to; for the most part a compilation from the older writers, and seldom referred to in the present day.

the canula is much shorter than ordinary. This canula is represented separate in fig. 3: but there ought to be several, of different lengths for different cases. On the upper part of this canula are two circles, each fastened to a different piece. These pieces are exhibited separate in fig. 4, and they are made so as to be screwed on each other. These circles are somewhat concave in their surfaces, which correspond reciprocally; so that their circumferences touch, while there is a tolerable vacuity towards their centre. By means of this simple mechanism, Mr. le Cat applied the plaster x, with a hole in it, on the lower circle A, whose screw passes into the hole of the plaster: this done, he screws the upper piece B on the lower A, and he squeezes the plaster tight between these 2 circles. The instrument becomes then as in fig. 5. The plaster, which he had chosen, is that of Andreas a Cruce; but one may use Burgundy-pitch, or any other powerful emplastic, at pleasure. His plaster was 3 inches broad. To the upper end of the canula he adapted a very exact silver stopple c, fig. 3. The part, where he intended to make the puncture, was shaved, wider than the plaster.

Thus having prepared every thing, and the canula being armed with its trocart, and fortified with the plaster, as it appears fig. 5, h. He performed the puncture on Friday the 23d of October 1744, by thrusting in the trocart and canula up to the circles and plaster, which he applied and made to stick in all its parts on the head, by pressing it with his hand and fingers made very warm, and also with hot linen cloths. When the plaster was thoroughly well fastened on, he pulled out the trocart, and drew 4 or 5 oz. of serosity, of a brownish white, or the colour of pale white wine, and somewhat foul; after which he closed the canula with its stopple c.

By chemical experiments, this liquor was found to be neither acid nor alkaline, being put on the fire, it evaporated quite away, and left at bottom a frothy neutro-saline sediment.

Saturday, Oct. 24, he unstopped the canula, and drew the same quantity of water. The infant was ill on the Sunday; he therefore did not disturb him that day. Monday the 26th he was better. He drew 5 oz. more of water. Tuesday he suffered him to take rest. Every time that he made this evacuation, he bound the head with a strong capeline.* Notwithstanding these precautions, the infant died in the night between Tuesday and Wednesday; and it will presently appear, that this hydrocephalus was of an incurable sort. He opened it, and found the brain applied against the dura mater as usual; but this brain was thin, and as it were spread out; it only formed a kind of thin sack filled with water. He opened, and saw that the disease was nothing more than an excessive dilatation of the two lateral ventricles, by the waters collected there. The glandula pinealis

* A bandage peculiar to the head.—Orig.

was almost wasted, as well as the plexus choroides, of which some few vestiges only remained. On the contrary, the other vessels, which lined the inside of this sack, were very visible.

As the brain is a soft viscus without elasticity, it manifestly appears, that it could not possibly resume its natural form, how slowly soever he had evacuated the waters; but perhaps the operation would have succeeded, if the seat of the dropsy had been on the outside of the brain. However that be, this trocart to him seems useful for several operations; and this is his first motive for presenting it to the Royal Society. His second motive for so doing is, the consequences which may be deduced from this observation with regard to the apoplexy.

How can one believe, that the apoplexy is caused by the extravasation of the liquids, or by the fullness of the vessels, after having seen a brain filled with water, and distended so vastly as this was, without any one apoplectic symptom? Verduc, who in his pathology proposes an objection similar to this against his own system, endeavours to solve it, but has not succeeded. The objection remains victorious.

Nevertheless, when the brain of a person dead of an apoplexy is opened, and extravasated blood is found in it, his death is imputed to this extravasation alone, and the apoplexy is pronounced sanguineous. This has happened on the death of M. de Frequienne, president of our parliament. On opening him M. le Cat found about a tea-spoonfull of blood extravasated within the medulla oblongata, between the 3d and 4th ventricle, at the beginning of the latter. Could so small a quantity of blood press on the principles of the nerves so as totally to intercept the course of the spirits? No, certainly; for this would be mistaking the effect for the cause. This extravasated blood was but an accident owing to the convulsive motions of the dura mater, and of the vessels of the whole basis of the skull, seized with the apoplectic disorder, which most commonly is nothing else but the matter of the gout or rheumatism fixing on this source of the nerves. Now this general attack, which swells and distends the dura mater throughout this whole basis, makes the blood stagnate in the vessels, some of the weakest of which burst, and at the same time closes all the canals of the nerves, and consequently kills the patient. Unless a person would choose to say, that those broken canals were those, which concurred in the substance of the brain to the formation of the spirits, that give motion to the heart; which opinion is not free from difficulties; since it is well known, that this organ receives the influences of several nerves at a time, all which ought to bear their part in this accident, which, after all, is but the rupture of a simple capillary vessel.

The drift of these reflections is to engage practitioners to have somewhat less confidence in their theories, and, for example, not to make a poor apoplectic patient die under the lancet; a thing which he had seen several times, from the notion which they hold, that it is the over great quantity of blood, that kills; for, besides that this false opinion is fatal to this patient in particular, it will still be so to all future apoplectics, if the prejudice in favour of this theory be such as to prevent seeking the true causes, and the real remedies of the apoplexy.

XLI. Observations on the Effects of the Vitrum Antimonii Ceratum. By Mons. Geoffroy, of the R. Acad. of Sciences, and F. R. S. Translated from the French by Tho. Stack, M. D., F. R. S. p. 273.

This medicine, the preparation of which was first published in the Edinburgh Medical Essays, is made by mixing an ounce of the glass of antimony in powder with a drachm of yellow wax. This mixture is kept in an iron ladle over a slow clear charcoal fire about half an hour, stirring it continually with an iron spatula, till the wax is consumed, and ceases to emit fumes. Such is the process of the preparation, published in the Edinburgh Essays. In the memoirs of the Royal Academy of Sciences for the year 1745, M. G. gave the detail of this operation, with some remarks on the changes, which wax may occasion in the glass of antimony.

Of all the preparations of glass of antimony this is doubtless the most perfect; for it is infinitely superior to the chylista of Hartman. This chylista is nothing more than a glass of antimony well pounded, and opened by acids, and then digested in spirit of wine impregnated with mastic, which never can cover the particles of this glass with coats of equal impenetrability with those formed by wax bituminized by burning. This medicine succeeds equally in bloody fluxes, diarrhœas, simple loosenesses, quartan agues, even the most obstinate, and in certain cases of the fluor albus. It must be given with caution, beginning with a very small dose, as 1, 2, or 3 grains, especially when it has been levigated again after its calcination; and thus it may be safely given to children, and even to pregnant women. In giving it to robust persons, M. G. always began by a small dose, as 4 or 5 grains, which he gradually increased to 18, according to the effects produced by less considerable doses. It sometimes vomits or purges, and sometimes cures (especially in robust constitutions) without producing any visible effect. By gradually increasing the dose of this medicine, he had given as far as 24 grains at a time, which had no other effect, but to procure 2 or 3 moderate stools the next day; but in this case it would be imprudent to continue its use without interruption; because, as it passes slowly, the dose may possibly unite with the first at the time, that it begins to operate; and these 2 doses thus joined might cause a superpurgation, which is always to be dreaded.

He would never have ventured to give this medicine to pregnant women, if chance had not convinced him, that it is not more dangerous for them than for others, when given with caution. For, among several women, whom he cured of bloody fluxes with this medicine, there were some that were actually with child, and did not know it themselves, at the time of their taking it. They were all cured, and no accident happened to any of them. In pursuance of this observation he thought he might try it, with precaution, even on sucking children. In the mean time he was very attentive to the effect of the medicine: when the first dose vomited or purged sufficiently, he did not increase the second. Sometimes he diminished it, or even totally laid it aside for some days.

When this medicine produces nothing more than keckings at stomach, and a plentiful expectoration of thick slime, the dose may be safely increased half a grain or a grain every day. And this slight augmentation of the dose does not hinder the effect of the medicine from diminishing, in proportion as the patient comes nearer a perfect cure. When the patient has been purged too violently by one of the first doses of this medicine, which are always small, it is a proof of the weakness of the patient; and then he gave it to him but every second or third day. The distance of time observed between the doses of this medicine makes it operate less briskly, and more equally. When it vomits, the patient is to drink warm water at every motion. When the dysenteric flux is attended with sharp pains in the abdomen, with heat and tension, the vitrum antimonii is not to be given, till the pains are removed by emollient clysters, and other proper remedies.

He had not observed any difference in the effects of this medicine, whether the patient had, or had not, been bled or purged: whether the disease was recent, or of long standing; whether in fine it were attended with a fever, or not. They were all cured equally well, agreeable to what is said in the Edinburgh observations.

The vitrum antimonii ceratum is a good febrifuge; 3 or 4 days use of this medicine generally suffices for removing the fever accompanying diarrhœas, loosenesses, &c. But, in order to its having this effect, it must either purge or vomit the patient; otherwise it cures the looseness, but the fever continues, and requires a very long use of the medicine to cure it. When it operates in a sensible manner, it generally gives the patient an appetite, when he is near being cured: but the weakness of his stomach does not allow his giving way to it, without running great risks. When this remedy operates a cure without producing any visible effects, it would be dangerous to increase the dose till it causes evacuations; for, unless the patient be of a strong constitution, you endanger the bringing on a hypercatharsis.

The finer the powder it is reduced to, the more efficacious it is. Also the vegetable acids develope and increase the emetic quality of this medicine to such a degree, that you would always put the patient's life, who takes it, in great danger, if you did not absolutely forbid him the use of acid fruits, and aliments, that are liable to turn sour, as milk, wine, &c. This medicine succeeds equally well in uterine evacuations. In these cases it must be continued 15 or 20 days, giving it every other day, according to the patient's strength, or the quantity given at a dose. With this medicine alone he likewise cured a girl of 18, who had the fluor albus abundantly from the age of 12.

In obstinate quartan intermittents, which had resisted the most powerful febrifuges, he had given this medicine on the 2 days of intermission, omitting it the day of the paroxysm; and continuing it thus, and increasing the dose very gradually, the paroxysms grew considerably weaker, and generally the 4th did not return.

Excepting in the cases of fevers, all the patients, who used the vitrum antimonii ceratum, drank habitually of a ptisan made with rice, oatmeal, or hartshorn. These ptisans prevent the pains of the stomach, which this medicine sometimes occasions. He had always given this medicine in a bolus incorporated with the bitter extracts, or cordial electuaries. Great care ought to be taken, not to make it up with conserves or syrups of acid fruits, for the reasons already given.

XLII. Concerning a Dwarf. By John Browning, Esq. of Barton-hill, near Bristol. p. 278.

This surprising, but melancholy subject, the son of one Lewis Hopkin, was a young man entering the 15th year of his age, though his stature was no more than 2 feet 7 inches, and his weight 13 lb. labouring under all the miseries and calamities of very old age; weak and emaciated, his eyes dim, his hearing very bad, his countenance fallen, his voice very low and hollow; a dry husky inward cough, low and hollow; his head hanging down before, so that his chin touched his breast; consequently his shoulders were raised, and his back rounded, not unlike a hump-back. His teeth were all decayed and rotten, except one fore tooth below. He was so weak, that he could not stand erect without a support.

The father and mother both said, that he was naturally sprightly, though weakly, until 7 years old, would attempt to sing and play about, and then weighed 19 lb. and was as tall, if not taller, naturally straight, well grown, and in due proportion; but that from that period he had gradually declined, and grew weaker, losing his teeth by degrees. The mother was a jolly healthy woman, in the prime of life; the father enjoyed the same blessing. They said also, that this lad has a sister about 10 years of age in the same declining state.

XLIII. On Comets. By Mr. Rich. Dunthorne. Dated Cambridge, Oct. 5, 1751. p. 281.

There is a manuscript in the Pembroke-hall college library, chiefly astrological, containing 5 tracts of different authors concerning comets. One of them intitled Tractatus fratris Egidii de cometis (written on account of a comet which appeared in 1264) contains these passages relating to its place and motion.

Prolog. " Stella caudata seu crinita apparuit in regno Franciæ in oriente ante solis ortum a 19° kalendas Augusti usque 5° nonas Octobris in anno Domini 1264.—Cap. 1. " Cometem, cujus occasione hæc scripsimus, primo vidimus extra circulum zodiaci versus aquilonem contra cancrum, et demum eundem vidimus extra circulum versus austrum sub geminis inter canem et orionem.—Cap. 3. " Vidimus autem et stellam caudatam, cujus occasione hoc scripsimus, præter motum circularem diurnum, æque moveri motu retrogradationis, et nulli alii similis, secundum latitudinem ejus, quæ est a septentrione ad austrum. Visus est moveri per duos menses solares plusquam 40 gradus, vix per 3 gradus longitudinis permutans situm.—Cap. 7. " Cometes, cujus occasione hæc scripsimus, primo visa est in vespere post solis occasum, demum post paucos dies solem pertransiens in mane circa octavum gradum cancri, et ex hinc cito processit retro in geminos:——vidimus autem et cometem moveri ab aquilone ad austrum, secundum latitudinem quidem plus 50 graduum, et secundum longitudem quidem vix 5 gradus processisse."

Hevelius in his Cometographia has also given the following paragraph, among others, concerning this comet. " A. C. 1264, stella, quæ dicitur cometes, apparuit, videlicet in oriente, ante ortum diei, post stellam matutinam; apparuit, scilicet, ante auroram cum radiis multis: ipsi ejus radii longe lateque apparuerunt antequam oriretur ipsa stella cometes. Igitur veloci cursu laboravit ipsa stella cometes, ita quod præcurrerit et longe versus meridiem præcessit stellam matutinam, i. e. luciferum. Visa est circa festum S. Mariæ Magdalenæ, et usque ad octavam S. Augustini apparuit. Compilat. Chronol."

Though this whole account be very slender and rude, it is however much the best Mr. D. had met with, of any comet earlier than that which was observed by Regiomontanus in the year 1472, (except perhaps the account given by Nicephoras Gregoras of the comet of the year 1337, whose orbit is computed by Dr. Halley); for which reason, he was induced to try, whether he could investigate a set of elements capable of representing the places of this comet agreeable to the above description; and after several attempts, some of them indeed but tentative, he fixed on the following numbers for that purpose, viz. the place of its ascending node in ♍ 19°, the inclination of its orbit to the plane of the ecliptic $36\frac{1}{2}°$, the place of its perihelion in ♑ 21°, its perihelion distance from the sun 44500 such parts as the mean distance of the earth from the sun contains 100000, and the time of its being in perihelion July 6^d 8^h p. m. The motion of the comet in this orbit was direct.

From these elements Mr. D. computed the places of the comet for the months July, August, September, of the said year, which he thinks agree as well with the foregoing description as any regular computus can be expected to do; and the resemblance of all the elements gives some ground for conjecture, that this comet might possibly be the same with that which was observed by Paul Fabritius and others in the year 1556, whose orbit Dr. Halley has computed. (See his Synopsis Astronomiæ Cometicæ). Indeed the change in the place of the perihelion may perhaps be thought greater than could arise from the mutual gravitations of the comets disturbing each other; but then it may be considered, that neither the place nor time of the perihelion, nor the perihelion distance of the comet of the year 1556, could be determined very accurately from observations made only for 12 days, at 40 days distance from the perihelion, as those of Fabritius were, unless they had been more exact than his appear to be. If these were the same comet, its period is 292 years; and we may expect its return about the year 1848.

There are in the before-mentioned manuscript, besides the passages already quoted from Egidius, two other places which deserve to be taken notice of. One of them is so much of a small tract, intitled, Judicium de Stella Cometa, Anno Domini 1301, as concerns the place and motion of the comet; it is as follows: "A. D. MCCC primo, primo die Septembris apparuit cometa in occidente, et per mensem vel amplius visus fuit.—Ultima autem die Septembris duabus horis 40 minutis post occasum solis—inveni quod longitudo cometæ in signis et gradibus erat 20 gradus scorpionis, et latitudo * 26 gradus septentrionalis: Mars autem tunc erat in 20 gradu scorpionis directus exeuns, et sic fere conjuncti erant Mars et cometa accipiendo loca ipsorum per circulum transeuntem per polos zodiaci.—Verum et sexta die Octobris, scilicet in festo sanctæ fidis post occasum solis eadem hora inveni quod longitudo ejus erat primus gradus sagittarii, et latitudo ejus 10 gradus septentrionalis.—Cometæ latitudo ecliptica circa principium apparitionis suæ fuit 20 gradus et amplius septentrionalis.—Apparebat cometa moveri a septentrione in meridiem per oriens, ita quod ejus longitudo orientalis continue videbatur augeri, et ejus latitudo septentrionalis continue videbatur diminui.—In principio apparitionis suæ coma protendebatur ad septentrionem: et post motum successive movebatur per orientem ad meridiem versus stellam quæ dicitur altayr hoc est vultur volans."

Though this account is too imperfect for us to attempt determining the orbit from, it may yet help us to know the same comet again, if any should hereafter appear whose orbit will agree with this account; which he believes none of those already computed will do.

* This figure (2) is a different writing from the rest of the manuscript, and has manifestly been altered since it was first written; it seems to have been 16° at the first, which I think the truer reading.—Orig.

The other place hinted at as worthy of notice, is this short passage in a treatise De Significatione Cometarum: " Et nos invenimus modo quod apparuit intempore nostro unus cometa in principio piscium, et cauda attigit usque ad principium geminorum in nocte Mercurii, et hoc fuit in ultimam noctem Junii, anno 499 Arab. et sequebatur ordinem signorum quousque venit usque ad principium cancri, et dimisit ordinem signorum, et incepit deficere."

The word Junii here found seems to have been transcribed by mistake for the Arabic month Jumedi.j, the last day of which that year was Wednesday Feb. 7, A. C. 1106; whereas the last day of June fell on a Saturday. This reading agrees with the following notes concerning the same comet collected by Hevelius in his Cometographia, p. 821. " A. C. 1106 a prima hebdomada quadragesimæ cometam immensi fulgoris usque ad passionem Domini conspeximus." Lavath ex Urspurg.—" A. C. 1106, mense Februar. biduo post novilunium, visus est magnus cometa, ad occasum solis brumalem." Calvis. ex Tyr.

The new moon was Feb. 5, Ash-Wednesday that year Feb. 7, and Good-Friday, March 23.

If we suppose, with Dr. Halley, this comet to be the same with that which appeared in 1680, and that it was in perihelio Feb. 4, at noon (for it must have been seen in 2 or 3 days after it had passed its perihelion) some of its places would have been these:

				com. long.		com. lat.	
Feb.	7^d	6^h	♓	7°	$50'$	5°	$44'$ north.
March	14	$7\frac{1}{2}$	♉	11	49		
	19	8	♉	15	38		
	24	8	♉	19	2.		

The wide disagreement there is between the manuscript account of this comet, and its places here computed, must very much lessen, if it does not quite overbalance, the force of the arguments brought by Dr. Halley to prove the identity of these two comets. Indeed if this comet had been the same with that of 1680, it could not have come to the beginning of Cancer, without a change in the place of the perihelion too great to be easily admitted; nor could it have left the order of the signs without a change in the elements still greater.

XLIV. Concerning the Effects of Lightning. By Mr. Franklin. Dated Philadelphia, June 20, 1751. p. 289.

In captain Waddel's account (Phil. Trans. 492) of the effects of lightning on his ship, Mr. F. could not but take notice of the large comazants (as he calls them) that settled on the spintles at the topmast-heads, and burnt like very large torches before the stroke. According to Mr. F.'s opinion, the electrical fire was then drawing off, as by points, from the cloud; the magnitude of the flame showing the great quantity of electricity in the clouds. And had there been a good wire communication from the spintle heads to the sea, that could have conducted more freely than tarred ropes, or masts of turpentine-wood, he imagines there would either have been no stroke, or, if a stroke, the fire would have conducted it all into the sea without damage to the ship. His compasses lost the virtue of the loadstone, or the poles reversed, the north point turning to the south. By electricity we have here frequently given polarity to needles, and reversed it at pleasure. Mr. Wilson tried it with too small a force. A shock from 4 large glass jars, sent through a fine sewing needle, gives it polarity; and it will traverse when laid on water.

If the needle, when struck, lie east and wests the end entered by the electric blast points north. If it lie north and south, the end that lay towards the north, will continue to point north, when placed on water, whether the fire entered at that end, or the contrary end. The polarity is given strongest, when the needle is struck lying north and south; and weakest, when lying east and west. Perhaps if the force was still greater, the south end, entered by the fire, when the needle lies north and south, might become the north; otherwise it puzzles us to account for the inverting of compasses by lightning; since their needles must always be found in that situation, and by our little experiment, whether the blast entered the north, and went out at the south end of the needle, or the contrary, the end that lay to the north, still should continue to point north.

In these experiments the ends of the needles are sometimes finely blued, like a watch spring, by the electric flame. This colour given by the flash from 2 jars only, will wipe off; but 4 will fix it, and frequently melt the needles. Sometimes the surface on the body of the needles is also run, and appears blistered, when examined by a magnifying glass. The jars Mr. F. used held 7 or 8 gallons, and were coated and lined with tin foil. Each of them takes 1000 turns of a globe 9 inches diameter to charge it. He sent 2 specimens of tin foil melted between glass, by the force of 2 jars only.

I have not heard, says he, that any of your European electricians have been able to fire gunpowder by the electric flame. We do it here in this manner: a small cartridge is filled with dry powder, hard rammed, so as to bruise some of the grains. Two pointed wires are then thrust in, one at each end, the points approaching each other in the middle of the cartridge, till within the distance of half an inch: then the cartridge being placed in the circle, when the 4 jars are discharged, the electric flame leaping from the point of one wire to the point of the other, within the cartridge among the powder, fires it, and the explosion of the powder is at the same instant with the crack of the discharge.

XLV. Observations on Fungous Excrescences of the Bladder; also a Cutting Forceps for Extirpating these Excrescences; and on Canulas for Treating these Diseases. By M. le Cat, F.R.S. Translated by Tho. Stacke, M.D., F.R.S. p. 292.

A widow woman had for some years felt pain in the small of the back, thighs, &c. In the year 1734, she made bloody urine, and had one thigh and leg œdematous. These accidents, having disappeared, were succeeded by worse symptoms: she had frequent calls to make water, and did it often, a little at a time, and with pain, which was violent, particularly after the urine was discharged, and this was of a dull red colour, or a little tinged with blood.

All the profession, as well as M. le Cat, thought that she had the stone; but he would not pronounce positively, till he had searched her; which he did the 17th of October 1735. As soon as the sound was introduced blood came away, and in greater quantity, the more it was moved about. The free play of the sound was obstructed; he found no stone, but pretty sure signs of excrescences in the obstruction of the sound, and the issue of blood, which its motion occasioned. However, by dint of management he found a situation of the sound, in which, by giving a little jerk, he touched a hard body, the dull percussion of which conveyed nothing but obscurity to his hand or judgment. In order to come at the knowledge of this body, he passed the crooked sound destined for men, the bent of which he thought fitter to favour his inquiries. He found the same body again, but still with the same obscurity. These doubts held them a long time in suspense what course to take: but the extreme pain which the patient suffered, and the frequent hæmorrhages, which would soon have put an end to her life, made them determine to perform the operation; that is, to open the neck of the bladder, either to extract the stone, if any, or remove and treat the funguses, which existed beyond all doubt.

He cut the patient the 18th of Oct. 1735, by what he calls the rural apparatus, that is, without placing her on the table used in the hospitals, which could not well be carried to the country where this woman dwelt. He placed her on the edge of her bed: a chair turned upside down supported her shoulders. Unknown to the patient he caused a board to be put under the first mattrass of this edge of the bed; and when she was placed on it, under her backside, or the os sacrum, he laid another board, on which he put a straw cushion made compact and covered with linen cloth. Two straps fixed to the ends of this board were passed into the bars of the turned up chair, which supported the patient's body: and these pieces, viz. the chair and the board with the cushion, were fastened together by buckles fixed on the straps. The assistants, who were on each side of the patient, had each a strong large swathing band folded double, and passed into this fold in a slip knot: he used one of those strong woollen sashes or girdles, with which curriers bind or swathe their body. This slip knot was passed on the patient's wrists, who had seen nothing of these preparations, and she was bound fast, almost before she was aware of it. Then he introduced a common grooved staff, such as is used for abscesses of the bladder: he turned the groove towards the patient's left thigh, and on this groove he pushed his knife into the bladder. On that knife, which had a groove, he slid the gorget and forceps in the usual manner.

He searched for the stone in vain, and found nothing but excrescences, one of which was considerably hard: he extracted several clusters of them with the forceps; yet still he was not very certain but that there might be a stone behind a rampart of excrescences which he felt; and had not brought the crooked forceps with him to search behind this intrenchment. When he judged that the patient was fatigued by his searchings, and the extirpations which he made with the forceps; he had her put to bed, after having put a canula into the wound, contrary to his usual custom; for this case required it: these strange bodies were to be removed, if possible; that organ must be injected, and consequently the canula was absolutely necessary. The patient, who bore the operation exceedingly well, was blooded 2 hours after it: she had a pretty good night, and was blooded again the next morning. He left one of his pupils with her, and returned to Rouen.

The canula, which he left in the wound, was of the common sort, and therefore too narrow to admit of searching in the diseased part, and to give issue to those excrescences, which we ought to endeavour to disengage and bring away in this treatment; besides, it is extremely difficult to make the canula remain in the wound.

As soon as he got to Rouen, he ordered the canula (pl. 6, fig. 6) to be made; the advantages of which above the old one are: 1. To afford a wider passage for the substances that are to be evacuated and introduced. 2. To secure the instrument in the bladder, by its own structure chiefly, and particularly by the swelling at BB. 3. The neck AA, which is at the basis of the swelling, is embraced by the neck of the bladder; whence the surgeon may be sure how much of the canula enters it: and the openings CC, immediately above the swelling B, are fixed at the lowest part of the bladder.

Fig. 7, 8, 9, represent the same canula, but for further improvements, for cases which require the evacuation of gross substances, the passage for which cannot be too wide and direct.

He returned to the patient the next day; and found her in a fever, with many colicky pains; but at the end of the 3d day there was nothing extraordinary. He intended to make another search, but he feared renewing those accidents: he therefore contented himself with injecting a liquid digestive; and deferred any further trials till after the suppuration was well formed, which he expected about the 8th or 9th day.

He revisited the patient on the 7th, and found her a little feverish, but she had a good night's rest. There was a small discharge through the canula of tolerably white pus, but of an intolerable smell. The canula seemed to be much clogged with sloughs; and the stench made them suspect a collection and lodgment of these sloughs behind the canula. They resolved to put in the canula above described; and as there was a necessity of dilating, in order to introduce it; they agreed to take the advantage of this dilatation, to try to discover by the crooked forceps, which he had brought with him, if there might not be a stone to be extracted, or at least some more of these excrescences, and to break or bruise such as they should not be able to draw out, that they might fall off by suppuration.

He executed this trial on the 8th day. The dilatation was made between 2 grooved sounds, as it is done in the greater apparatus between the male and female conductors. He found no stone as yet, but brought away clusters of the tops of funguses. He crushed the rest of the excrescences, and placed the large canula.

Experience had shown him that this bruising of the funguses of the bladder is more painful and dangerous than can be imagined. They are far from being of the same nature with the polypus of the nose, which is pulled out with little or no pain, and without any bad consequence. The funguses of the bladder have more consistence, more solidity, and for that reason more sensibility. Accordingly, after this last operation, the patient was seized with a violent fever, which carried her off in 2 days. He opened her body, and found the bladder in the condition represented by the figures, and their explanation.

This observation made him think, that if he should meet with a parallel case, that is, a patient with fungous excrescences in the bladder, distinctly characterised, and accompanied with pains and excessive hæmorrhages, which render the palliative cure useless and unsuccessful: and if he had a constitution and courage proper to make him hope for success from a great operation; he would find a way to attack the excrescences with a cutting instrument, the operations of which are much surer and less painful than any other method. Practitioners advise to suppurate such of these excrescences, as the fingers cannot reach, that is, those which can neither be tied nor cut. But how can one bring such sensible parts to suppuration? We have no ointment that can raise a suppuration in a sound part. Funguses are a sort of vegetation, which, though preternatural, are still living, and, in some measure, sound parts: how then are they to be disposed to suppurate? It must be either by pulling them out, or by crushing them, as he had done. But seeing this operation is dangerous, an instrument should be contrived, which might be conveyed to the bottom of the bladder, like the forceps; and which might at the same time be able to cut these inaccessible excrescences, or the greatest part of them at least; the remains of which being cut open, would thereby acquire the necessary dispositions to suppurate, which are indicated for the cure. For this purpose it was, that about that time he contrived the cutting forceps or scissars, fig. 10, pl. 6, to cut the excrescences of the bladder or uterus, which are inaccessible to the fingers. A is the bend of this instrument on the flat of its blades. B, buttons, which terminate each blade, and are at some little distance from each other, even when the blades are closed together; that these ends might neither prick nor pinch the coats of the bladder.

Fig. 6, is the new canula. A, the neck, which is to be embraced by the neck of the bladder. B, the swelling, which is to be within the neck of the bladder. C, the head, which is to be in the cavity of this organ, together with its wide openings. D, the style or sound of this canula.

Fig. 7, the same canula improved, as its end B, which M. le Cat names introductor, is screwed on the canula A at C, and is unscrewed by means of the structure of this introductor.

Fig. 8, The introductor separated from the canula. A, a wire or rod of steel, which supports the end of the introductor, and serves to unscrew it from the canula. B, the extremity of the introductor, which ought to be made of silver. CC, elastic steel plates or blades. These plates have on the inside of their edge a female screw, which enters on a male screw of the outside of the end of the canula. Their springiness makes them separate when the introductor is mounted on the canula; and by this widening asunder they leave the openings or eyes of d, d, fig. 6, 7. But when they are unscrewed, they close together, as appears in fig. 8, whereby this end becomes slender enough to pass through the canula, through which this part of the instrument is drawn out, when the canula is placed in its situation; which is the intent of this structure; for by this means the outlet becomes larger, and the excrescences cannot be fretted.

Fig. 9. The canula stripped of the part above described. A, its funnel or tube and wide straight orifice; in which consists the improvement of this last canula, which he had principally in view in the rectification of the first.

XLVI. An Account of the Cinnamon-tree. By W. Watson, F.R.S.* p. 301.

Mr. W. laid before the R. S. a specimen of the bark and wood of the cinnamon-tree, nearly of the length and size of an ordinary walking-cane, transmitted from Mr. Benjamin Robins, in India, to Dr. Letherland. And in order to convey to them at the same time a yet more perfect idea of the tree itself Mr. W. sent with it a small branch of this valuable plant from his own hortus siccus.

* *Laurus Cinnumomum*, Linn.

Cinnamon in the stick is a great curiosity, and seldom seen in Europe. Clusius tells us, that he saw 2 specimens of it. Anciently indeed it was often brought in this manner, viz. with the bark surrounding the wood; and it is believed by authors of very great credit, that the wood, not divested of its bark, as we now see it, or the bark stripped from the wood, was called by different names. And notwithstanding the various controversies, which have arisen in endeavouring to fix properly these various terms, it appeared to the late Mr. Ray, that our cinnamon, the cinnamon of the ancients, and the cassia lignea of the ancients, were quite or nearly the same thing; and that they only had their difference from the soil in which they were produced, or from the circumstances under which they were brought. Thus the younger branches of the tree with their bark covering them, were called by the Greek writers κιννaμώμον, cinnamomum, and sometimes ξυλοκασία, or cassia lignea; but when they were divested of their bark, which, by its being dried became tubular, this bark was denominated κασία σύριγξ, or cassia fistula. But as, in process of time, the wood of this tree was found useless, they stripped the bark from it, and brought that only, which custom prevails at this day.

Both Theophrastus and Pliny mention a very odd, and doubtless a fabulous account of the manner of separating the bark from the wood. They say, that it is cut into short pieces, and sewed up in a fresh hide; and that then the worms produced by the putrefaction of the hide destroy the woody part, and leave the bark untouched. However the cinnamon, or cassia cinnamomea of Herman, the cassia lignea, and cassia fistula of the ancient Greek writers, might approach near each other, they were applied by the moderns to very different substances. By cinnamon is now always understood that only produced in Ceylon; by cassia lignea, the cinnamon of Sumatra, Java, and Malabar, much inferior in every respect to the former, though nearly agreeing with it in appearance, and not at all woody, as the appellation seems to insinuate; and by cassia fistula, a fruit not described or used by the ancient Greeks, and agreeing with it in no one particular, only that both are vegetable productions: great care should be taken therefore that this confusion is not productive of error.

Burman, in his Thesaurus Zeylanicus, takes notice of his being in possession of 9 different sorts of cinnamon of Ceylon; the most excellent of which is that which is called by the inhabitants rasse coronde, and is what is most usually brought to Europe.

What we now call cinnamon, is only produced in Ceylon, of which the states of Holland are in possession; and so jealous are they of this tree, which affords so valuable an article of commerce, that the fruit or young plants are forbidden by an order of state to be sent thence, lest other powers might avail themselves of it. And this they have been hitherto successful enough to keep to themselves; though in Ceylon, according to Mr. Ray, the cinnamon-tree grows as common in the woods and hedges, as the hazel with us, nor is of greater esteem with the inhabitants than other wood, but is used by them as fuel, and applied to other domestic purposes. Probably the prohibition of sending cinnamon-trees from Ceylon is of no long standing, as Paul Herman, who resided there some time, and was after his return chosen professor of botany at Leyden, tells us, in his Hortus Lugduni-Batavus, published in 1687, that he sent several of these trees to some considerable persons in Holland, and that they continued also as well in the gardens of others, as in his own, for 2 or 3 years, and were killed by a severe winter. Mr. W. was credibly informed, that 3 of these trees in pots were presented to the late King William, by whom they were placed in the garden at Hampton-court, and were intended to be sent to Jamaica, as a country proper for their increase, under the care of the earl of Inchiquin, who was then going thither as governor. But for want of attention these trees were left behind; and as the knowledge of hot-houses, as we now see them, was unknown, and the state of gardening otherwise extremely low, these invaluable trees were suffered to die here; whereas had they been planted in some of our islands in America between the tropics, in all probability before this time we might have been supplied from them, and large sums been annually saved to the public, as great quantities of cinnamon are consumed in diet and medicine.

XLVII. Observations and Experiments on Animal Bodies, Digested in a Philosophical Analysis, or Inquiry into the Cause of Voluntary Muscular Motion. By Charles Morton, M. D., F. R. S.* p. 305.

The author of this paper is led by the experiments to which he refers, and the arguments he employs, to the following conclusions: viz. that a muscle being given, in its natural state, in a living animal body, the blood, which is present in every part of its contracting substance, and which, in effect, to the sense of the given muscle, (which is occasionally rendered more acute) puts on an in-

* Dr. Charles Morton was born in Westmorland, about the year 1716, and was a practising physician at Kendal in 1745. In 1744 he married Miss Mary Berkeley, niece of Lady Betty Germaine. His second wife was Lady Saville, mother of Sir Geo. S., to whom he was married in 1772, and who died Feb. 1791. The latter part of the same year, when he was 75 years of age, he married his third wife, Miss Eliz. Pratt, a near relation of Lady Saville. And he died at his apartments in the British Museum in Feb. 1799, being about 83 years of age. In 1751 Dr. M. was admitted a licentiate of the College of Physicians; and on the establishment of the British Museum in 1756, he was appointed under librarian of the M.S. and medal department; and in 1776, he succeeded Dr. Maty as principal librarian, which he enjoyed till his death. In 1760 he succeeded Peter Duval as Secretary to the R. S., which situation he resigned in 1774, when he was succeeded by Dr. Horsley, now the learned Bishop of St. Asaph. Dr. M. has only 2 papers in the Philos. Trans. viz. that above abstracted, and another in vol. 59, on a supposed connection between the writing of ancient Egypt and China. In 1759 he published an improved edition of Dr. Barnard's engraved Table of Alphabets. And, in 1772, Whitlocke's Journal of the Swedish Embassy in 1653, 1654. Dr. M. was a man of a sweet and amiable disposition, of great uprightness and integrity, and much admired as a scholar.

creased heat, and again lays it down at the command of the will, is the immediate mechanical cause, by which the muscle does instantly contract, and is again relaxed, at the command of the will.

Whence it would appear that muscular voluntary motion is performed merely as a sensation, (Hartley Conjecturæ de Sensu, &c.) extremely acute, and under the nicest management of the will; which explains its velocity in a great measure.

XLVIII. An Account of the Eruption of Mount Vesuvius, from its first Beginning to the 28th of October 1751, in a Letter from Mr. R. Supple. p. 315.

This communication is rendered unnecessary, from a more particular account being given at p. 245 of this volume.

XLIX. Of the Lunar Eclipse which happened Nov. 21, 1751; observed by Mr. James Short, F. R. S. in Surry-street. p. 317.

The weather was exceedingly tempestuous, and the sky overcast with clouds, so that the following times cannot be depended on to less than 2 minutes.

Penumbra very visible at.	7^h	58^m	0^s
Beginning of the eclipse at.	8	6	0
End of the eclipse at.	11	6	0

The quantity of this eclipse seemed about the middle to be larger than according to all the tables.

The Transit of the moon over the meridian.

Preceding limb passed the meridian at.	12^h	5^m	18^s
Subsequent limb passed the meridian at.	12	7	50

Mr. Pound observed a similar eclipse at Wanstead, just two sarotic periods before this, and has described it in the Philos. Trans. N° 347, and makes the following remark, "This eclipse is the more considerable, as happening very near the moon's perigee, and therefore useful to verify her anomaly; as also to limit the greatest diameter of the shadow of the earth, and consequently the parallax of the moon. This may be very properly compared with that of the 19th of October 1697, whose middle was at 7^h 41^m p. m. at London, and the quantity the same as now."

It may be added to Mr. Pound's remark above, that this eclipse happened nearer to the moon's perigee, than that which he observed in the year 1715, and therefore more proper for verifying the moon's anomaly, and limiting the greatest diameter of the shadow of the earth.

L. A Letter from the Reverend Father Augustin Hallerstein, of the Society of Jesus, Pres. of the Astron. Col. at Pekin in China, to Dr. Mortimer, Sec. R. S. Dated Pekin, Sept. 18, N. S. 1750. p. 319.

This letter contains no real or useful information; but only complaints of the missionaries' want of means and instruments and information, &c.

LI. *On Hernias with Sacks. By Mons. le Cat, F. R. S. from the French, by Tho. Stack, M. D., F. R. S.*, p. 324.

A Hernia by Rupture, having nevertheless a Sack.—In giving a private course of operations to his English pupils, on the body of a lad of 18 years old, M. le Cat discovered this hernia. The aponeurosis of the musculus obliquus externus ran over the whole tumor, and entirely covered it. At the anterior and lateral internal part of this tumor was the ring lengthened into the shape of a perpendicular button-hole; which had nothing to close it but a cellular lamina, which covered all this bag, being a continuation of the cellular membrana adiposa. Through this button-hole appeared the cellular coat, with which the peritonæum furnishes the spermatic vessels. The intestine occupied the rest of this bag; and at the bottom was contained the testicle, which consequently had never taken the way of the ring to come out of the belly, as it usually does; but having passed on one side, it had gradually pushed out the aponeurosis of the musculus obliquus externus; and the intestine having followed it, and broke the true lamina of the peritonæum, they had in concert formed this elongation. At least this is the most natural explanation he could give of this singularity. That the testicles are originally in the belly, is a fact sufficiently known. He had dissected fœtuses, in which he found them there, near the bladder. It is pretty common to feel them in the rings in children; and he had found them there even in lads of upwards of 20 years old.

A Hernia having Two Sacks.—Continuing the above-mentioned course, he found in the body of a bachelor of 48 years of age, a rupture with a double herniary sack, the first of which was formed by the expansion of the aponeurosis of the obliquus externus, as in the preceding observation, excepting that this expansion was only on the outer side, that the ring was in its usual place, that the bottom of the bag formed by this expansion had some empty spaces, where the expansion was wanting. In short, the bag was neither so complete, nor so thick as that of the foregoing observation; but on the other hand, there was a 2d bag, formed as usual by the true lamella of the peritonæum.

Another Sort of Duplicity of the Herniary Sack.—A coachman about 65 years of age, had a rupture of long standing, of the strangulation of which he had already cured him in 1748. Having taken off his truss, in order to get it mended, he was seized with strangulation the 19th of Feb. 1750. After applying all the remedies prescribed in such cases without success, he was obliged to perform the operation on the 21st at 8 in the evening. Having laid the bag open in the usual manner, which contained a little watery humour, he was much surprized at discovering within this bag a second bag, or pocket, which could be nothing else, but either a second herniary bag, or an incomplete hernia; that is, a portion only of one side of an intestine elongated, and come down through the ring. The number of considerable blood-vessels on this pocket, its thickness and fibrous texture seemed to evince the latter. But first, on pressing this bag, all its contents returned into the abdomen; 2dly, the patient assured him, even at the instant, that his rupture had kept up since its reduction in 1748; and he found this bag adhering, not only to the first bag, but also attached by old and strong adherences to the testicle and spermatic vessels; and it was impossible that this state should be the effect of 3 days of strangulation. However, as the patient might possibly have deceived him in his account; and it was dangerous to open a bag which had too near a resemblance with the gut of an incomplete hernia, he came to a resolution, which equally suited the 2 suspected cases. He separated the testicle and spermatic vessels from this sack, and pushed back this pocket, or second bag, into the belly.

The patient having died on the 9th day after the operation, they found that the pocket which had given them so much uneasiness, and which he had reduced into the belly, was really a herniary sack formed by the true peritonæum; and therefore that the first sack must have been either an interior aponeurotic lamina of the abdominal muscles, or the cellular membrane thickened by the long duration of the hernia and its strangulations. The considerable thickness of the true or second sack renders this notion very probable. He says that the first sack must have been formed by an interior aponeurotic lamina, and not from an exterior one, like that of the first observation; because, in this operation, he had freed the ring, in his usual manner, above this first sack, and without opening it. Then he passed the grooved catheter over this sack, under the aponeurosis or pillar of the musculus obliquus externus: and therefore this sack could not be a continuation of this external aponeurosis, but that of some more inner lamina, or of the cellular membrane of the very peritonæum, separated from the true lamina by the serosities which they found in it.

Two other observations are subjoined:

1*st*. *A Natural Blind Duct, being a Production of the True Lamina of the Peritonæum by the Rings.*—In the dead body of a woman, 46 years old, he found this duct of the thickness of a goose-quill, being a production of the true lamina of the peritonæum stretched out by the rings; of which Swammerdam and Nuck dispute the discovery, and Blancard denies the existence. What made him discover this, was, that its extremity was widened into the shape of a bubble as large as the top of a finger, and full of a watery humour. This woman had never had a hernia, nor even the least tendency towards one.

2*d*. *Strictures and Carnosities in the Urethra.*—Nothing is more common at this day than to hear people assert, that strictures and carnosities of the urethra are mere chimeras; that the bodies of persons, who were thought to have these strictures and carnosities, had been opened, and that none of these had been found. He himself has made this observation, and he inferred thence, that there were urethras, in which a phlogosis, a fungous inflation gave occasion to the deception, being taken for strictures and carnosities: but if he had drawn this general inference, that of all the urethras, where these strictures and carnosities are thought to be found, not one has any thing in them, he should have been deceived, and would now make his recantation.

One of his boarders preparing to perform the operation of cutting on the dead body of a bachelor, aged 45, the sound could not pass; the pupil forced, and made a false passage. Mr. le Cat opened this canal, and found, 1st, that a simple small stile could not pass into the urethra, by pushing it from the glans towards the prostate; but that it passed, by pushing it from the prostate towards the glans; 2dly, a little before the place, where the bulb becomes less thick, and begins to surround the urethra, that is, about a large finger's breadth from its beginning, there was a stricture entirely like that which Dr. Willis discovered in the upper longitudinal sinus of the dura mater; 3dly, some few lines lower down was a caruncle, or a fleshy firm bump, of the size of a pea; and below this bump, the urethra was extremely straightened; 4thly, the basis of this carnosity formed a kind of valve, and there he found the false passage, that went into the substance of the bulb.

LII. On the Effects of Lightning at South-Moulton in Devonshire. By Joseph Palmer, Esq. p. 330.

On Thursday June 6, 1751, about 3 o'clock in the afternoon, (that day, and some others before, having been extremely hot and sultry, and the wind pretty strong in the south-east) a flash of lightning, attended with an uncommon thunder-clap, which immediately followed or rather accompanied it, fell on the windows and walls of the church and steeple of South-Moulton in Devon, greatly damaging them. Many stones in the walls, &c. were broken and splintered in an extraordinary manner; also much damage done to the bells and iron spindles, and the church clock stopped. In the adjoining fields the ground was much torn up, as if ploughed, and an oblique hole made of about 3 feet deep.

*LIII. An Improvement of the Bills of Mortality. By Mr. J. Dodson.** p. 333.

There has lately been a scheme proposed for amending the form of the bills of mortality of London, in a pamphlet called "Observations on the Past Growth and Present State of London," by Mr. Corbyn Morris, who has enumerated many excellent purposes to which it may be applied, but has omitted to mention

* Mr. Dodson was an ingenious and very industrious mathematician, and the author of several useful books; but we have found few or no particulars of his life. He was some time master of the Royal Mathematical School in Christ's Hospital, London. His publications chiefly were, 1. Anti-logarithmic Canon, folio, 1742. 2. The Calculator, a collection of useful tables, large 8vo, 1747. 3. Mathematical Repository, being a collection of analytical questions and solutions, in 3 vols. 8vo, Ann. 1748, 1753, 1755.

that of giving a greater degree of certainty to the calculations of the values of annuities on lives; a benefit too considerable to be passed by silently. The present possessors of entailed estates are, in common law, justly called tenants for life. Marriage-settlements generally convey the reversion of a considerable part of the bridegroom's estate to the bride, for her natural life after his decease; to which two things all the freehold estates in these kingdoms are liable: and if to these be added the great number of copyholds determinable on lives; the great quantities of church, college, and other lands, leased on lives, and the estates possessed by ecclesiastical persons of all degrees; we shall find that the values of the possessions and reversions, of much the greatest part of the real estates in these kingdoms, will one way or other depend on the value of lives. Likewise the incomes annexed to all places, civil and military, all pensions, and most charitable donations, are annuities for life. The interest or dividends of many personalities in the stocks have been, by the wills of their possessors, rendered of the same kind; besides which, there are some annuities on lives which have been granted by the government, and have parliamentary security for their payment; and others that have been granted by parishes, in consequence of acts of parliament made for that purpose.

After this summary view of the extensive property, vested in annuities on lives, it would be very easy to name a great variety of circumstances, in which the computations of the values of 1, 2, or more lives, will become necessary to those persons who do not chuse to have their property determined by customs which seem to have been established merely for want of good methods of calculation.

The advantages attending the determination of those things by calculation, rather than by custom, being therefore considered as evident, it may seem strange that, notwithstanding many of these tenures have subsisted from the very origin of private property in these kingdoms, yet we do not meet with so much as an attempt towards computing their values, till that of the late justly celebrated Dr. Halley, by the assistance of the bills of mortality of Breslaw in Silesia, which was soon followed by Mr. De Moivre's truly admirable hypothesis, that the decrements of life may be esteemed nearly equal, after a certain age. It has been the opinion of some authors, that, since his hypothesis was originally derived from the Breslaw observations, it cannot be near so well adapted to the inhabitants of these kingdoms, as what has been derived from the bills of mortality of London. But this argument does not, Mr. D. conceives, appear to be conclusive; 1st, because those bills, as hitherto kept, are not well adapted to answer this purpose; 2dly, because the manner in which the inhabitants of London, and those of most of the country towns and villages, live, their occupations, diet, and diversions, nay the very air they breathe, are as different, as those of

London and Breslaw can possibly be; and consequently so must the times of their dissolution. All which has been, with a great deal of clearness, evinced by the gentleman above quoted; 3dly, because those persons who suppose that Mr. De Moivre's hypothesis has its foundation peculiarly in the Breslaw observations, are greatly mistaken: for Mr. D. having lately been endeavouring to discover some further helps to the speedy valuation of lives, he found that, on the contrary, if the London observations had been then in Mr. De Moivre's hands, he might as justly have derived his hypothesis from them.

For the same thing, which Mr. De Moivre mentions, concerning the equal annual decrease in a certain number of persons, happens in the table of the London observations; and the like happens in other instances, to be met with in the London observations, as published by different authors. Add to this, that having calculated the value of an annuity on a life of 10 years of age, by both tables, and also by the hypothesis, Mr. D. finds it to turn out thus:

By the Breslaw tables of observations........ 17.7237 years purchase.
By supposing the decrements of life equal.... 16.8814
By the London tables of observations........ 16.3907

From which there seems to be some reason to conclude, that the hypothesis (as it gives an answer less than the Breslaw, and greater than the London observations) may be the best method of the three. And it is further remarkable, that the result by the hypothesis, is nearer to that by the London, than to that by the Breslaw observations. However, if the argument for using the London observations has any force at all, the computation of the value of each person's life must be made from observations drawn from the bills of mortality, kept at the place of his or her residence: and therefore it is, that Mr. D. contributes as much as he can, to preserve a sufficient number of good bills of mortality. There seems to be an objection, both to the hypothesis, and to the observations; for it is well known that females, especially at two periods of their life, are obnoxious to fatal disorders not incident to the other sex, nor distinguished in the present bills of mortality; and consequently neither the tables of observations, nor the hypothesis (which is derived from them) will render the calculations of the values of lives sufficiently certain; unless there be a periodical distinction of sexes in those bills: as it would probably appear, if such a distinction had been introduced, that there is a wide difference between the values of a male and female life of the same age.

But there will be a great inconvenience, in rejecting the hypothesis, which none of these gentlemen have remedied; viz. the prolix and laborious computation hitherto directed for finding the values of lives from tables of observations; whereas, by the hypothesis, as its author justly observes, more can be concluded in a quarter of an hour, than can be performed in a quarter of a year, by any method which the others have demonstrated. Whence it may be presumed, that the hypothesis will continue to be used, till better methods are substituted instead of those derived from it.

When the bills of mortality digested into a proper form, shall have been kept a convenient time in every city or considerable town, and also in every hundred, or other proper division of the country; then, and not till then, the hypothesis may be tried by the facts that will appear from the bills, and be confirmed or rejected accordingly. Indeed Mr. D. is almost persuaded, from what has been above remarked, that the hypothesis will, in general, appear to be the nearer the truth, the more those bills of mortality shall be in number, and the correcter they are kept. He proceeds therefore to mention those alterations which he thinks may be of advantage, in the form of the bills of mortality, in every part of these kingdoms, over and above those mentioned by Mr. Morris, in the before-quoted pamphlet.

1. That there be a distinction made on the face of the bills of mortality, between the persons who were born in the place where such bills were kept, and those that were not. This will be effected with a very little trouble, if the searchers of each parish be instructed to ask the question of the friends of the deceased, and annex the answer to their report. This precaution will facilitate many of the good purposes proposed by Mr. Morris; and particularly with regard to fixing the values of lives, it will enable the persons who shall apply the bills to calculation, to draw their conclusions only from the lives that were both begun, and ended, in or near the same place; the want of the possibility of doing which is the principal objection to the London bills, as hitherto kept; 2, that there be a distinction, with regard both to age and disease, made on the face of the bills, between the sexes; and that one case be added to the list of diseases; viz. complaints peculiarly incident to the female sex. This will not only solve the difficulty above started, but also answer many purposes in political arithmetic, as well as to the sagacious physician; 3, that a further division be made in time; for whereas Mr. Morris's scheme exhibits no age between 40 and 50, Mr. D. proposes, that the numbers dying between 40 and 45, and between 45 and 50, should be particularized in the bills; the design of this being to fix the periods that are fatal to the fair sex, with more certainty.

These alterations, together with those proposed by Mr. Morris, being made, the yearly bill of mortality for London, will appear as in a specimen which Mr. D. annexed, and according to which form nearly such tables are often kept.

LIV. Concerning the Dissection of a Rupture. By Mons. Le Cat. Translated from the French, by Tho. Stack, M.D., F.R.S. p. 341.

Mr. le Cat here mentions, that when he sent the remarks on the singular hernia of Catherine Guillematre, (Phil. Trans. N°. 460) he had already made some fruitless attempts to cure her, but had not then lost all hopes of success. He imagined, that a long use of emollient cataplasms might restore suppleness to the intestine which constantly kept out of the belly, and was turned inside out, because it was the portion continuous to the cæcum, colon, rectum, and anus, which could be of no use, but much incommoded the patient by this extraordinary situation. But all his trials were of no avail, though they were carried so far as to render this gut quite bloody: its long exposure to the air made it become too thick and hard; and at the same time so robust or insensible, that all these vigorous applications made no bad impression on the rest of the animal economy. In fine, Catherine Guillematre quitted the hospital without any other benefit but that of having afforded M. le Cat and his colleagues an opportunity of instructing themselves.

From that time he had no news of this woman till the 6th of May, 1750; when he was informed, that her body actually lay in the dead ward, and that she died in the hospital of old age and a broken constitution, as much as of any disease.

He was extremely curious to embrace this opportunity of having ocular demonstration of the probable conjecture, which he had made in this woman's lifetime, and a confirmation of his having solved the enigma arising from this singular hernia; which, on opening the body, was accordingly confirmed.

LV. An Account of Dr. Bohadsch's Treatise, communicated to the Royal Society, entitled Dissertatio Philosophico-Medica de Utilitate Electrisationis in Curandis Morbis, printed at Prague, 1751. *Extracted and translated from the Latin by Mr. Wm. Watson, F.R.S.* p. 345.

The author of this treatise, Dr. Bohadsch, was a Bohemian, a very learned gentleman, who, while he was in England about 2 years before, was frequently at the meetings of the R.S., and was very conversant with, and much esteemed by many of that body, from whom he received very great civilities. He was more particularly taken notice of by his grace the late Duke of Richmond, whose loss we yet lament.

This treatise, from its title, promises only an account of the advantages of electrization in medicine: but this is not the whole of which it treats; it exhibits also a series of observations of the effects of electricity on both solid and fluid bodies, on animals in a state of health, as well as on the distempered.

Our author first takes notice that electricity, being continued for some hours, lessens the weight of the body electrified. He exemplifies this first on fluid bodies; two equal portions of which, before electrizing, he accurately weighs; and then the difference between these 2 portions, one of which has been electrized between 4 and 5 hours, and the other, though in the same room, not electrized at all, is attributed to the operation of the electric effluvia, viz. 4 oz. of river water exposed in a glass vessel of 4 inches diameter were electrized 5 hours, and lost in their weight 8 grains. But 4 oz of river water, in the same kind of glass, but not electrized, lost in the same time only 3 grains. And so of other fluids, less or more. Also each lost more by electrizing in a tin vessel, than in a glass one. When the vessels were narrower, all the fluids lost proportionally less. And when the opening was nothing, or close stopped, the evaporation was no thing by electrization.

Hence our author concludes, 1. That electricity augments the natural evapo ration of liquors, unless those of a viscous kind, as oil of olives, which from their tenacity lose nothing of their weight. 2. That electricity increases the evaporation of liquors in proportion as they are more or less volatile. 3. That electricity operates most in those vessels, which are most permeable to its effluvia, viz. in vessels of metal more than those of glass. 5. That the effects of electrizing are not observed in vessels closely stopped.

He afterwards put to the trial several substances of a more solid form. And from these experiments he observes, that the electricity diminishes the weight of solid bodies, only if these are impregnated with humours liable to evaporate: and therefore it is only on the fluids in them that the electricity operates.

Dr. B. then exhibits some experiments made by persons of credit, to discover, whether electricity would accelerate the growth of plants; and from several trials he found that it did. There then follows a series of experiments, which prove, that electricity augments the transpiration of animals. He proceeds to give a theory of those distempers in which electricity seems to have the greatest effects. He confines himself however more particularly to the hemiplegia; of which distemper he gives the history, corresponding with what we find in the best medical writers. He likewise gives the usual method of cure, and shows that the attempts of relieving this malady by electricity, nearly square intentionally with the remedies most celebrated in practice. That the electrical sparks and commotion produce the same effect, though in a more powerful manner, as warm sulphureous baths, frictions, sinapisms, stinging with nettles, &c. generally made use of in the cure of this distemper. This reasoning does very well in theory; but Mr. W. would have been glad to have seen it justified by practice, and his own observations. But instead of these, our author contents himself with giving over again the lying stories of Pivati, &c. He finishes this dissertation, by deducing several conclusions from what he has premised, which are as follow:

1. That electricity may be advantageously applied to medicinal purposes. 2. That it augments the natural transpiration of animals. 3. That this acceleration of transpiration in men is through the exhaling capillary vessels, and not through the subcutaneous glands. 4. That the nervous fluid may be called the electrical fluid. 5. That the nerves subservient to sensation are not different from those subservient to motion. 6. That the immediate cause of the hemiplegia is the immeability of the nervous fluid through the nerves. 7. That of all other distempers the hemiplegia seems most properly the object of electricity. 8. That it may be of use also in intermitting fevers. 9. That a palsy in the left side of the body is owing to the right side of the brain, and vice versa. 10. That anger, the parent of numerous evils, is sometimes useful to paralytics. 11. That as long as the paralytic limbs are rigid, it is an argument, that the bursal ligaments of the joints, and the sheaths of the tendons, are deficient in the fluid adapted by nature for their lubrication. 12. That every species of palsy does not arise from the nerves being either obstructed or compressed.

LVI. Of a Horizontal Top, invented by Mr. Serson By Mr. James Short, F.R.S. p. 352.

The horizontal top, the invention of Mr. Serson, who was unfortunately lost in his majesty's ship the Victory, is pretty well known. This ingenious person found, that when this top is set a going in the proper way, its upper side, which is polished, about 2 minutes after it was set up, moved in such a manner, as to give a true horizontal plane; and that this plane was not at all disturbed by any motion or inclination you give the box, in which it is placed, and therefore might be proper to be used aboard a ship; by which means seamen might be enabled to take the altitude of the sun or stars, in order to find their latitude, even though they cannot see the horizon in thick hazy weather. Some gentlemen were of opinion, that the air had some share in the cause of this horizontality. Mr. Short therefore applied to Mr. Smeaton, who had the best air-pump he ever saw, all of his own invention and construction. Having set the top a going, they put a receiver over it, and immediately exhausted the air. By repeated trials it had been found that the top, when set a going in the open air, played or spun during the space of 35 minutes of time, from the instant of its being set up till it had lost the circular motion: but they found, that in the exhausted receiver it played or spun during the space of 2 hours 16 minutes, preserving a perfect horizontality for the space of $\frac{3}{4}$ of an hour; and therefore, that the air has no share at all in the cause of its horizontality, but that the air is a great impediment to its motion.

LVII. Observations made in going up the Peak of Teneriffe. By Dr. Thomas Heberden, and communicated by Wm. Heberden, M.D., F.R.S. p. 353.

At 2 o'clock in the afternoon they set out from the villa or town of Orotava, about 6 leagues distant from the peak of Teneriffe. The weather was cloudy; and before they had travelled quite a league, they found themselves surrounded by a very thick mist or fog, which lasted about a league: all which time they travelled among gardens and woods of pine-trees, after which they came to an open country; the soil very dry; here and there a single pine-tree, and some few Spanish broom-plants; some loose stones, as large as a butt; others, which seemed to have been burned, and are supposed to be cast out from the volcano of the peak. The sky very clear, and the thick mist, which they had passed through, now seemed a sea of ash-coloured clouds below them. Having travelled 2 leagues on this soil, they arrived at 8 o'clock in the evening at the Falda del Pico, or foot of the peak. Here they were obliged to leave their horses; the road, by reason of its steepness and loose sandy soil, being impassable to them. At half a league's distance they baited under some large rocks, called La Estancia de los Ingleses, or the English baiting-place, being first used as such by some of our countrymen in ascending the peak. Here they rested all night, making fires to temper the air, which they found very cold. When the morning drew near, they proceeded on their journey, ascending for a quarter of a league the same soil (but more steep and loose) till they arrived at some large rocks of mal-payses (or stone burnt by a volcano); among which, as the ground was more firm, they walked with less trouble, or rather climbed, being frequently obliged to make use of their hands to help them forward. Having gone about a quarter of a league in this manner, they arrived at the famous cave of Teyde. It is surrounded on all sides (or rather buried) with large mal-payses, or volcanian rocks, between which you discover the entrance about 6 feet high, and 4 feet wide. The cave seems to be about 15 feet wide at the entrance; the extremity they could not discover. From its entrance to the surface of the water, which covers the bottom, seems to be about 12 or 14 feet. The top and sides of the cave are of smooth stone. The bottom is covered with ice or snow; above which is a body of water about half a yard deep. This cave is the grand reservoir of snow of the island, whence they are supplied, when their common reservoirs, which they prepare for cooling their liquors, fail them.

At somewhat more than a quarter of a league's distance from the cave, they came to a plain of sand; from the middle of which arises a yellowish pyramid of sand or cinders, which the inhabitants call La Pericosa, and we the Sugar-loaf; around the base of which perspire vapours incessantly. The sugar-loaf is about an 8th part of a league to the top, which is very difficult of ascent, occasioned by the loose soil, and steepness of the road. About 8 o'clock in the morning they gained the summit or caldera. It is about 12 or 15 feet deep: the sides, sloping down to the bottom, form a concavity, or crater, resembling a truncated cone, with its base uppermost. The crater seems nearly circular; its diameter about 40 fathom. The ground is very hot; and from near 20 spiracula, as from so many chimneys, you perceive a smoke or vapour of a strong sulphureous smell. The whole soil seems mixed or powdered with brimstone, which forms a beautiful coloured surface.

One of the rocks forms a sort of vault or nich; against which the vapour condensing produces what the inhabitants call azufre de gota, or drop brimstone. The nich, against which the vapour is condensed, is of a greenish colour, sparkling with yellow like gold. The same colour you perceive on almost all the stones thereabout. A small part of the Sugar-loaf is white like lime; and there is another less part, whose internal substance seems a sort of red clay, and whose superficies is covered with a salt.

In the middle of one of the rocks was a hole, about 2 fingers breadth in diameter, whence proceeded a noise like a great body of liquor boiling very strongly; and one of the company burnt his hand by applying it to the spiraculum at $\frac{1}{4}$ of a yard distance. This Sugar-loaf is covered with snow the greatest part of the year. The snow was lying on it from October 1742 to June 1743.

The different accounts of various authors concerning the height of this famous peak would have induced one less inquisitive than Mr. H. to satisfy his curiosity, by examining its real altitude: for which end, between 3 and 4 o'clock in the afternoon of a very serene day, when not a cloud appeared, either on the summit, or in the whole atmosphere, (to prevent any accidental refraction) having pitched on a plain along the sea-side for a horizontal stand, and measuring trigonometrically a base sufficiently corresponding to the angles with the greatest accuracy, he observed the height to be 2566 fathoms, or 3 miles, wanting only 74 fathoms. Two subsequent observations by himself, as well as 2 antecedent ones some years before by John Crosse, Esq. the British consul, served only to confirm his opinion of the justness of this observation.

Though the body of the mountain is covered with clouds, the peak is generally seen above them quite clear; but sometimes the contrary happens; the whole body of the mountain without a cloud, and only the summit of the peak covered with a thick white cloud, as with a cap. This is often observed in the finest weather; when the Spaniards say, El Pico tiene su sombrerillo puesto; i. e. 'The Peak has put his little hat on;' and think it a certain sign of rain.

During 6 or 7 years, that Dr. H. lived in the villa of Oratava, as he had a continual sight of the peak, he several times observed the above phenomenon, and did not remember one instance in which the prediction of rain failed.

LVIII. Observations of the Weather in Madeira, made by Dr. Thos. Heberden, and communicated by Wm. Heberden, M. D., F. R. S. p. 357.

The thermometrical observations are made with Fahrenheit's thermometer, and the calculations deduced from 2 observations daily; at 7 o'clock in the morning, and at 3 in the afternoon. The same method of calculation is to be understood of the barometer. The rain fell through a funnel 15 inches in diameter. The Lesté, Levant, or hot winds, are very troublesome. The remedy is, to keep within doors. October 1749, comparing 2 of Fahrenheit's thermometers together, one of them exposed on the north side of his house to the open air, the other within doors, the difference was as follows:

	Hour.	Therm. within-doors.	Therm. exposed to the air.
Lesté, Oct. 20,	10, 12, 4	73, 76, 77	81, 82, 77

Madeira, Anno 1749. *Anno* 1750.

	Barometer. 1749			Thermometer. 1749			Barometer. 1750			Thermometer. 1750		
	Mean Height.	Greatest Height.	Least Height.	M. H.	G. H	L. H.	Mean Height.	Greatest Height.	Least Height.	M. H.	G. H.	L. H.
Jan.							29.195	29.8	29.4	64.	68	62
Feb.							29.692	29 75	29.5	63.8	67	61
March	29.81	30.2	29.8	64.66	70	61	29.12	29.65	29.3	66.5	71	61
April	30.075	30.2	29.8	60.7	68	64	29.285	29.4	29.1	66.45	68	65
May	29.55	30.1	29.6	66.53	69	65	29.775	29.9	29.5	66.25	68	65
June	30.017	30.15	29.75	68.75	72	64	29.875	30.1	29.5	69.06	72	6
July	30.027	30.1	29.95	74.58	75	72	29.887	29.95	29.8	73.	75	71
Aug.	30.013	30.1	29.95	75.07	77	74	29.386	30.1	29.75	75.4	78	72
Sept.	30.054	30.15	29.85	76.53	78	72	29.915	30.05	29.7	74.93	77	72
Oct.	29.841	30.	29.7	72.2	77	68	29.797	29.9	29.5	73.87	77	70
Nov.	29.68	30.	29.55	68.6	73	67	29.875	30.05	29.55	70.825	76	67
Dec.	29.675	29.9	29.4	64.9	68	62	29.843	30.2	29.7	66.27	74	64

An Account of the quantity of Rain, which has fallen in the Island of Madeira.

	Anno 1747.	1748.	1749.	1750.
	Inch Dec.	Inch Dec.	Inch Dec.	Inch Dec.
January	20.525	8.600	2.097	7.150
February	.485	10.958	1.203	1.771
March	4.339	5.241	.932	1.123
April	.528	.722	.777	.039
May	.353		5.290	1.087
June	1.321	.420	.113	.226
July	.200			.176
August	.018	2.700		.003
September	.540	.810	.855	1.682
October	.010	3.303	1.512	6.601
November	5.181	2.654	3.059	5.611
December	7.351	1.500	6.527	1.882
Totals	40.851	37.508	22.365	27.351

The Years 1749 and 1750, were such dry years, that the corn was destroyed, and the fruit-trees suffered much, particularly the peach-trees, the fruit either falling to the ground while green, or, if it remained longer on the tree, being full of white worms.

LIX. Extract of a Letter from Mr. Willem Van Hazen to Mr. Philip Miller, F.R.S. concerning the Quantity of Rain, which fell at Leyden in the Year 1751. p. 360.

During the course of the year 1751, it rained at Leyden no less than 163 days; and the quantity of rain which fell was 41 inches.

LX. Of a Double Child. By Thomas Percival, Esq. p. 360.

This uncommon child was born January 1752, at Hebus near Middleton, 5 miles from Manchester. The child, or children if they may be so called, are both females. The one is a perfect healthy looking fine girl. The imperfect one adheres to the perfect one by the cartilago ensiformis, by a cartilaginous substance 4 inches in circumference. The body seems to be of a soft fleshy substance of very little regularity: it has no head nor neck, nor any respiration: from the upper parts of its body come out two short arms. On the right, which is the longer. are 4 fingers, but no thumb; on the left, which is very short, its hand is very deficient, and on it are only 2 fingers. The thighs, legs, and feet, are the most perfect, though the legs have only one bone in them. It has no vertebræ of the back or loins. The os sacrum, as well as the os pubis, imperfectly ossified. All its joints are very rigid and stiff. It has no anus, but passes off its water in the natural way. Its sternum is very imperfect; and it has no clavicula. It seems insensible of pain, not removing its arms or legs, if laid in an uneasy posture.

LXI. On the Phenomena of Electricity in Vacuo. By Mr. Wm. Watson, F.R.S. p. 362.

From a comparison of experiments in electricity made in vacuo, with those already made in open air, it appears that our atmosphere, when dry, is the agent by which, with the assistance of other electrics per se, we were enabled to accumulate electricity in and upon non-electrics; that is to communicate to them a greater quantity of electricity than these bodies naturally have. That, on the removal of the air, the electricity pervades the vacuum to a considerable distance, and manifests its effects on any non-electric substances, which terminate that vacuum; and that by these means, originally-electric bodies, even in their most perfect state, put on the appearance of non-electrics, by becoming themselves the conductors of electricity.

The experiments treated of in this paper must be considered to have been made in a vacuum by Mr. Smeaton's air-pump, that rarefies 1000 times. The electrical machine, with its prime conductor, need here no particular description; but that of the glass, in which the vacuum was made, should be more minutely considered. It consisted of a glass tube nearly 3 feet in length, and of almost 3 inches in diameter. A ring of brass, exactly fitting this tube, was cemented to both its extremities, into each of which was screwed a hollow brass cap, nearly of an hemispherical figure. Into the top of one of these caps was adapted a brass box of oiled leathers, through which was admitted a slender brass rod of a length sufficient to reach within 8 inches of the other extremity of the tube. Into the top of the other brass cap was fastened a brass rod, like the former, only of 8 inches in length. Thus the extremity of one of these brass rods might at pleasure, without letting in the air, be made to touch the other; and for the better observing what difference in effect would arise from an increase of surface, a small brass circular plate was made to screw into each of these extremities. The intent of being able to bring the extremities of these rods near together, and to separate them again to what distance you pleased, was, that it might without difficulty be determined, whether, and to what distance, the electrical fluid would manifest itself in vacuo, farther than in air of the same density with the external.

The tube then thus fitted, and made dry both within and without, was placed in a cylinder of brass, of about 2 inches long, and of a diameter just sufficient to admit the brass cap before mentioned; and round the rim of this brass cylinder, to prevent the ingress of air, was adapted a narrow piece of wet leather. These being placed on the plate of the air-pump, which stood upon cakes of wax, a piece of wire passed from the prime conductor to the long brass rod, at the other extremity of the tube, and by these means, on setting the electrical machine in motion, the long brass rod in the tube was electrified. When the brass plate at the bottom of this rod was placed near, or even at the distance of 2 inches from the plate of the other rod, the brushes of electrical fire were seen passing from the periphery of the upper plate to that of the lower, and every part of the air pump snapped on the touch of any one standing on the floor, and gave the other usual signs of the accumulation of electricity. But, as these plates were made to recede from each other, this effect grew less and less; so that when they were removed 5 or 6 inches from each other, no snaps could be drawn from the air-pump; as the dissipation of the electric fluid was now as easy from every part of of the prime conductor, as from the upper brass plate in the tube.

On exhausting this tube, and electrizing as before, the air-pump still standing upon cakes of wax, the electrical fire was not only seen to pass from one plate to the other at the distance of 5 inches, but the same effect ensued at the greatest distance, to which in the tube the brass plates could be drawn. Being therefore desirous to see a further effect, and to avail himself of the whole length of this tube, Mr. W. took from the inside of it the short brass rod, to which the lower brass plate was fixed, and fastened this plate at the very bottom of the tube into the cap. The consequence was, that the electricity, meeting with scarcely any resistance, passed from the top to the bottom of the tube, and electrized the air-pump as before: and it was a most delightful spectacle, when the room was darkened, to see the electricity in its passage; to be able to observe, not, as in the open air, its brushes or pencils of rays an inch or 2 in length, but here the coruscations were of the whole length of the tube between the plates; viz. 32 inches, and of a bright silver hue. These did not immediately diverge as in the open air, but frequently, from a base apparently flat, divided themselves into less and less ramifications, and resembled very much the most lively coruscations of the aurora borealis.

At other times, when the tube has been exhausted in the most perfect manner, the electricity has been seen to pass between the brass plates in one continued stream of the same dimensions throughout its whole length; and this, with a subsequent observation, seems to demonstrate, that the cause of that very powerful repulsion of the particles of electrical fire one to the other, which we see in open air, is more owing to the resistance of the air than to any natural tendency of the electricity itself; as we observe that its brushes from blunt bodies, when the electricity is strong, diverge so much, as to form, when seen in the dark, an almost spherical figure. This figure seems therefore to arise from the electricity's endeavouring to insinuate itself between the particles of air. The figure that an elastic fluid of less density must form, when let loose, and equably compressed by one more dense and more elastic, must necessarily approach to that of a sphere.

On admitting a very small quantity of air into the tube, these phenomena disappeared; not so much from the small quantity of air admitted, as from the vapours which insinuated themselves with it. These lined the sides of the glass, and conducted the electricity imperceptibly from one end of the tube to the other. These experiments seem to evince, that however great the vacuum could be made, the electrical coruscations would pervade it through its whole length.

Hence it appears that our atmosphere, when dry, is the agent by which we are enabled to accumulate electricity on non-electrics; as in the experiment before us, on the removal of it, the electricity passed off into the floor through a vacuum, of the greatest length we have hitherto been able to make, became visible in this vacuum, and manifested itself by its effects on the air-pump, being the non-electric substance, which terminated that vacuum; whereas, when the air is not taken away, the dissipation of the electricity is from every part of the prime conductor. We see here also, contrary to what we have found hitherto, that an originally-electric body, viz. a dry glass tube, puts on the appearance of a non-electric, by becoming itself the conductor of electricity, that is, by its keeping out the air, and suffering the electricity to pervade the vacuum.

Mr. W. was desirous of knowing, for the further illustration of his propositions, whether the Leyden experiment could be made through the vacuum. For this purpose he made the before-mentioned exhausted tube part of the circuit, so necessary to this experiment. In this experiment it is absolutely necessary that the whole quantity, or nearly so, of the accumulated electricity, should be discharged in the same instant of time. Accordingly on making the experiment, at the instant of the explosion, a mass of very bright embodied fire was seen to jump from one of the brass plates in the tube to the other; but this did not take place when one of the plates was farther distant from the other than 10 inches. When the distance was greater, the fire then began to diverge, and lose part of its force; and this force diminished in proportion to its divergency, which was nearly as the distance of the 2 plates.

The difficulty however of applying the Torricellian vacuum to these experiments has been happily got over by Lord Charles Cavendish, our worthy vice-president. This noble lord, who to a very complete knowledge of the sciences joins that of the arts, and whose zeal for the promotion of true philosophy is exceeded by none, has applied it in the following manner, and his lordship put his apparatus into Mr. W.'s hands. This apparatus consisted of a cylindrical glass tube of about $\frac{3}{10}$ of an inch in diameter, and of $7\frac{1}{2}$ feet in length, bent somewhat like a parabola in such a manner, that 30 inches of each of its extremities were nearly straight, and parallel to each other, from which an arch sprung, which was likewise of 30 inches. This tube was carefully filled with mercury; and each of its extremities being put into its basin of mercury, so much of the mercury ran out, until, as in common barometrical tubes, it was in equilibrio with the atmosphere. Each of the basins containing the mercury was of wood, and was supported by a cylindrical glass of about 4 inches in diameter, and 6 inches in length; and these glasses were fastened to the bottom of a square wooden frame, so contrived, as that to its top was suspended by silk lines the bent tube filled with mercury; so that the whole of this apparatus without inconvenience might be moved together. The Torricellian vaccuum then occupied a space of about 30 inches. In making the experiment, when the room was darkened, a wire from the prime conductor of the common electrical machine communicated with one of the basins of mercury, and any non-electric touching the other basin, while the machine was in motion, the electricity pervaded the vacuum in a continued arch of lambent flame, and as far as the eye could follow it, without the least divergency.

That the electricity was not furnished from the glasses employed in these operations, nor from the circumambient air, Mr. W. had heretofore, in his communications on this subject, endeavoured to evince. He had shown, that electricity is the effect of a very subtil and elastic fluid, occupying all bodies in contact with the terraqueous globe: and that every where, in its natural state, it is of the same degree of density; and that glass and other bodies, which we denominate electrics per se, have the power, by certain known operations, of taking this fluid from one body, and conveying it to another, in a quantity sufficient to be obvious to all our senses; and that, under certain circumstances, it was possible to render the electricity in some bodies more rare than it naturally is, and by communicating this to other bodies, to give them an additional quantity, and make their electricity more dense; and that these bodies will thus continue until their natural quantity is restored to each; that is, by those, which have lost part of theirs, acquiring what they have lost; and by those, to which more has been communicated, parting with their additional quantity. Both one and the other of these is, from the elasticity of the electric matter, attempted to be done from the nearest non-electric: and when the air is moist, this is soon accomplished, by the circumambient vapours, which here may be considered as preventing in a very great degree our attempts to insulate non-electric bodies. But these matters he had copiously treated of in his former communications on this subject.

If therefore the beforementioned principles are true, and if the electricity is not furnished by the globe in its rotation, nor by the air, it ought to be visible in the vacuum of the before-described glass tube, in its ingress to the frame of the electrifying machine, if this machine, and the man who turns its wheel, are supported by electrics per se; and if, during this operation, the electricity, as fast as furnished, is taken off by a bystander, or otherwise, from the prime conductor; as under these circumstances the vacuum is the only passage open to its progress, and from its elasticity the electricity should protrude itself through it. And from experiment this is the case; for on a piece of wire being connected with the end of the long brass rod, or with the brass cap at the upper extremity of that tube, and the other end of the wire fastened to any part of the frame of the electrifying machine, and this last put in motion, the electrical coruscations are seen to pass as before, from one of the brass plates contained in the tube to the other; and to continue, unless the air insinuates itself, as long as the machine is in motion. If, under these circumstances, the hand of a person standing on the floor be brought near the sides of the glass, the coruscations will direct themselves that way in a great variety of forms, extremely curious to behold.

This experiment therefore, in which the electricity is seen, without any preternatural force, pushing itself on through the vacuum by its own elasticity, in order to maintain the equilibrium in the machine, which had lost part of its natural quantity of electricity by the present operation. Mr. W. considers as an experimentum crucis of the truth of the doctrines here laid down; viz. not only that the electricity is furnished by those bodies hitherto called non-electrics, and not by the electrics per se;* but also, that we are able to add to, or take from, that quantity of electricity, naturally adherent to bodies.

By what denomination shall we call this extraordinary power? from its effects in these operations, shall we call it electricity? from its being a principle neither generated nor destroyed; from its being every where and always present, and in readiness to show itself in its effects though latent and unobserved, till by some process it is produced into action, and rendered visible; from its penetrating the densest and hardest bodies, and its uniting itself to them, and from its immense velocity; shall we, with Theophrastus, Boerhaave, Niewentyt, Gravesande, and other philosophers, call it elementary fire? or shall we, from its containing the substance of light and fire, and from the extreme smallness of its parts, as passing through most bodies we are acquainted with, denominate it, with Homberg and the chemists, the chemical sulphureous principle, which, according to the doctrines of these gentlemen, is universally disseminated? We need not indeed be very solicitous in regard to its denomination; certain it is, that the power we are now treating about is, besides others, possessed of the properties before-mentioned, and cannot but be of very great moment in the system of the universe.

LXII. Extracts of Father Augustin Hallerstein's Astronomical Observations made at Pekin in 1746 *and* 1747. *By Dr. Bevis.* p. 376.

These are observations of the appulses and occultations of the planets and fixed stars, made by the Jesuits at Pekin. They are not made with much accuracy, and are now of little or no use to science.

* Since the communication of this paper to the Royal Society in February 1752, viz. in the succeeding summer, the truth of this doctrine is put out of all doubt by the discovery made in France, in consequence of Mr. Franklin's hypothesis, of being able, by a proper apparatus, to collect the electricity from the atmosphere during a thunder-storm, and to apply it to the usual experiments, which demonstrates, that the matter of thunder and lightning and that of electricity are one and the same. That the electricity did not proceed from the glass, or other electrics per se, as they had been usually called, Mr. W. first discovered in the year 1746. See Phil. Trans. vol. xliv, p. 713, and explained further vol. xlv, p. 95, et seq. and though the electric matter may be taken from the atmosphere during a storm of thunder, or even when it is only charged with what are usually called thunder clouds, that is, when the atmosphere is replete with heterogeneous phlogistic matter, yet it must not be considered as coming from pure dry air, which, as I before mentioned, I conceive to contain in its natural state scarcely any of the electric matter, and is the agent, by which we are enabled to communicate electricity to other bodies.—Orig.

LXIII. Extracts of several Letters of Mordach Mackenzie, M. D. concerning the Plague at Constantinople. By Dr. Clephane, F. R. S. p. 384.

Before he transcribes his friend Dr. Mackenzie's letters relating to the late plague at Constantinople, Dr. C. premises a few particulars concerning the plague in general, as he finds them scattered here and there in Dr. Mackenzie's former letters to him on that subject.

In a letter dated March 24, 1749, Dr. M. observes that, in his time the plague, whether at Constantinople, Smyrna, or any other part of the Levant, had been mostly sporadic, seldom epidemical. That therefore the articles in our news-papers, which so often mention the plague raging violently, are almost always false. At Constantinople, and all over the east, people shun the plague, and the infected, as much as we do; and every body, physicians as well as others, who have been with the sick, or in places infected, are all obliged to perform 40 days quarantine. The Armenians and priests are the only people who attend them; and they only to give them necessaries at a distance, or to perform the last functions of the church: and this the priest is obliged to do by his religion. The European plagues are much more violent than the eastern: those being really the Thucydidian, which sweep all away; while these are only gentle corrections to put us in mind of mortality.

The Doctor, in another letter, finds fault with the method used in England to prevent infection by shipping; "for, to what purpose (says he) keep ships in Sandgate creek for weeks, and even months, without landing and serening the goods? I hope you will allow, there is little to be feared from the bodies of men, who get in good health from Smyrna to England, which voyage is seldom performed in less than 7 or 8 weeks; which I presume will be thought too long for infection to remain in the blood without producing some effect. Therefore as all the danger is from the goods or cargo, greater care ought to be taken of this and less of the men. Your nation differs much from Italy or Marseilles, where a ship may, and often does, arrive in 8 days; for which reason, though it be necessary to look after the men, as well as the goods, still however they make a great distinction. You make none."

It is observable, that from the beginning to the status or acme of the disease, they almost all die, afterwards its violence begins to abate, and about the end of the season most people recover. The symptoms of the distemper are chiefly, irregular fits of heat and cold; shiverings; violent head-ach, and retchings, for the first 3 or 4 days; great anxiety about the præcordia, &c. both before and after the eruptions, a wild staring countenance; sweats for the most part about the head and breast only, at the same time the extremities cold; a dry parched yellow furred tongue. The more violent those symptoms are, the greater the danger. Some are delirious, and raving; others to a great degree stupid and dull; both these are fatal appearances. Some die in 5 or 6 days; some outlive 20 days, and then die: some walk the streets for many days, and afterwards die. Bleeding at the nose is reckoned a salutary sign. A swelling in the throat is a common symptom: for which if you bleed, it proves almost always fatal: for it is so far from abating this symptom, that after it a greater difficulty of breathing ensues, and the patient seldom survives it above 3 or 4 hours.

The medical writers are divided as to the expediency of bleeding in the plague, some contending for it warmly, others as warmly condemning it. The Doctor distinguishes between the different stages of the distemper, and says, that as in the beginning, during the ebullition, bleeding may be of some service, so when the disease is advanced, and especially after the eruptions, it will prove fatal, as well as purging, or any other violent evacuation. A moderate diaphoresis ought always to be kept up. To the buboes, parotides, &c. they commonly apply a roasted fig with some white sugar powdered: and this they reckon the best suppurative. They do not open the tumours, but leave them to break of themselves. They give the sick cold water to drink, and order the cool regimen quite through the distemper.

Abstract of Dr. Mackenzie's first Letter concerning the late Plague at Constantinople. Dated Constantinople, July 23, 1751.

" We have at present the most violent plague, that has been at Constantinople in my time, by all reports. They are all taken the same way, with a shivering and vomiting, a violent head-ach, thirst and fever, of which they die the 3d or 4th day, rather in a stupor than a delirium; and such as have the misfortune to be near the infected person, are taken in 7 or 8 days, though there are already many instances to the contrary. The Greeks and Armenians suffer most, next to them the Jews. The Turks suffer less in proportion than other nations."

Dr. Mackenzie to Dr. Clephane, F. R. S. Dated Constantinople, Nov. 23, 1751.

" During the 20 long years I have lived in this country, here and at Smyrna, there has scarcely been a year, excepting 3, in which the plague did not threaten, more or less; and in all that interval I observed no other difference in the seasons, than that the winters might begin more early, and continue somewhat longer, and with greater rigour; though, by my thermometers, this difference never exceeded 5 or 6 degrees; which is no great difference here, where the south and north winds make a difference of from 15 to 20 degrees in 24 hours: so that I cannot see any other apparent cause of the virulency of the disease this year, besides the occasion of greater communication. In the months of February, March, April, and May last, the distemper was so strong at Cairo, as appears by letters from the English consul there, that no doors were opened for 3 months. In the mean time there arrived here in May last 4 ships laden with Cairo goods, which goods and men being landed, they spread the infection over all the city at once, after which, one conveyed it to another by contact. The only apparent cause of the virulency in this case, is 4 ships arriving from Cairo, instead of 1 or 2, at the same time; and if you please, you may add to this some little difference of the seasons, mentioned in my letter to Dr. Mead, and a greater quantity of cucumbers, melons, and fruit, than usual, on which the poorer sort of people feed.

However I do not believe the number of the dead anywise equal to common report, for the reasons following: The Turks have no bills of mortality; but they reckon, that in and about Constantinople there are consumed daily 20,000 killows of flour. Every killow is reckoned to weigh 20 oques, and every oque is equal to 400 drachms, and 160 drachms thought sufficient for a person for 24 hours, or one complete day, taking men, women, and children together. Therefore one killow makes bread enough for 50 persons per day; but the consumption of bread in the months of July, August, and September, was 3000 killows short; from which it is concluded, that $3000 \times 50 = 150,000$ must have died of the plague, without making any allowance for the great number of people, that run away to Prusa, Nicomedia, Adrianople, the islands, and such as must have died of other diseases in 3 months in a populous city of a million of souls, by the calculation of 20,000 killows per day.

Next I must observe to you, that there are two vulgar errors with regard to the plague established in this country. They say that a plague which begins early, ends soon; which is false; for, in the year 1735, the plague began at Smyrna the 15th of February pretty hot, so that all the houses in Frank-street were shut up in February, and it continued till the latter end of November. Another vulgar error is, that the heat kills the plague at Smyrna, and the cold at Constantinople; which is very true with regard to Constantinople, but very false with regard to Smyrna; for proof look back to the year 1735, when the vigour of the malady showed itself most in the months of June and July, though so very hot, that some people were said to die of the heat in going from the town to the villages near it, so that it is very certain the heat does not kill the plague at Smyrna, as is generally thought and said."

Dr. Mackenzie to Dr. Mead, F. R. S. Dated Constantinople, Oct. 29, 1750.

"This is the only summer since I have been in Turkey that I can say we have been without any plague. The air was very temperate, no heavy rains, but high winds at N. E. from which point our etesian winds blow, commonly called milhem in the Turkish language. Fruits have not been so plenty, or of such a good quality as usual; few fevers of the intermittent kind, but not so regular as usual in their symptoms; for they were seldom attended with any head-ach, the tongue not much charged, and the urine seldom made any sediment of the lateritious kind; and if they were not taken in time, a yellow jaundice came upon them the 6th or 7th day: and in the beginning of the fever, the patient seldom vomited bile as usual, but rather a pituitous matter."

Dr. Mackenzie to Dr. Mead. Dated Constantinople, Nov. 23, 1751.

"I remember to have written to you my sentiments of this distemper some years ago; and from all the observation I could make in the interval, I have no reason to change my opinion, viz. that it is brought from Cairo commonly; and that when once a house or ship is infected, it is very difficult to eradicate the animalcula, semina, effluvia, miasmata, or whatever name is proper for the reliques or remains of it, which getting once into a nidus, lodge there: Condensed by the cold during the winter, and when rarefied by a certain degree of heat, they act on bodies which have a disposition, as women and children mostly, and so spread by contact only, without communicating any malignancy to the ambient air. Otherwise very few could escape: whereas we found this last time, and on all such occasions, that whoever kept their doors shut, ran no risk, even if the plague were in the next house; and the contact was easily traced in all the accidents which happened among the Franks. The patients were this year sick at stomach, and troubled with vomiting and nausea for 3 or 4 days after they were infected, and before the eruption of the buboes, carbuncles, or tokens; and in about 4 days more after the eruptions they died, or showed good symptoms of recovery, such as, the fever, with all its symptoms, decreasing; the eruptions tending to maturation and suppuration, the nausea ceasing, and some appetite beginning."

LXIV. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries for the Year 1751, *pursuant to the Direction of Sir Hans Sloane, Bart. By J. Wilmer, M.D.* p. 396.

This is the 30th presentation of this kind: completing to the number of 1500 different plants.

LXV. Of Dr. Bianchini's Recueil d'Experiences faites à Venise sur la Medecine Electrique. By Mr. William Watson, F.R.S. p. 399.

The account of this work indeed may be now thought less necessary, as, since the Abbé Nollet's journey to Italy, and our want of success here in our attempts to do the like, every body has considered what the Italians printed on the transmission of odours through the pores of glass, and on the subject of medical electricity, as too hasty a publication. Mr. Winkler, however, from Leipsic, sent to the Society, long since these publications, some tubes and globes, which he said had transmitted odours by electrifying. What he conjectured the glasses would do, fell infinitely short of what he first gave out; but even after the most careful trials, and complying with his instructions most scrupulously, we were disappointed in our expectations.

The gentlemen concerned in conducting these experiments, published by Dr. Bianchini, divided them into 3 classes. The first class contains a series of experiments made with tubes and globes containing odoriferous or other substances, in order to observe, when these were closely stopped, whether the odorous, as well as other effects of the substances included, would pervade the glass. The second class includes experiments made with tubes and globes, which have nothing within them; but the persons electrified hold in their hands, or sometimes place under their naked feet, odoriferous, purging, or even the most poisonous substances, in order to observe, whether the persons electrified in this manner would be sensible of the effects of these substances. The third class gives a series of experiments different from the two former, in which the substances before-mentioned are mixed with the water, as in making the experiment of Leyden. From these experiments we are to discover, whether from receiving the shocks from these bottles, the person is sensible of the effects in his body of the substances contained in them. But after many numerous and accurate trials of all these, on several persons, no such effects were felt by them. There appears, through the whole course of the experiments contained in this work a great deal of care and accuracy. They were made by persons fully acquainted with the manner of employing their apparatus, and many of the experiments were several times repeated.

After what has been done here at London, at Paris, and at Wirtemberg, with the like success, these experiments cannot, to unprejudiced persons, but be conclusive, that the extraordinary accounts from Italy and Leipsic, had no foundation in fact; and that no method has yet been discovered, by which from electricity the powers of medicines could be made to insinuate themselves into the human body.

This conclusion, however, does not, nor is meant to operate, against the advantages said to be gained by electricity itself. So subtil and so elastic a fluid admitted in a large quantity into our bodies, as, from undoubted experience, it greatly heats the flesh, and quickens the pulse, may, more especially when assisted by the expectation of success in the patient, in particular cases be attended with very great advantages.

LXVI. The Case of the Operation of the Empyema, successfully performed by Mr. Joseph Warner, F.R.S. and Surgeon to Guy's Hospital. p. 407.

Thomas Hines, aged 27, was admitted into the hospital, Dec. 19, for a pain in his right side, and cough; which he had laboured under for 3 weeks. He was immediately put under the physicians care; but notwithstanding all proper methods used for his relief, his disorder increased till Jan. 13 following, when Mr. W. was consulted.

On inquiry, he found him afflicted with the following symptoms, a quick, low pulse, frequent cough, and difficulty of breathing, which last symptom was greatly increased on lying on his left side, or on sitting upright. He appeared greatly emaciated, his countenance very pallid or sallow. The right side of the thorax was somewhat enlarged; the integuments were visibly thickened, but without the least discoloration, or perceivable fluctuation. However, being persuaded from the foregoing symptoms, that there probably was an extravasated fluid underneath, he advised the operation, which was accordingly done on the spot, in the following manner:

The patient being conveniently seated, he made an incision of about 3 inches long, with a knife, between the 10th and 11th rib, counting from above, and at about 4 inches distance from the vertebræ. The direction of the incision was agreeable to the course of the ribs, and on being made nearer to the superior edge of the 11th rib, than to the inferior edge of the 10th rib, the intercostal artery by that means escaped being wounded. On dividing the intercostal muscles, very near 20 oz. of matter were discharged, after which he introduced his finger through the wound into the cavity of the thorax, but found no adhesion of the lungs. Whence he conjectured that this abscess was originally formed in the cellular membrane of the pleura, which had at length made its way into the cavity. What seemed to corroborate this conjecture, was that the violent symptoms, which happened on lying on the sound side, or on sitting upright, did not occur till within a week before his application to him. From the moment the matter was discharged, he found immediate ease, his respiration became quiet; his fever and cough gradually abated, till in about 6 weeks he became perfectly well in all respects, and was accordingly dismissed the hospital. The discharge from the wound continued in considerable quantities for the first fortnight; during which time the wound was kept properly open with tents; but when the discharge was no more than what might be expected from any superficial wound of the same size, all tents were disused, and superficial applications only made use of.

LXVII. Of the Eruption of Mount Vesuvius in Oct. 1751, *written at Naples, Jan.* 15, 1752, *N. S.* p. 409.

After the usual phenomena of smoke and flame, and bellowings, &c. there, on the 25th of October, in a place called Atrio del Cavallo, on the east side of the mountain, a fiery fluid, like melted glass in a furnace, burst out, or rather seemed to boil over, which ran down the declivity of the mountain with great velocity and force, carrying along with it large stones, gravel, calcined earth, &c. In 6 hours time it ran 4 miles, and covered vast tracts of fine land; destroyed many farm-houses, villas, and vineyards. The reason why it does so much mischief is, that it spreads itself, where the ground is plain, and covers in some places above an acre in breadth; but where there is a hollow ground, it forms a current river, making banks of its own substance, by cooling and hardening towards the edges; and when this current happened to be opposed by a rising ground, (the high banks of the cooling lava preventing its passage on either side) it formed high mountains of lava of 50 or 60 feet; till at last, by the weight and force of the red-hot river flowing incessantly from the bocca above, it burst out from under this new hill, and forming a second fiery river, proceeded down the country, destroying all where it came. It was shocking to see trees, and vines loaded with fruit, floating on this river of fire. And, to our great astonishment, though we plainly saw the fluidity and rapid current of this matter, yet was it so impenetrable, that no weighty body would sink in it; nor did a sharp heavy iron instrument, thrown at it with great force, make the least impression on it, but, remaining on it a few minutes, it became red-hot like the lava. Nor could the pious procession and liquefaction of St. Januarius's blood on the spot put a stop to the destructive inundation; for it has run for these 2 months past, and runs a little still.

LXVIII. Of an Hydrophoby. By Tho. Wilbraham, LL.D., F.R.S. p. 412.

On March 29, 1752, Isaac Cranfield, a waterman, about 30 years of age, was received into the infirmary in Westminster, with an hydrophobia on him. He had been that morning with Mr. Heathfield, one of the surgeons to that infirmary, for advice; who being informed of that remarkable symptom, asked him, if he had not been lately bitten by a dog? he answered, no. But his wife, who was with him, put him in mind, that he had received a wound from a dog about 9 months before. This he presently recollected: and said, it was a strange dog he met with at a public-house, that, as he was going to stroke him, gave him a little bite in the hand.

The same day, about one o'clock, Dr. Coxe, Dr. Watson, and Dr. W. met together to consult on his case. When he came to be examined, he repeated to them the manner of his being bitten, as just mentioned; and said further, that he no sooner found himself hurt, but he gave the dog such a blow with a poker, as laid him dead on the spot. The wound, being slight, soon healed up, and he thought no more of it; and he enjoyed good health till about 2 o'clock the Thursday morning before, when he was seized with a violent sickness and vomiting. The day following he continued very ill, and particularly felt an unusual pain, whenever he attempted to drink. Friday and Saturday that symptom grew worse; and on Sunday he could not swallow the least quantity of liquor, without the utmost misery. This was the day they saw him. He looked somewhat wild in his eyes; but in his discourse discovered no signs of madness. His pulse was extremely quick, but not weak and depressed. They examined his fauces, and found an inflammation. They desired him to give them an opportunity to see how he could bear an attempt to get down some liquid. He readily consented. He chose to sit down on the floor, then took a cup of water in his own hand, and put it to his mouth. The moment the liquor reached his throat, he suddenly sprung up on his feet, and ran about the room in the most violent agony that can be conceived. It must be observed, that he could get down small quantities of food that was solid, all the time this symptom was upon him.

He informed them he had been let blood twice the day before he came to them. They agreed to take from him 12 oz. more, and to give him 1 gr. of extract. Theb. every hour, till there appeared some signs of stupor from the medicine. They also ordered him a clyster of decoct. furfuris with nitre. The blood was found next day not differing from that of a person in health. The extract was made up in pills of 1 gr. each, which he could swallow without difficulty. Dr. W. saw him again at 8 o'clock at night, at which time he had taken 5 grs. of opium, but did not appear to be in the least affected by it, being much in the same state he had left him in at one. He had had the clyster twice, but no stool either time. He went on with the pills till he had taken 15 grs. but no effect could be perceived from them. He passed the night in great anxiety, being for the most part on his legs, and at times light-headed. A good deal of frothy saliva was discharged from his mouth. About 8 o'clock in the morning he died. A few minutes before he expired, he said, that he was sensible he was going to die; and expressed much concern for the loss which his wife and children would have of him. That day they had him opened. The lungs were found full of blood. Water in the pericardium in the usual quantity. The blood in both ventricles of the heart fluid. The œsophagus without any morbid appearance. (Vide Boerhaave Aphor. 1140.) The aspera arteria full of such frothy substance as came from his mouth. The stomach filled with liquor, notwithstanding the small quantity he had drank since Wednesday evening. No other parts were examined.

LXIX. Improvements made in the Air-Pump. By Mr. S. Smeaton, p. 415.

The chief causes of imperfection in the common pumps arise, first, from the difficulty in opening the valves at the bottom of the barrels; 2dly, from the piston's not fitting exactly, when put close down to the bottom; which leaves a lodgment for air, that is not got out of the barrel, and proves of bad effect.

In regard to the first of these causes; the valves of air-pumps are commonly made of a bit of thin bladder stretched over a hole generally much less than one tenth of an inch diameter; and to prevent the air from repassing between the bladder and the plate, on which it is spread, the valve must always be kept moist with oil or water. It is well known that at each stroke of the pump, the air is more and more rarefied, in a certain progression, which would be such, that an equal proportion of the remainder would be taken away, were it not affected by the impediments just mentioned: so that when the spring of the air in the receiver becomes so weak, as not to be able to overcome the cohesion of the bladder to the plate, occasioned by the fluid between them, the weight of the bladder, and the resistance that it makes by being stretched, the rarefaction cannot be carried further, though the pump should still continue to be worked.

It is evident, that the larger the * hole is, over which the bladder is laid, a proportionably greater force is exerted on it by the included air, in order to lift it up; but the aperture of the hole cannot be made very large, because the pressure of the incumbent air would either burst the valve, or so far force it down into the cavity, as to prevent its lying flat and close on the plate, which is absolutely necessary. To avoid these inconveniences as much as possible, instead of one hole, Mr. S. made use of 7, all of equal size and shape; one being in the centre, and the other six round it: so that the valve is supported at proper distances, by a kind of grating, made by the solid parts between these holes: and to render the points of contact, between the bladder and grating, as few as possible, the holes are made hexagonal, and the partitions filed almost to an edge. As the whole pressure of the atmosphere can never be exerted on this valve, in the construction made use of in this pump; and as the bladder is fastened in four places instead of 2, the breadth of the hexagons are made $\frac{3}{10}$ of an inch; so that the surface of each of them is more than 9 times greater than usual. But as the circumference of each hole is more than 3 times greater than common, and as the force that holds down the valve, arising from cohesion, is, in the first moment of the air's exerting its force, proportionable to the circumference of the hole; the valve over any of these holes will be raised with 3 times more ease than common. But as the raising of the valve over the centre hole is

* If we examine the force, that air rarefied 140 times can exert in a common valve through a hole of one tenth of an inch diameter, we shall find it not to exceed 6 grains at a medium.—Orig.

assisted on all sides by those placed round it; and as they all together contribute as much to raise the bladder over the centre hole, as the air immedietely acting under it; on this account the valve will be raised with double the ease, that we have before supposed, or with a 6th part of the force commonly necessary. It is not material to consider the force of the cohesion, after the first instant: for after the bladder begins to rise, it exposes a greater surface to the air underneath, which makes it move more easily. He has not brought into this account the force that keeps down the valve, that arises from the weight of the bladder, and the resistance from its being stretched; for he conceives these as small in comparison of the other.

But supposing all those difficulties to be absolutely overcome, the other defect mentioned in the common construction, would hinder the rarefaction from being carried on beyond a certain degree. For as the piston cannot be made to fit so close to the bottom of the barrel, as totally to exclude all the air; as the piston rises, this air will expand itself; but still pressing on the valve, according to its density, hinders the air within the receiver from coming out: hence, were this vacancy to equal the 150th part of the capacity of the whole barrel, no air could ever pass out of the receiver, when expanded 150 times, though the piston was constantly drawn to the top; because the air in the receiver would be in æquilibrio with that in the barrel, when in its most expanded state. This I have endeavoured to overcome, by shutting up the top of the barrel with a plate, having in the middle a collar of leathers, through which the cylindrical rod works, that carries the piston. By this means, the external is prevented from pressing on the piston; but that the air, that passes through the valve of the piston from below, may be discharged out of the barrel, there is also a valve applied to the plate at the top, that opens upwards. The consequence of this construction is, that when the piston is put down to the bottom of the cylinder, the air in the lodgment under the piston will evacuate itself so much the more, as the valve of the piston opens more easily, when pressed by the rarefied air above it, than when pressed by the whole weight of the atmosphere. Hence, as the piston may be made to fit as nearly to the top of the cylinder, as it can to the bottom, the air may be rarefied as much above the piston, as it could before have been in the receiver. It follows therefore, that the air may now be rarefied in the receiver, in duplicate proportion of what it could be on the common principle; every thing else being supposed perfect.

Another advantage of this construction is, that though the pump is composed of a single barrel,* yet the pressure of the outward air being taken off by the

* It is obvious that these improvements will equally obtain, whether the pump is constructed with a single or double barrel.—Orig.

upper plate, the piston is worked with more ease* than the common pumps with two barrels: and not only so, but when a considerable degree of rarefaction is desired, it will do it quicker; for the terms of the series expressing the quantity of air taken away at each stroke do not diminish so fast, as the series answering to the common one.

Having found the gages that have been hitherto made use of, for measuring the expansion of the air, very unfit to determine in an experiment of so much nicety; Mr. S. therefore contrived one of a different sort, which measures the expansion with certainty, to much less than the 1000th part of the whole. It consists of bulb of glass something in the shape of a pear, and sufficient to hold about a half a pound of quicksilver. It is open at one end, and at the other is a tube hermetically closed at top. By the help of a nice pair of scales, he found what proportion of weight a column of mercury, of a certain length, contained in the tube, bore to that which filled the whole vessel. By these means he was enabled to mark divisions on the tube, answering to a 1000th part of the whole capacity, which being of about one tenth of an inch each, may by estimation be easily subdivided into smaller parts. This gage, during the exhausting of the receiver is suspended in it by a slip-wire. When the pump is worked as much as shall be thought necessary, the gage is pushed down, till the open end is immerged in a cistern of quicksilver placed underneath: the air being then let in, the quicksilver will be driven into the gage, † till the air remaining in it becomes of the same density with the external; and as the air always takes the highest place, the tube being uppermost, the expansion will be determined by the number of divisions occupied by the air at the top.

The degree, to which he has been able to rarefy the air in experiment, has generally been about 1000 times, when the pump is put clean together: but the moisture that adheres to the inside of the barrel, as well as other internal parts, on letting in the air, as in the succeeding trials worked together with the oil, which soon renders it so clammy, as to obstruct the action of the pump on a fluid so subtile as the air is, when so much expanded; but in this case it seldom fails to act on the air in the receiver, till it is expanded 500 times: and this he found it to do, after being frequently used for several months without cleaning. He also generally found it to perform best the first trial at each time of using;

* Because, though the pressure of a column of air, equal to the diameter of the piston-rod, still presses on it, yet as there is only the friction of one piston, and that not loaded with the weight of the atmosphere; the friction of the leather against the side of the barrel, and that of the rack and wheel, is much less: so that, notwithstanding the addition of friction in the collar of leathers, that of the whole will be less.—Orig.

† The bulb of the gage may be emptied of its quicksilver, without taking that out of the tube; and the tube being held horizontal, the column of mercury in it will have no power to contract or expand the air at the top.—Orig.

though nothing had been done at it from the time preceding; which after a great many trials made with this view, he also attributes to the moisture of the air mixing with the oil.

Mr. S. also endeavoured to render the pneumatic apparatus more simple and commodious, by making this air-pump act as a condensing engine at pleasure, by singly turning a cock. This not only enables us to try any experiments under different circumstances of pressure, without changing the apparatus, but renders the pump a universal engine, for showing any effect that arises from an alteration in the density or spring of the air. Thus, with a little addition of apparatus, it shows the experiments of the air-fountain, wind-gun, &c.

This is done in the following manner: the air above the piston being forcibly driven out of the barrel at each stroke, and having no where to escape, but by the valve at the top; if this valve be connected with the receiver, by means of a pipe, and at the same time the valve at the bottom, instead of communicating with the receiver, be made to communicate with the external air, the pump will then perform as a condenser.

The mechanism is thus ordered. There is a cock with 3 pipes placed round it, at equal distances. The key is so pierced, that any 2 may be made to communicate, while the other is left open to the external air. One of these pipes goes to the valve at the bottom of the barrel; another goes to the valve at the top, and a third goes to the receiver. Thus, when the pipe from the receiver, and that from the bottom of the barrel, are united, the pump exhausts: but turn the cock round, till the pipe from the receiver, and that from the top of the barrel, communicate, and it then condenses. The third pipe in one case discharges the air, taken from the receiver into the barrel; and in the other lets it into the barrel, that it may be forced into the receiver.

LXX. Of Aphyllon and Dentaria Heptaphyllos of Clusius, omitted by Mr. Ray. By Mr. William Watson, F. R. S. p. 428.

Mr. Watson presented to the Society some specimens of 2 plants, then in flower, which he said were not frequently found in England. One of them was the anblatum of Cordus, or aphyllon of John Bauhin. This plant is denominated squamaria by Rivinus, and dentaria crocodylia by Tabernæmontanus. Linneus, in the Flora Suecica, calls it lathræa caule simplicissimo, corollis nutantibus, labio inferiore trifido. Mr. Ray, in his Synopsis Plantarum Angliæ, takes notice of its being found near Dorking in Surrey, but the plant now presented was collected near Harefield in Middlesex,

The other plant offered was the dentaria heptaphyllos baccifera of Caspar Bauhin, or dentaria tertia baccifera of Clusius. This plant is treated of by Linneus, in the Hortus Cliffortianus, and by Van Royen, in the Floræ Leydensis

Prodromus, under the appellation of dentaria foliis inferioribus palmatis, summis simplicibus.

This plant, which is frequently met with on the continent of the northern parts of Europe, has been but lately discovered to grow in England, and that only in one place; viz. in a wood not far from Harefield in Middlesex, where it was first discovered by Mr. Blackstone, an ingenious apothecary in Fleet-street. This is one of those few plants omitted by the late Mr. Ray in his excellent Synopsis, which are to be found natives here; and, from their great scarcity, it is not wonderful that they were unobserved by that great naturalist.

LXXI. Of a Machine for Killing Whales. By John Bond, M. D. p. 429.

Whales being of the same structure internally with quadrupeds, must come frequently to the surface of the water to breathe; and when they expel the rarefied air from their capacious lungs, through a narrow tube, which protrudes above the upper jaw, they occasion a great noise, which the fishers term the blowing of the whales. This noise alarms the fishers, who are waiting for that signal; on which they furnish a boat with necessary instruments, and row quietly towards the whale. The harpooner, as they call him, sits rowing in the head of the boat, and observes certain silent signals, which the boat-steerer gives him, to inform him that he is near enough to strike the whale. Then the harpooner takes the harpoon in both hands, and darts it into the whale; which, as soon as struck, plunges directly to the bottom, and moves with such prodigious velocity, that the rope which follows the harpoon often cuts deep grooves in the boat, and a man stands ready with an ax to cut the rope, if it does not run freely from the coil. The whale being hurt by the harpoon, stays longer than usual under water, till the blood, by the violent motion of the body, is collected about the heart, and consequently obstructed in the head; the nervous influx is interrupted, the swimming bladder relaxed, and the whale becomes languid, and rises to the top to breathe fresh air where it rests for some time, to recruit its exhausted spirits; which the fishers observing, row up and dispatch the whale with long lancets.

It appears from this account, that the greatest difficulty consists in making the rope fast to the whale, by means of the harpoon; which is barbed in the common form of a dart, and is generally 20 oz. weight, and about 2 feet long, with a small stalk of flexible iron, and a socket at the end, about which the rope is spliced with a shaft of wood put into it, so that they cannot throw it any distance with any degree of certainty; therefore are never sure of darting a whale, till they are within a yard, or directly above her; and there they are so much afraid of being dashed to pieces, that they often miss good opportunities, though they seldom meet with any so tame. They frequently see 40 whales within 30 yards of their boats, but cannot strike one, unless it be sleeping, or suckling its young ones. Hence the bad success, and necessity of giving a premium to indemnify the adventurers. Hence we also see that a machine, which would project a harpoon 30 yards with sufficient force and proper direction, must give a chance for taking 30 whales for one in the common way.

Several machines have been proposed to answer this end, but have all proved abortive. The crossbow was tried, but was too weak, and subject to break with the frost in those cold climates. Gunpowder was next applied, it is said with no better success; for, besides the difficulty of applying it to throw those heavy bodies in the form of darts, especially such as must carry a rope along with them, it frightens all the whales from the place, where it is fired, either by the light, or by the explosion, which it poduces; perhaps both ways; but probably more by the sound than the light; for in the summer time there is in those parts a continual day for several months, so that a flash would not be remarkable.

The machine which he recommends instead of those, is the ancient Balista, which is accurately described in the 13th chapter of Polybius, translated into French by Mons. Folard, who has nicely distinguished it from the catapulta, with which most of the ancient historians have confounded it, though these machines had distinct offices; for the catapulta threw vast masses of metal and stone in a parabolic curve, and the balista projected darts, some of 60 lb. weight, in a horizontal direction. The projectile power of both these machines depended on twisted ropes, which moved a lever placed in their centre. In the catapulta this lever moved vertically, and threw off globular bodies, as abovementioned; but in the balista there were 2 levers, which moved horizontally, and acted like a cross-bow. The force of this machine may be increased to any necessary degree, by multiplying the number of springs or ropes, and increasing the length of the lever, which turns the windlace, that draws back the cross cord, or in other words charges it. It has all necessary motions, and is contrived to stand on a pedestal in the head of a boat. It is so simple, that any person may learn how to use it in a short time; and when once it is successfully applied, we shall be no longer obliged to the instruction of the Dutch, who reckon it their interest to obstruct our success in every useful branch of trade.

LXXII. An Engine for raising Water by Fire; being an Improvement of Savery's Construction, to render it capable of working itself, invented by Mr. De Moura of Portugal, F. R. S. described by Mr. J. Smeaton. p. 436.

This engine consists of a receiver, a steam and an injection-cock; a suction and a forcing-pipe, each furnished with a valve; with a boiler, which, on account of its bulk and weight, is not sent with the rest; but, as it may be of the common globular shape, and having nothing particular in its construction, a description of it will not be necessary, as also the rest of these parts already mentioned being essential to every machine of this kind, a further account of them may be dispensed with. What is peculiar to this engine, is a float within the receiver, composed of a light ball of copper, which is not loose in it, but fastened to the end of an arm, made to rise and fall by the float, while the other end of the arm is fastened to an axis; and consequently, as the float moves up and down, the axis is turned round one way or the other. This axis is made conical, and passes through a conical socket; which last is soldered to the side of the receiver. On one of the ends of the axis, which projects beyond the socket, is fitted a second arm, which is also moved backward and forward by the axis, as the float rises or falls. By these means, the rising or falling of the surface of the water within the receiver, communicates a correspondent motion to the outside, in order to give proper motions to the rest of the geer, which regulates the opening and shutting of the steam and injection-cocks; and serves the same purpose as the plug-frame, &c. in Newcomen's engine. The particular construction and relation of those pieces will better appear by the figure and references, than can be done by a general description.

A B fig. 11, pl. 6, is an arm, which is fastened to a b, a conical axis, which goes through a conical socket in c, a triangular piece soldered to the receiver. This piece has this shape, to give liberty to the arm to rise and fall, that carries the float on the inside. D E is a small cistern, soldered to the receiver; which, being kept full of water, keeps the axis and socket air-tight. This cistern is constantly kept full of water, by means of a small leakage through the wooden peg c, which follows the packthread c d to the cistern. e is a small weight to counterpoise the float within. f is a slider; which being set nearer to, or farther from, the axis, will rise or fall a greater or less space, as may be required; and is fastened by the screw g. This slider is furnished with a turn-about, h i, which is also fastened by a screw and nut at the end i, and serves to adjust the length of F G G H, a chain, which gives motion, by means of the shorter chain k l, to I K L, the balance, which opens and shuts the cocks; and moves on the small axis L. G G are two pulleys, supported by two arms, that are fastened to the side of the receiver, and give the chain a proper direction in order to move the balance. M N is the steam-cock; the end N being supposed to be detached from a pipe, that gives it communication with the boiler. O is the injection-cock, whose key is turned by the arm O m. P Q is the injection-pipe, communicating between the forcing-pipe above the valve, and the top of the receiver. R S is the arm, by which the key of the steam-cock is worked. I K two rollers annexed to the balance, which, by striking on the arm R S, open and shut the steam-cock, as the balance is moved backward and forward. R n o is the steam-cock's key-tail, which is furnished with two small rollers, n, o, which open and shut the injection-cock, by acting on the arm O M in such a manner, that when the steam cock is opened, the injection is shut, and vice versa. T is a bell of advice which, moving along with the balance, continues to ring as long as the engine is at work. V is a cock, which serves to discharge the air from the receiver, and is opened by hand when necessary. W is a weight sufficient to raise the balance to a perpendicular position, when it is inclined to the right, and also to overcome the friction of the float, axis, pulleys, chain, &c.

To put the engine in motion, press down the arm A B, which will bring the balance over to the right side, and in its motion will open the steam-cock, and shut the injection; set open the cock at V, that the air may be discharged by the entrance of the steam into the receiver. This done, shut that cock, and let go the arm; the weight W will bring over the balance to the left; and in its motion shut the steam-cock, and open the injection; this presently condensing the steam into water, in a great measure leaves a vacuum in the receiver. Things remain in this situation, till the pressure of the atmosphere has caused the water to mount through the suction-pipe into the receiver, where, as its surface rises, it causes the float to ascend; and, depressing the arm A B, raises the balance till it has passed the perpendicular; and in its descent, which is done by its own gravity, the roller K lays hold of the arm R S, again opens the steam-cock, and shuts the injection. The receiver being now almost filled with water, the balance cannot return, till the surface of the water in it subsides, and suffers the float to descend. This is performed by the elasticity of the steam; which, at the same time that it fills the receiver, drives out the water through the forcing-pipe; and when the surface is descended so low, as to suffer the weight W to bring the balance beyond the perpendicular towards the left, it then falls of its own accord, and in falling the roller I lays hold of the arm R S, shuts the steam-cock, and opens the injection, as before.

When the engine is to be stopped, observe when the balance lies to the right, to turn round the arm O M of the injection-cock, so that the tail of the steam-cock may miss it in the next motion; so that at the same time that the receiver is filled with steam, and the steam-cock shut, the injection not being opened, the motion will stop for want of it.

LXXIII. Concerning the Shells of Crabs. By Dr. Parsons, F. R. S. p. 439.

Dr. P. had no doubt of the animal's casting his shell at certain seasons; he only wanted to be satisfied, that the old exuviæ were those of the soft crab; which the mutilated claw assured of, however difficult it might be to conceive the manner of the animal's quitting it. The manner of his acquiring a new limb is in nowise different from that of his obtaining a succeeding new shell; which is from a latent organization of the part ready for being indurated in due time, after the discharge of the old one; when and not before, the testaceous matter has room for its secretion through its proper emunctories. It is in every circumstance analogous to all the other animals which annually cast their integuments; and, in its soft state, resembles that of a hen's egg before the testaceous matter is secreted by the glands of the membrane; being soft and flexible: for this matter of all crustaceous animals, as well as of the eggs of fowls, is always successive to the entire formation of the membrane under it; nor are the glands capable of admitting the minima of the testaceous matter, till they have grown into a state proper for that purpose. Hence it may be concluded that the crab, the lobster, or other such animals, which had this property, are at first furnished with this membrane entire, and sufficient to be a defence against the violence of the agitated waves, and the rolling of sand, gravel, or other bodies, that might prove hurtful even before it can grow hard. This seems to be the method ordained by the Creator for the preservation of every animal, however differing in other little circumstances. The snake, adder, lizard, or any other kinds, which we see endowed with this property, have the new skin entire under the shrivelled, falling, old one; and it is, no doubt, the case with crabs, lobsters, and other crustaceous animals.

In order to throw a little more light on this matter, it may not be disagreeable to observe the manner of the induration of the surfaces of the shells of eggs. It has been supposed that these consist of a mucus indurated on the surface of the membrane: but this is not the case. The particles of the shelly matter are solid, though never so minute, and are carried with the fluids of the animal to the membrane, now ready to receive them into the ducts of its glands; and are thence thrown into such order in the cellules of the external surface, as to acquire a structure no less firm in proportion, than bricks laid on one another; and as capable of bearing any fair pressure, as a well built arch.

When they are thus hardened and complete, they may be rendered as soft and flexible, by being macerated in vinegar, as if the shelly particles had never been placed on them. And this is not because the matter is quite dissolved; for a vegetable acid is not capable of making a total dissolution of it; but the minute angles are destroyed, and the particles (which were before fixed like wedges to each other, to which they were inevitably guided in the secretion by the very structure of the receiving cellules of the membrane) are become round, by the destruction of their angles, and admit of being rolled in some measure on one another, so as in the whole to yield to the natural flexibility of the membrane.

LXXIV. Spherical Trigonometry reduced to Plane. By Francis Blake, Esq., F.R.S. p. 441.

It is observable, that the analogies of spherical trigonometry, exclusive of the terms cosine and cotangent, are applicable to plane, by only changing the expression, sine or tangent of side, into the single word side: * so that the business of plane trigonometry, like a corollary to the other, is thence to be inferred. And the reason of this is obvious; for analogies raised not only from the consideration of a triangular figure, but the curvature also, are of consequence more general; and though the latter should be held evanescent by a diminution of the surface, yet what depends on the triangle will nevertheless remain. These things may have been observed; but on revising the subject, it further occurred to Mr. B., and he takes it to be new, that from the axioms of only plane trigonometry, and almost independent of solids, and the doctrine of the sphere, the spherical cases are likewise to be solved.

Suppose, first, that the 3 sides of a spherical triangle, abd, fig. 12, pl. 6 are given, to find an angle a; which case will lay open the method, and lead to the other cases, in a way that appears the most natural. It is allowed that the tangents, ae, af, of the sides, ad, ab, including an angle, a, make a plane angle equal to it; and it is evident that the other side, db, determines the angle made by the secants ce, cf, at c the centre of the sphere; whence the distance, ef, between the tops of those secants, is given by case the fifth of oblique plane triangles, which, with the aforesaid tangents, reduces it to case the 6th of oblique plane triangles also †: and thus this 11th case of oblique triangles, so intricate hitherto, becomes perfectly easy. The 12th case is reducible to the 11th, and the rest, whether right-angled, or oblique, we are authorised to consider as reducible to right-angled triangles, whose sides are not quadrants, but either greater or less than such. Conceive therefore, now, in a right-angled spherical triangle, gkh, fig. 14, that the tangent, gm, and secant cm, of either leg, gk, is already drawn; and in the point, m, of their union, draw a perpendicular, ml, to cm, the secant, directly above the other leg, viz. a perpendicular to the plane of the secant and tangent, that it may be perpendicular to both (Eucl. 4, 11); for then will the tangent, gl, of the hypothenuse, gh, drawn from the same point, which that of the leg was, constantly terminate in the perpendicular line, that the radius and tangent may make a right angle (Eucl. 18, 3.) Where these tangents, g m, g l, and the perpendicular line, m l, together with the secants, c m, c l, will evidently form two right-angled plane triangles, g m l, c ml; and to one or other of these the spherical cases are easily transferred. Thus, if in the spherical triangle, g k h, the hypothenuse, g h, base, g k, and angle, g, at the base, be the parts given and required, when any two are given, the third may be determined by means of a plane triangle; and at a single operation. We

* See M. de la Caille's remark at the end of the spherical trigonometry prefixed to his Elements of Astronomy.—Orig.

† The angle to be found in this case must always be that formed by the two tangents.—Orig.

have, for instance, in the right-angled plane triangle, g m l, formed as above, the base g m, and hypothenuse g l, to find, by case the 5th of right-angled plane triangles, the angle included, which is the same as on the sphere. And then if the base g k, the angle g at the base, and the perpendicular k h, be the spherical parts given and required: or if the angles g and h, and the hypothenuse g h, be the parts given and required, we have only that former proportion of the hypothenuse and base, and angle at the base, in the triangles P N D, D F G, fig. 13, obtained by the complements, to transfer to the plane. But secondly, suppose the spherical proportion is of the 3 sides, any 2 being given, the 3d may be also found at a single operation, in the 2d right-angled plane triangle c m l, formed as above. We have, for instance, the hypothenuse and base, c l, c m, viz. the secant of the spherical hypothenuse and base g h, g k, to find, by the 5th of right-angled plane triangles, the angle, c, at the centre, which is the measure of k h, the side that was sought. And then again, if the hypothenuse, one leg, and the opposite angle, be the spherical parts given and required; or if the two angles and a leg be the parts given and required, we have only the former proportion of the three sides in the triangles, P N D, D F G, obtained by the complements, to transfer to the plane. Whence, the 6 proportions of right-angled spherical triangles being comprehended in this method, it is fully demonstrated, that all the cases of these triangles are so to be resolved.

The same might be deduced without the method of complements, but neither in so short nor satisfactory a way, and it shall therefore be omitted.

LXXV. Of a Manuscript Treatise presented to the Royal Society, intitled, A Treatise on Coral, and several other Productions furnished by the Sea, in order to illustrate its Natural History. By the Sieur de Peyssonnel, M. D. &c. Extracted and translated from the French, by Mr. William Watson, F. R. S. p. 445.

This curious treatise, containing upwards of 400 quarto pages in MS, was transmitted to the R. S. from Guadaloupe, where the author resided as physician botanist. It is the result of the observations of above 30 years. It is divided into 2 parts; the first relates to coral only, and is subdivided into 10 chapters; to which is subjoined a catalogue of the remedies and compositions, as well chemical as galenical, in which coral is an ingredient. The 2d part is subdivided into 8 dissertations, each of which has for its object some production of the sea; and the whole tends to evince, that as well coral, as the other marine bodies here specified, are produced by animals, viz. different kinds of urtica marina et purpura. To these the author has added a complete index, referring to every thing taken notice of in the whole work.

This work is the result of a great number of very curious observations and inquiries, and has for its object a part of natural history not hitherto well known.

The first chapter of the work contains the opinions of the ancients concerning coral, and the observations made on it since their time; among which are the opinions of Peireskius, Boyle, Piso, Boccone, Venette, the Comte de Marsigli, and those of M. de Peyssonnel himself. In the 2d is an examination, whether coral is a plant, or a congelation; in which are included 2 extracts, one from M. Tournefort's Elements of Botany, and the other from the Memoirs of the Royal Acad. of Sciences. The 3d chapter exhibits new observations, from which are discovered the urticæ marinæ and purpuræ, which form coral; where likewise are explained the formation and mechanism of this marine production. In the 4th chapter we find new chemical observations on the distillation of coral, which tend to prove that coral is the production of insects.* In the 5th are exhibited the definition, etymology, colours, and different sizes of corals, and of the insects inhabiting them. The 6th shows the places where they fish for coral, and the manner of fishing for it. In the 7th we have the manner of working upon, and of polishing coral, and the commerce with it. The 8th, 9th, and 10th chapters give the chemical preparations of coral, its virtues and uses in medicine, when variously prepared.

The subjects of the 8 dissertations of the 2d part of this work, are the several species of vermicular tubes found in the sea, the madrepores, millepores, lithophytes, corallines, sponges, the various shell-fish, which inhabit the sea without changing their place, and the formation and mechanism of these several substances.

This then is the general scope of our author; and though every part of his work deserves to be considered, Mr. W. on account of the space usually allowed to works of this nature, confined himself to such parts only, as seemed most to merit the attention of the R. S.

It had been long the received opinion, that coral was soft in the sea, and was hardened by the air on taking it out of the water; and the learned Mr. Boyle was not willing to quit this opinion. But as experiments are the only way of assuring ourselves of the truth, Boccone, for this purpose, went to sea in one of the coral-fishers vessels, and by plunging his arm into the water had an opportunity of examining the coral, as they were fishing it up, before it came into the air. He invariably found it hard, except at its extremities; where, on pressing it between the nails of the fingers, it furnished a small quantity of a milky fluid, resembling in some degree the juice of spurge or sow-thistle. Boccone observes further, that he saw several furrows under the bark of the coral, which terminate at the extremities of the branches, about which one might clearly see several small holes of the form of a star, which he imagines are destined for the produc-

* Improperly called insects, being the production of worms.

tion of branches. Venette's account of coral in his treatise of stones is much the same as Boccone's.

The Count de Marsigli, in a letter to the Abbé Bignon, in the year 1706, takes notice that, in order to give the most exact account of the production of coral, he wanted to be assured, whether the milky juice before mentioned was found there both in winter and summer, which was a matter of dispute even among the coral-fishers. For this purpose he went in winter for a few days to sea with the coral-fishers, and made several important discoveries in the nature of coral. He sent the Abbé Bignon an account of some branches of coral, which he found covered with flowers, and which was a thing unknown even to the coral-fishers themselves. These flowers were about a line and a half in length, supported by a white calyx, from which proceeded 8 rays of the same colour. These were of the same length, and of the same distance one from the other, and formed a star-like appearance. These bodies, which the Count de Marsigli imagined were flowers, M. Peyssonnel afterwards discovered to be the insects inhabiting the coral. As to the fact, whether the coral furnished a milky juice in winter as well as in summer, Count de Marsigli observed, that in December he found the milky juice between the bark of coral and its substance, in the same manner as he did in the month of June preceding.

M. de Peyssonnel was unwilling that the idea, which the ingenious discovery of the Count de Marsigli had given, in regard to the flowers of coral, should be lost; and therefore, being at Marseilles in the year 1723, he went to sea with the coral-fishers. Being well apprised of what Marsigli had observed, and the manner of his making these observations, as soon as the net, with which they bring up the coral, was near the level of the water, he plunged a glass vessel in it, into which he conveyed some branches of coral. Some hours after, he observed that there appeared a number of white points on every side of this bark. These points answered to the holes, which pierced the bark, and formed a circumscribed figure with yellow and white rays, the centre of which appeared hollow, but afterwards expanded itself, and exhibited several rays resembling the flower of the olive-tree; and these are the flowers of coral described by Marsigli. Having taken this coral out of the water, the flowers entered into the bark, and disappeared; but being again put into the water, some hours after they were perceptible again. He thought them not so large as the Count de Marsigli mentions, scarcely exceeding in diameter a large pin's head. They were soft, and their petals disappeared when they were touched in the water, forming irregular figures. Having put some of these flowers on white paper, they lost their transparency, and became red as they dried. He observed, that these flowers grew from the branches in every direction, from broken ones, as well as from those which were whole; but their number lessened towards the root; and after many observations he determines that what Marsigli took for flowers were truly insects [worms.]

Coral is equally red in the sea as out of it; and this redness is more shining when just taken out of the water, than even when it is polished. The bark of coral, by being dried, becomes somewhat pale. The extremities of its branches are soft, to the length of 5 or 6 lines; they are filled with a whitish juice tending to yellow. The coral-fishers said, that in the month of May this juice sometimes appeared on the surface of the bark; but this, notwithstanding great attention, our author could not observe. The body of coral, though hard, seems to give way a little when pressed between the fingers; and being broken at different distances, when just taken from the water, there always came from it a small quantity of milky juice through certain tubes, which appeared to be destined towards the bark.

Having inquired of the fishers in what direction the coral grew in the sea, they acquainted him, where the depth of the sea permitted them to dive, that they had found it growing sometimes perpendicularly downwards, sometimes horizontally, and sometimes upwards. Having verified these observations during the 8 days he staid with the fishermen, he adds, that he had never found any pores perceptible in the substance of the coral; that there issued forth less milk from the large branches, than from the smaller ones; and that the first was harder and less compressible.

The bark of coral covers the whole plant from the root to the extremities of the smallest branches. It will peel off; but this is only when just taken out of the water. After it has been exposed for a short time to the air, you cannot detach it from the body of the coral, without rubbing it to powder. This bark appears pierced with little holes, which answer to small cavities on the substance of the coral. When you take off a piece of this bark, you observe an infinite quantity of little tubes, which connect the bark to the plant, and a great number of little glands adhering to these tubes; but both one and the other do not distinctly appear, except when they are full of juice. It is from these tubes and glands that the milky juice of coral issues forth. Besides these, you see in various places the bark push itself outwards, where the substance of the coral is hollowed, and formed into the little cells, taken notice of by Boccone and Marsigli. In these you see little yellowish bodies, of the length of half a line, which terminate at the holes in the bark; and it is from these that the flowers appear.

Our author has found branches of coral, which, having been broken, have fallen on other branches, have fastened themselves there, and have thus continued to grow. He has found, when a piece of stone, or shells, or other hard bodies, have offered themselves between the ramifications of coral, that it has expanded itself over them, and inveloped them in its substance. He has seen pieces of coral growing upon detached pieces of rock, glass bottles, broken pots, and other substances, from which the plant could receive no nourishment. It has been said by great authority, that coral grows from the rocks perpendicularly downwards; but our author has seen some growing to a round flint, which must necessarily have vegetated upwards, like most other plants.

M. de Peyssonnel proceeds to examine, whether coral is a plant, according to the general opinion, or a petrification or congelation, according to some; and after exhibiting the various arguments delivered in support of these, he concludes that coral, as well as all other stony sea-plants, and even sponges, are the work of different insects, particular to each species of these marine bodies, which labour uniformly according to their nature, and as the Supreme Being has ordered and determined. The coral-insect, [worm.] which is here called a little urtica, purpura, or polype, and which Marsigli took for its flower, expands itself in water, and contracts itself in air, or when you touch it in water with your hand, or pour acid liquors to it. This is usual to fishes or insects of the vermicular kind.

When our author was upon the coasts of Barbary in 1725, he had the pleasure of seeing the coral-insect move its claws or legs; and having placed a vessel of sea-water with coral therein near the fire, these little insects expanded themselves. He increased the fire, and made the water boil, and by these means kept them in their expanded state out of the coral, as happens in boiling shell-animals, whether of land or sea. Repeating his observations on other branches, he clearly saw that the little holes perceptible on the bark of the coral, were the openings through which these insects went forth. These holes correspond with those little cavities or cells, which are partly in the bark, and partly on the substance of the coral; and these cavities are the niches which the insects inhabit. In the tubes, which he had perceived, are contained the organs of the animal; the glandules are the extremities of its feet, and the whole contains the liquor or milk of coral, which is the blood and juices of the animal. When he pressed this little elevation with his nails, the intestines and whole body of the insect came out mixed together, and resembled the thick juice furnished by the sebaceous glands of the skin. He saw that the animal, when it wanted to come forth from its niche, forced the sphincter at its entrance, and gave it an appearance like a star with white, yellow, or red rays. When the insect comes out of its hole without expanding itself, the feet and body of it form the white appearance, observed by Marsigli; but being come forth, and expanded, it forms what that gentleman and our author took for the petals of the flowers of coral, the calyx of this supposed flower being the body of the animal protruded from its cell. The milk before mentioned is the blood and natural juice of the insect, and is more or less abundant in proportion to its health and vigour. When these insects are dead, they corrupt, and communicate to the water the smell of putrid fish.

The substance of coral, by a chemical analysis, scarcely furnishes either oil, salt, or phlegm: live coral with its bark furnishes about a 40th part of its weight in these; but the bark of coral alone, in which are contained these animals, affords a 6th part. These principles resemble those drawn from human scull, hartshorn, and other parts of animals.

After the accounts here laid down, we are able to assign the reasons of all the particular facts we observe in coral. We see why a branch of it, broken off and detached from its stem, may flourish. It is because the coral insects, [worms.] which are contained in its cells, not having been injured, continue their operations: and drawing no nourishment from the stem of the coral, are able to increase, detach and separate. How they live and are nourished, is proposed to be explained in treating of the urtica of the madrepora, in which these animals are vastly larger, and appear very distinctly.

In each hole or star of the madrepora, on which our author lays the evident proof of his new system, the urtica, placed in the centre of each pore, causes it to increase in every direction by lifting itself farther and farther from the centre of the stone. And in coral, and in the lithophyton, the urtica, being niched in their crusts or barks, deposits a juice or liquor, which runs along the furrows perceived on the proper substance or body of coral, and, stopping by little and little, becomes fixed and hard, and is changed into stone; and this liquor, being stopped by the bark, causes the coral to increase proportionably, and in every direction. In forming coral, and other marine productions of this class, the animals labour like those of the testaceous kind, each according to his species, and their productions vary according to their several forms, magnitudes, and colours.

After what has been here laid down, none will surely consider these marine productions as mere plants; they are truly zoophytes, formed by the labour of the animals, which inhabit them, and to which they are the stay and support.

Swammerdam seems to have proceeded very far in these discoveries, as we may see by his 19th letter to Boccone. He goes further, and says, that having with a microscope examined a piece of coral, he found that each particle of it was composed of 10 or 12 angular and crystalline spherules; and having sawed across a piece of coral, and given it the highest polish, he found, with the microscope, and even without it, that coral from its centre is disposed in strata, which he conjectures are formed by the application of the above-mentioned spherules.

M. de Reaumur having been made acquainted with what M. de Peyssonnel had observed, sent him a letter in the year 1726; where he takes notice, that no one had hitherto considered coral as the work of insects. But it seemed to him difficult to establish this doctrine in the generality of marine productions, as was our author's opinion. That in whatever mode you considered coral and lithophytes, it did not appear possible that they were the constructions of the insects inhabiting them. That the only system to be adopted on these matters, was what he mentioned to our author before; viz. that the bark of these bodies only is a plant properly speaking; and that this deposits a stony matter, which forms the stalk necessary to sustain it. That then, in his opinion, all the difficulties vanish with regard to the organization of coral.

In the course of this work our author mentions, that besides the animals to which coral owes its formation, there are 3 kinds, which he describes at large, which pierce and corrode the coral while in the sea, without preventing its increase. Contrary to what has been generally received, and to what even Marsigli asserts, coral grows among the rocks, and in the caverns of the sea, open to every exposure. It had always been said, that it never grew in caverns open to the north; they must always be exposed to the south, at least to the east or west; but on the coast of Barbary, which lies open to the north, coral is not less frequently found than elsewhere. It is generally observed to grow better and more readily in shallow, than in deep water; and though they generally fish for it at the depth of 10 or 12 fathom, they sometimes get it, though but seldom, at 120.

M. de P. then gives the manner of coral-fishing, and describes 2 different machines made use of for this purpose: one for fishing up the coral where the bottom is smooth; being the same which is described by Gassendi in his life of Peyreskius. The other, which is called in the Provençal language the salabre, is constructed so as to be employed where the bottom of the sea is rocky and unequal. He takes notice of the great skill and address of the coral-fishers in the management of these machines, as well as their sagacity in finding, at considerable distances from the shore, the very places where some time before they have been successful. He observes that all the productions of the sea, of which he treats, have been considered by naturalists sometimes as stones, and sometimes as plants. Their stony substance deceived some, their tree-like appearance others; insomuch that most writers, who have seen these bodies in their cabinets, have only considered their figures. They have denominated pora that class of them which seemed pierced with holes. Of these they found some having their holes large; and these they called madrepora. There is another confusion among writers concerning these bodies: all those which had a tree-like form, whether their surfaces were smooth, without holes, or whether they were rough and unequal with them, were all styled corals. Those of any other form than that just now mentioned, were called madrepora, lithophyton, or alcyonium. It therefore appears necessary to establish some essential characters, to be able to distinguish these different bodies one from another; but before these marks of distinction are laid down, our author thinks proper to examine what these bodies are, and how they are formed. He proceeds to remark that divers productions are found in the sea of a stony nature. These bodies are always equal, and always the same in their different species: they have the same arrangement of parts, the same essential figure; and differ in nothing but their outward form, like different vegetables. They are all pierced with holes and pores, which are of the same size and figure, and are of the same disposition in each species; so that it appears evident that they are all produced from the same matter. But how they are produced, and their mechanism, has been hitherto unknown.

When treating of coral, our author has given several observations of other persons relating to it; but he finds none relating to the madrepora, and the other sea productions. But the knowledge which he had acquired on the nature of coral, conducted him to the discovery, which he made, of the animals that form the madrepora.

As this system is new, he thinks it necessary to give his observations, as they enabled him to form it. He defines the madreporas to be all those marine bodies which are of a stony substance, without either bark or crust, and which have but one apparent opening at each extremity, furnished with rays proceeding from the centre to the circumference. He then takes notice of the means by which he found the madrepora to be the habitation of animals. So early as the year 1719, when his curiosity carried him to the coral fishing on the coast of Provence; and though intent only upon coral, and neglecting to examine any other marine production, he nevertheless observed that the extremities of the madrepora were soft, and covered with a mucosity, which had a fishy smell. Thence he suspected that they contained some kind of animal; but his curiosity stopped here. Afterwards, being on the coasts of Barbary, the fishermen brought him, in a barrel of sea-water, one of those madreporas which are called in Provence, fenouille de mer, or sea-fennel. It had been put into the barrel as soon as it was taken out of the sea; and he observed, that the extremities of this madrepora were soft and tender, furnished with a transparent mucosity, like that of snails; these extremities were of a beautiful yellow colour, and were 5 or 6 lines in diameter. In this he saw an animal, resembling the cuttle-fish, polype, or sea-nettle. The body of this fish filled the centre; its head was placed in the middle of it, and was surrounded by several feet or claws: these feet filled the intervals of the partitions observed in the madrepora, and were at pleasure brought to its head, and were furnished with yellow papillæ. Its head or centre was lifted up occasionally above the surface, and often contracted and dilated itself like the pupil of the eye. He had the pleasure of seeing it move distinctly all its claws, as well as its head or centre. We can easily conceive all these motions, from what we have lately seen in the fresh-water polype, discovered by M. Trembley: and it is to be observed that the great sea polype (which is found on our own coasts, and usually called a prule) the animal of the madrepora, that of coral, and the fresh-water polype, scarcely differ except in magnitude; so that from having seen one, an idea of the rest may easily be formed. And Mr. W. mentions this with the more freedom, as on a visit with M. Trembley in Sussex at the late Duke of Richmond's, he saw the same order and economy observed in the coralline, as is mentioned by M. de Peyssonnel of the coral and madrepora. This phenomenon M. Trembley had discovered some time before; and having put some fresh collected coralline into a phial of sea-water, brought it to Goodwood; where after it had been suffered to remain at rest a few hours, by the assistance of a microscope a great number of very small white polypes, exactly in form resembling the fresh water polype, but infinitely less, were seen to protrude themselves from the inequalities of the coralline, each of which served as an habitation for a polype. When the water was still, these animals came forth, and moved their claws in search of their prey in various directions; but on the least motion of the glass, they instantly disappeared; as was the case of the coral insect described by our author.

But to return. The flesh of the animal of the madrepora is so soft, that it divides on the gentlest touch. This soft texture prevented M. de P. from detaching any one; and he observes that there are in those seas several large species of urtica, which become pappy on the least touch. He mentions one sort of above a foot in diameter, whose body is as large as a man's head, and which is of a poisonous nature. After the madrepora had been preserved 3 days, the contained animals covered its whole surface with a transparent jelly, which melted away, and fell to the bottom of the water as the animal died; and both the water and madrepora then had a putrid fishy smell. After having destroyed and consumed all the animals, the extremities of the madrepora became white.

Imperatus seems to have bordered upon this discovery, when he says, "that the extremities of the madrepora are soft, of a obscure purplish colour, containing a membranous substance; whence one might suspect, that it partakes of a sensitive and animal life."

Our author made the experiment here laid down on every species of madrepora, which he found during the 3 months he continued on the coasts of Barbary. He observed always the same appearance, allowing some little difference for the colour and size of the animals, the texture of their bodies, and that of the bodies themselves, on which they were produced.

From what has been extracted, concerning the coral and madrepora, an idea may be formed of the millepora, lythophyton, corallines, and sponges; each of which is, according to our author, the habitation of numerous animals, and formed by them. He has given, from his own observations, particular accounts of each of these productions, and divided them into genera and species with great accuracy; and though in common they are the habitations of animals, each species varying in form and bulk, and composing its cell in various forms and manners, and of different consistences, constitutes their essential character. As oysters, scallops, muscles, cockles, snails, &c. have a power given them, by the Author of Nature, of forming and enlarging their separate dwellings; to these bodies, the subjects of this treatise, the same power is given, but in large families. In the madrepora, its animal occupies the extremity; in the millepora, the substance; in corallines and sponges, the void places; in coral and lithophytes, the cortical parts. Each of these animals, according to their kind, furnish substances differing as much in consistence as in form. That of coral is extremely hard, and compact; the madrepora and millepora are of a stony, but more loose texture; the coralline is still more soft; the lithophyton, of a substance nearer horn than stone; and the sponge is soft and elastic.

We observe a great variety in the operations of nature: the crab, the cuttle-fish, and the sea-spider, are endowed with a testaceous covering; the esculent sea polype, and others of that class, have no such defence. So most of the animals hitherto noticed in this treatise, have a secure retreat; but there is a production denominated, by Imperatus, lorica-marina, which has no such convenience. It is as it were a soft madrepora. It grows at the bottom of the sea, and is a series of circular tubes, of about half an inch long, and 2 or 3 lines in diameter. Each of these, at the end most remote from the centre, is furnished with a sphincter, from which are occasionally protruded the legs or claws of the animal, like those before mentioned. The tubes themselves are likewise at pleasure lengthened and shortened. They are fastened to the rocks by a common broad surface, after the manner of coral and such like marine productions, and are of a coriaceous substance. Hither likewise may be referred the soft lithophyton, usually called the sea-mulberry, and described by our author, which, on observation, exhibits nearly the same phenomena as the preceding.

As to our author's opinion concerning the propagation of these animals: he supposes that they spawn as oysters do; and that their spawn is inveloped in a viscous substance, like that of testaceous and other fish; and that by this viscosity it is fastened indifferently to whatever solid body lies in its way, whether it be a rock, glass, broken pots, flint-stones, &c. This viscous matter, coming to stagnate, is changed, according to its nature, into a solid and forms a lamina or stratum, such as is observed at the base of these productions, and serves as it were for their first principle. The egg, inveloped in this viscous substance, is hatched in its proper time, and furnishes the animal, which resembles the sea-polype and other soft fish. These animals have all the necessary organs, and among others a particular gut, which, in the cuttle-fish, is filled with a black liquor, the use of which, according to the vulgar opinion, is that of being poured out at pleasure, to prevent the animal being taken when pursued: but this liquor, according to our author, serves the animals, the subjects of this treatise, with a matter capable of becoming hard; and furnishes the increase of the body or shell of the animal, which, like other shells, remains always of the same form, and is of a size proportionable to the animal. In the madrepora it lifts itself up under the animal, which always lies upon it; but in the millepora it increases from the centre as the animal grows larger; and thus these marine productions grow in just proportions.

These animals are nourished without changing their place, like American oysters, which fasten themselves to the roots of the mangles; or like what has been heretofore called concha anatifera, which fastens itself to old planks. Nature has furnished these polypes with claws, which they occasionally protrude from their cells, and seize their prey, as it passes by them; and thus they are nourished and increase, according to their particular mechanism and construction.

There are some species of the polype of the madrepora, which are produced singly, others in clusters. The first of these kinds may arise from the parent animal furnishing only one egg at a time: other species deposit a number of these eggs at the same time; which, coming to life altogether, are joined in such a manner, that they seem to constitute one and the same body.

The millepores grow one upon another; their little animals produce their spawn, which attaching itself either to the extremity of the body already formed, or underneath it, gives a different form to this production. Hence the various shapes of the millepora, which is composed of an infinite number of the cells of these little insects, which altogether exhibit different figures, though every particular cellule has its essential form, and the same dimensions, according to its own species.

On the whole, we see, that M. de Peyssonnel, if his system is admitted, has made a great alteration in this part of natural history. Naturalists had been divided, whether coral, and the harder productions of the sea, should be considered as plants or stones. Those who considered them as stones, among whom was Dr. Woodward, imagined themselves justified in this opinion from their excessive hardness, and from their specific gravity; and they were confirmed in this by observing, that if these bodies were calcined, they were converted into lime. Guisonæus, in his letter to Boccone, says positively that coral is not a plant, but a real mineral, composed of much salt and a small quantity of earth: he supposes its form given it by a precipitation, something like that of the arbor Dianæ of the chemists.

Dioscorides, Pliny, Cæsalpinus, Boccone, Ray, Tournefort, and Geoffroy, thought coral to be a plant, from its root being fixed to rocks or stones, as those of trees are to the earth; and from its sending forth a trunk, which ramified into branches. This opinion was seemingly strengthened by Boccone's observation of the milky juice at the tops and in the cells of coral, and most of all by the Count de Marsigli's discovering, in the year 1706, what he conjectured were the flowers of coral. Both these opinions, countenanced by long time, and great authority, M. de Peyssonnel has endeavoured to overturn; and to show that these productions were neither stones nor vegetables, but animals; and that, like oysters, and other shell-fish, nature has empowered them to form themselves a stony dwelling for their protection and support, each according to its kind.

Some account of M. de Peyssonnel's discoveries was transmitted by him to the Royal Academy of Sciences at Paris in the year 1727; but they were not much attended to, till Mr. Trembley's discovery of the fresh-water polype. This added much to their weight, and occasioned M. de Jussieu, in the year 1741, to visit the sea-coasts of Normandy, in order to satisfy himself of the nature of these marine productions, and his observations confirmed those of M. de Peyssonnel. The sentiments of that great naturalist M. de Reaumur, on this subject, may be seen at large in the preface to the 6th volume of his history of insects.

LXXVI. Concerning Inoculation, in a Letter from Mr. Rich. Brooke, Surgeon, to James Parsons, M. D. Secretary to the R. S. for Foreign Correspondence. p. 470.

In the year 1747, Mr. B. inoculated a young gentleman in Maryland, then about 20 years of age. He made a slight incision, about an inch in length, on the belly of the biceps muscle. In that he laid the lint impregnated with variolous matter, covered with a digestive pledget; then bound them on with a roller. When he went afterwards to look at the arm, the roller being too slack, he found the pledget and lint were moved to the opposite side from the wound; the incision itself was but a little discoloured, but the part on which the lint lay, after its removal, was inflamed, and full of red pimples. He was afraid that the gentleman would not be affected with the disorder; but he had the fever, eruptions, &c. at the usual times.

As he had but about 30 pustules in all, he went through the different stages of the disorder without the least threatening symptom. This induced him to try to communicate the disorder without making any incision, only applying the infected lint to the arm, and confining it with an adhesive plaster. The few patients on whom he tried this method were children, and always with success. The absorbent vessels, he believed in young subjects especially, would always take in a sufficient quantity of the matter to contaminate the whole mass of the circulating fluids, and though the density of the pores, or scaly inspissations of the materia perspirabilis, in adults, might in some measure prevent the disorder from being communicated by contact; yet friction would easily remove that obstacle; for by this means we might make the cuticle as thin as we please, and the warmth induced by friction would dilate the mouths of the absorbent vessels, and draw a moderate flux of juices to the part, so that they might take in a sufficient quantity of variolous matter to bring on the disorder.*

LXXVII. A Sequel of the Case of Horace Walpole, Esq. relating to the Stone, since his first Account in April, 1750.† p. 472.

After having found himself for 2 years together perfectly well, and free from all symptoms of his former disorder, having taken for some time no more than one half of the quantity of soap and lime-water that he had before used; in November 1750, Mr. W. came out of the country in his coach in the usual travelling pace, without the least inconvenience. But having ventured sometimes to go in a coach, after he came to town, on the stones, he began at times to feel the symptoms of the same disorder, which on any motion, besides that of going in a chair, even in walking to any degree, increased on him; and driving only in his chariot through the two parks to Kensington, without going on the stones, he found himself greatly affected, by making frequently and involuntarily water, and sometimes bloody, accompanied with sudden stops, and severe pains. However, taking the precaution of going by water as far as the Old Swan, and being carried from thence in a chair as far as Whitechapel, he ventured in a chariot, fitted up with the best French springs, to go into the country with Mrs. Walpole about midsummer last; but before he had got half way to Epping, though the horses went but a gentle pace, he felt as great uneasiness, attended with the same severe symptoms, as he had ever done; which frequently returned, and continued during the whole journey for 4 days together, with little or no abatement, except while in bed; whereas formerly, after he had lain some time, he was perfectly easy the whole night. In alighting from the coach, on his arrival at his house in the country, he had indeed a cruel fit; but after he had rested one night, and kept himself as quiet as possible for several days, he found himself perfectly well again; and as he never went in a coach, and did not walk much, during his whole stay in the country last year for about 5 months together, he never felt the least symptom of uneasiness.

A few days before leaving the country in November last, he took a turn or two round his park in the chariot, free from pain; which encouraged him to undertake a journey to town again in the chariot, by short stages, and gentle driving: and it was performed in 5 days to Whitechapel, without his being sensible of the least inconvenience any part of the way; neither had he felt any since

* After the above account was communicated to the Royal Society by Mr. Brooke, the experiment was tried on 4 children by Dr. Conyers at the Foundling Hospital, but was followed neither by the variolous fever nor eruption in any one of the instances.—Orig.

† See p. 135 of this vol. of these Abridgments.

his arrival in town; and he continued well, taking daily, as he had constantly done from the time he went last into the country, the full quantity of soap and lime-water, as formerly he took.

LXXVIII. Part of a Letter from Mr. John Parker, an English Painter at Rome, to his Father at London, concerning the late Eruption of Mount Vesuvius. Dated Rome, Dec. 20, 1751. p. 474.

The eruption lasted about 25 days in all, and broke out of the side of the mountain, preceded by an earthquake, felt all over Naples at the time of the eruption. The mountain in the middle of the crater or cup, which formerly threw out the stones, sunk down, with about a third of the bottom of the said cup. The breadth of the matter it threw out is in some places half a mile over, in the least part near 60 feet; and it has filled a valley about 60 feet deep, and raised a mountain in the same place, of matter and ashes, about 50 feet high; and its whole length from the mouth to where it stopped, is about 5 miles; but it did not arrive at the sea by near 5 miles. The matter, or lava, seems to be composed of iron, antimony, sulphur, and salts, and is not always of the same colour, taste, &c. in every place. The thing I can compare it to most, is the large cinders thrown out of your great iron works, but covered over in many places with the above salts and sulphur. While the lava ran red-hot, a man threw a mass of the cool lava from a height upon it, which, far from sinking into it, rebounded like a ball. Its motion was as slow as the common walk of a man. It broke out in 5 different places. Mr. P. walked on it for about a mile while near 3 feet of the top were cooled; but for many feet underneath as red to the sight as the furnace of a glass-house. It covered and burnt up trees, houses, &c. in short all it found in its way.

LXXIX. The Case of a Piece of Bone, with a Stone in the Bladder, successfully extracted, by Mr. Joseph Warner, F.R.S. and Surgeon to Guy's Hospital. p. 475.

Eliz. England, aged 48, in all other respects a healthy woman, had been afflicted with the symptoms of the stone in the bladder for about 2 years. After the usual preparation Mr. W. proceeded to the operation in the following unusual manner: He cut the urethra obliquely upwards on the right side, to about half its length, by introducing a small knife into the groove of the staff, and found very little force requisite to the introduction of the necessary instruments into the bladder, and in the extraction of the stone, &c. On laying hold of the stone it broke; so that only a part of it, about the size of a pigeon's egg, was extracted on the first introduction of the forceps; but at the second time, he extracted a ragged piece of bone, weighing 16 gr. Before it was cleansed, its cavities appeared filled and covered with a mixture of hairy and stony particles; whence he conjectures that it probably was the nucleus of the stone. Nothing remarkable occurred during the cure, but that the patient, ever after the second day from the operation, was capable of retaining her urine, and soon perfectly recovered.

LXXX. Of a Water-Spout, raised off the Land, in Deeping-Fen, Lincolnshire. By the Rev. Mr. Benj. Ray, of Cowbit near Spalding. p. 477.

May the 5th, 1752, a phenomenon appeared about 7 in the evening, in Deeping-Fen, which, from its effects, seemed to be a water-spout, broken from the clouds. A watery substance, as it seemed, was seen moving on the surface of the earth and water, in Deeping-Fen. It passed along with such violence and rapidity, that it carried every thing before it: such as grass, straw, and stubble; and in going over the country bank, it raised the dust to a great height; and when it arrived in the wash, in the midst of the water, and just over against where Mr. R. lived, it stood still for some minutes. This watery substance spouted out water from its own surface, to a considerable height, and with a terrible noise.

On its second route, it proceeded in a side line into the river, breaking in its passage a fishing-net, and there moved along, till it came to the church, where it again stood a little while, and then made its next passage through the space between the church and the parsonage house, towards Weston hills and Moulton chapel. In its way to these places, it tore up a field of turnips, broke a gate off the hinges, and another into pieces. Those who saw it evaporate, affirm it ascended into the clouds in a long spearing vapour, and at last ended in a fiery stream. There was a mist, like smoke, frequently round it. Three more were seen at the same time in different places.

LXXXI. Of Two Methods, by which the Irregularity of the Motion of a Clock, arising from the Influence of Heat and Cold on the Rod of the Pendulum, may be prevented. By John Ellicott, F. R. S. p. 479.

The first of these methods consists in a particular construction of the pendulum itself, which occurred to him several years before. About the year 1732, an experiment, which Mr. Ellicott made to satisfy some gentlemen, that the rod of a pendulum was liable to be considerably influenced by moderate degrees of heats and cold, led him to consider, that as metals differ from each other in their density, it was highly probable they might also differ from each other in their expansion; and that this difference of the expansions of two metals might be so applied, as in a great measure to remove those irregularities in the motion of a clock, which arise from the effect of heat and cold on the length of a pendulum. With this view, not long afterwards he contrived the pendulum represented by fig. 1, pl. 7. In which ab represents a bar of brass, made quite fast at the upper part by pins, and held contiguous, at several equal distances, by the screws 1, 2, 3, &c. to the rod of the pendulum, which is a bar of iron; and so far as the brass bar reaches is filed of the same size and shape, and consequently does not appear in the figure; but a little below the end of the brass bar, the iron is left broader, as at dd, for the conveniency of fixing the work to it, and is made of a sufficient length to pass quite through the ball of the pendulum to c. The holes 1, 2, &c. in the brass, through which the shanks of the screws pass into the iron rod of the pendulum, are filed as in the drawing, of a length sufficient to suffer the brass to contract and dilate freely by heat and cold under the heads of the screws; eeee represents the ball of the pendulum; f, f, two strong pieces of steel, or levers, whose inner centres, or pivots, turn in two holes drilled in the broad part of the pendulum rod, and their outer ones in a strong bridge, or cock, screwed on the same part of the rod, but omitted in the draft; because, when put on, it covers this mechanism; g, g, are two screws entering at the edge, and reaching into the cavity near the centre of the ball. The ends of these screws next the centre are turned into the form represented in the drawing, which, pressing with the weight of the ball against the longer arms of the levers, cause the shorter arms to press against the end of the brass bar at b. Things being in this situation, let us suppose that the rod of the pendulum, and the brass annexed to it, grow longer by heat; and that the brass lengthens more than the iron of the same length; then the brass, by its excess of dilatation, will press the short ends of the levers downwards at b, and at the same time necessarily lift up the ball, which rests on the long ends of the same levers at f, f, to any proportion necessary; and provided the ends of the screws press on the levers at a proper distance from the centres, the ball will be always kept at the point of suspension, notwithstanding any alteration the rod of the pendulum may be liable to from heat or cold. What this distance ought to be, may very nearly be determined, if the difference of the expansion between the brass and iron bars be known; for the proportion the shorter arms of the levers ought to bear to the longer ones, will always be as the excess of the expansion of the brass is to the whole expansion of the iron, as may be thus easily demonstrated.

Let the line ab, fig. 2, drawn perpendicular to the line ef, represent a bar of iron; the line cd a bar of brass; the pricked line bg the expansion of the iron, and dh the expansion of the brass bar, by the same degree of heat; let the line gi be drawn parallel to the line ef, then will ih represent the difference of the expansion of the two metals; through the points h, g, draw a right line cutting the line ef, as in k; this line may be supposed to represent one of the levers turning on its centre at g, h the point where the brass bar acts on the shorter end of the lever, and k the point where the screw acts on the longer end of the lever, which being the place where it intersects the line ef, it is evident the ball of the pendulum will be as much raised by the lever, as it would have been depressed by the expansion of the iron; but the triangle ihg is similar to the triangle bgk; and therefore as ih, the excess of the expansion of the brass, is to bg, the whole expansion of the iron, so will hg, the shorter arm of the lever, be to gk, the longer arm of the lever. Q. e. d.

At Fig. 1 is placed a strong double spring, whose ends pressing against the under edge of the ball, hinder it from bending the brass bar by its forcible action at the point b, which, when the ball is of a considerable weight, it might otherwise be very liable to do.

The description here given is exactly agreeable to the original contrivance; and the only alteration he afterwards made in it, consists in placing the screws g, g, within the ball of the pendulum, as represented in fig. 4. But as the success of this contrivance depended entirely on the supposition that metals were expanded differently by the same degree of heat, before putting it in execution, he inquired what experiments had already been made on this subject: when Mr. John Eames put into his hands Mr. Graham's account of his quicksilver pendulum, as it is now commonly called, published in the Philos. Trans. N° 392, which account was introduced by the following paragraph:

" Whereas several, who have been curious in measuring of time, have taken notice, that the vibrations of a pendulum are slower in summer than in winter; and have very justly supposed this alteration has proceeded from a change of length in the pendulum itself, by the influences of heat and cold on it, in the different seasons of the year; with a view therefore of correcting in some degree this defect of the pendulum, I made several trials, about the year 1715, to discover whether there was any considerable difference of expansion between brass, steel, iron, copper, silver, &c. when exposed to the same degrees of heat, as nearly as I could determine; conceiving it would not be very difficult, by making use of two sorts of metals differing considerably in their degrees of expansion and contraction, to remedy, in great measure, the irregularities, to which common pendulums are subject. But though it is easily discoverable, that all these metals suffer a sensible alteration of their dimension by heat and cold; yet I found their differences in quantity, from each other, were so small, as gave me no hopes of succeeding this way, and made me leave off prosecuting this affair any farther at that time."

The reading this paragraph proved at that time sufficient to make him lay aside all thoughts of succeeding in a contrivance founded on principles, which a gentleman of so great abilities, and known accuracy in making experiments, had after trial judged to be insufficient. And it was not till about the latter end of the year 1734, that he again resumed them on the following occasion. A gentleman desirous to make some experiments concerning the expansion of metals, employed him to make an instrument like one invented by Mr. Muschenbroek for that purpose, which he calls a pyrometer. On looking over Mr. Muschenbroek's experiments, he not only found the difference between the expansion of some of the metals much greater than he expected, but, as he thought (if they were to be depended on) sufficient to answer his former purpose. This led him to consider the structure of the instrument which Mr. Muschenbroek made use of in his trials, and on examination he thought it liable to some objections, which would probably make the result of experiments with it very uncertain. He therefore endeavoured to contrive one of a different construction, that might be more to be depended on. Such an instrument he some time afterwards completed: and though it was not in every respect so accurate as he could wish, he is fully persuaded that such experiments as are carefully made with it, may be depended on, as very near the truth. Having made a great variety of experiments with this instrument on bars of different metals, as nearly of the same dimensions as possible, he found, on a medium, their several expansions by the same degree of heat to be as follows:

Gold	Silver	Brass	Copper	Iron	Steel	Lead
73	103	95	89	60	56	149.

Thus finding so great a difference between the expansion of brass and iron, he immediately determined to make a pendulum after the manner above described, composed of those two metals, and also ordered a clock to be made, with the utmost care and exactness, with which he intended to make the experiments. These were both finished in the beginning of the year 1738; and having no reason to doubt of success, he showed the pendulum to the late Mr. Machin, and gave him a drawing and description of it, in order to its being communicated to the Royal Society; but objections were made to it, of which the only one that appeared to have any weight was, that it had been found by experiment, that two bars of different metals, screwed together so as to be in contact with each other, would not expand regularly and smoothly, but by jerks. In order to examine into the force of this objection, he directed two bars of equal dimensions to be made, one of brass, the other of iron, of about 2 feet in length, fastened together after the same manner as the two rods of the pendulum, which he intended to place so, that, by acting very near the centre of an index of a considerable length, even the smallest alteration in the bars would be made sensible, and by the motion of the index, he should be able to form a judgment, whether the rods moved regularly and freely, or not; but before this was put into execution, he contrived, by fastening the two bars to the back plate of a clock, not only to make them answer the end above proposed, but at the same time to lengthen or shorten a pendulum of a common construction, in such a manner, as sufficiently to correct the irregularities arising from the influence of cold or heat upon it. The manner of applying them is represented in fig. 3.

In which, aaaa represent the back plate of the clock, bbb a triangular piece of brass, screwed by two screws, through the slits cc, to the plate, yet so that it may be drawn backward or forward by means of the screw at d; ef is a brass bar, about 2 feet in length, made fast at the bottom, by a screw and two pins at f, to an iron one of equal dimensions, to which it is likewise screwed by the screws 1, 2, 3, &c. after the same manner as the rod of the pendulum already described. The iron bar is fastened at the upper end of the triangular piece of brass, nearly under that part of the brass bar marked e; gh is a strong brass or iron lever, moveable on a centre at g, and is supported by the upper end of the brass bar; ii is the cock, on which, in a common clock, the pendulum is hung; kk, part of the rod of the pendulum, whose spring passing through a fine slit in the cock ii, is fastened to a stud rivetted into the lever at l. The slit in the cock must be made so close, as to prevent the spring from having any lateral motion in it.

From this description it is evident, that if the brass bar expand more than the iron one, it will raise up the lever, and consequently the pendulum, which is fastened to it; and as the length of the pendulum is only from the centre of oscillation to the under part of the slit, through which the spring passes, the pendulum will be thereby shortened; and by making the point of the brass bar to act on a proper part of the lever (to which it is capable of being adjusted by means of the screw d) the pendulum may be shortened to whatever degree shall be necessary.

To prevent the pendulum from bending the bars, which it would be liable to do, if the ball of the pendulum was of any considerable weight, the end of the lever, farthest from its centre of motion, is hooked to the end of a chain, which is wound about and fastened to a small pulley at m. On the same arbor, to which this pulley is fixed, is fastened another pulley, of a much larger diameter, to which is hung, by a silk line, the weight or counterpoise n. By means of this counterpoise, any part of the weight of the pendulum may be taken off from pressing against the brass bar. And if, on the end of the arbor to which the pulleys are fixed, an index be placed, so as to point to a graduated circle, the least motion of the lever will not only be easily perceived, but also whether that motion is uniform and regular, or not. And on having, some time after, made a clock with this contrivance added to it, he found the index not only to move very sensibly, but very regularly, and never, that he could perceive, by jerks. And he doubts not, but, when the point of bearing of the brass bar on the lever is once well adjusted, it will be found to lengthen or shorten the pendulum to as great a degree of exactness, as any other method whatever.

The method he took for adjusting the longer arms of the levers of the pendulum to the shorter ones, is represented in fig. 4. To a strong post, fixed to the wall, is fastened a small shelf, supported by two brackets a, b. In the middle of this shelf is fastened a wire, by the screw e; to the end of which the pendulum is to be hung. Below this shelf, at the distance of about 40 inches, is placed the index cd, turning freely on a centre; the length of the index is 50 inches. At the distance of half an inch, on a part of the index produced beyond the centre, is placed a steel pin; and in the back of the pendulum, as near the centre of oscillation as may be, is drilled a hole to receive this pin; when the pendulum is hung on the wire against the post, and the wire is screwed higher or lower by the screw e, till the pin resting against the upper part of the hole (which is filed into a proper shape for that purpose) keeps the index nearly in a horizontal position. Below the bottom of the pendulum is placed a second index fg, exactly like the former, except that it is kept in a horizontal position by the screw k, bearing against the end of the iron rod. When the experiment is to be made, the pendulum is first put into a box, and gradually heated by a large fire, to a considerable degree, being often turned, that every part may be equally exposed to the fire. And having continued shut up in the box for some time after it is removed from the fire, that the two rods may be heated as uniformly to the same degree as possible, the pendulum is hung on the wire, and the two indexes made to stand nearly in a horizontal position. The two graduated plates h, i, are then slid on a wire, till the divisions in each marked o are pointed to by the indexes. As the pendulum cools, the lower index will be seen gradually to descend; but if the ends of the two screws, in the ball of the pendulum, act on proper parts of the levers, the upper index will continue in the same place. If the ends of the screws be either too far off, or too near the centres of the levers, the index will either rise or descend; and by comparing the number of divisions it has varied, with those which the lower index has varied, a near estimate may be made, how much the screws require to be altered; and, in a very few trials, they may easily be adjusted to a very great exactness. In order to make an actual trial how far this contrivance of the pendulum will answer the end proposed, it is necessary, that the clock, to which the pendulum is fitted, be made with great exactness, and entirely to be depended on; for otherwise the experiments will be very uncertain, as he found in the clock he first made use of. In order to render this clock as perfect as possible, he made it in several respects different from the common ones, in hopes of removing some imperfections he apprehended they were liable to. But as in this attempt he fell into an error, which it was a considerable time before he discovered, his making the trial was thereby greatly retarded. And in order to prevent others from falling into the like mistake, he gives the following short account of it.

In a common clock, the pendulum is usually hung by a spring to a cock on the back plate of the clock, while the wheel and pallets, by which the pendulum is kept in motion, are placed in the middle of the frame; and the pendulum is moved by a piece of steel, called the crutch, riveted to one end of the arbor, to which the pallets are fastened. This disposition of the pieces he apprehended liable to some considerable objections: to remedy which, he contrived to fix the pallets to the upper part of the pendulum itself, above the centre of motion; and, in order to make the pendulum vibrate as freely as possible, it was made to turn on two steel points, and was hung in the middle of the frame, exactly under the swing-wheel, and so as to vibrate in the same plane with it. By this means he was in hopes, that it would have moved with much greater freedom and regularity, than when hung after the common method; and on trial it was found to move with so great freedom, that a pendulum of above 20 pounds weight, when hung in its place without the clockwork, and made to vibrate through an arch of 2 degrees, was found to make above 1200 vibrations, before it had lost half a degree, and was observed to have a sensible motion above 20 hours afterwards; and the clock, when first put together, was kept going, for several days, by a weight of only 11 ounces, hung to the end of a single line. But it was not long before he discovered, that this great freedom made it liable to be considerably affected by the least motion. A remarkable instance of this he communicated to this Society, which was published in the Philos. Trans. N° 453. But the greatest objection to this method was, the points being subject to wear; and he found that the least alteration in them would occasion the clock to vary much more than he could have imagined. To remedy this inconvenience, he made the pendulum to move upon edges, like those on which the beam of a pair of scales turns; but he found these likewise liable to wear, though not in so short a time as the points; so that, after much time spent in making several experiments, in order to remedy this inconvenience, he found himself obliged to lay this method wholly aside, and to hang the pendulum on a spring, as usual.

This alteration being made, he found that the clock went very regular; and, after a sufficient trial, he was fully satisfied the pendulum would answer his expectations. But, fearing lest he might be thought prejudiced in favour of his own invention, he engaged the Rev. Mr. Professor Bliss to make trial of it; and accordingly, in the beginning of the year 1750, he sent to him, at Oxford, a clock for that purpose; and in January last he received from him a letter, giving his opinion of it, of which the following (so far as relates to the clock) is an exact copy.

"SIR,

"I have now had thorough trial of the clock; and am perfectly satisfied that your pendulum takes off the effect of heat and cold, as well as either the gridiron-pendulum (as it is commonly called) or the quicksilver pendulum; and this upon sufficient trial for near 2 years. It has this advantage of both the fore-mentioned ones, that it may, by lengthening or shortening the levers, be easily adjusted to the exact proportion of the difference of the iron and brass, which neither of those kinds is capable of, without very great trouble and difficulty. I was indeed prejudiced against the method of doing it by levers, as I had heard the late Mr. Graham say, that he had tried levers in different ways, that he found they did not work regularly and freely, but by jerks. However, in your method I am satisfied, by the fullest experience, that they succeed as well as either of the other sorts, or perhaps any other kind that may be invented hereafter."

Before concluding, he observes that, in the year 1748, he made a model of a contrivance to be added to a pocket-watch, founded on the same principles, and intended to answer the like purpose, as the pendulum above described. And, at a meeting of a council of the Society, on Feb. 15th last, he produced a watch which he had made for a gentleman, with this contrivance added to it, and also the model, by which was shown what effect a small degree of heat would have upon it.

LXXXII. Of a New Tackle or Combination of Pulleys. By Mr. John Smeaton, p. 494.

The axis in peritrochio, and the compound pulley, are the only mechanic powers which can with convenience be applied for moving large weights, when the height to which they are intended to be raised is considerable. The excellence of the former is, their working with little friction; that of the latter, in their being easy to be moved from place to place, and applied extempore, as occasion requires.

The present method of arranging pulleys in their blocks may be reduced to 2. The first consists in placing them one by the side of another on the same pin; the other in placing them directly under one another, on separate pins. But in each of these methods an inconvenience arises, if more than 3 pulleys are framed in one block. For, according to the first method, as the last line, by which the draught is made (or, as it is commonly called, the fall of the tackle) must necessarily be on the outside pulley or shieve; the difference of their friction will give it so great a tendency to pull the block away, that as much will be lost by the rubbing of the shieves against the block, on account of its obliquity, as will be got by increasing the number of lines.

The 2d method is free from this objection; but, as the length of the two blocks, taken together, must be equal to the sum of the diameters of the 6 pulleys, besides the spaces between for the ropes, and the necessary appendages of the framing, when more than 3 pulleys are in each block, they run out into such an inconvenient length, as to deduct very considerably from the height to which the weight might otherwise have been raised: so that, on those accounts, no very great purchase can be made by the common tackles of pulleys alone.

In order therefore to increase its power, sometimes a 2d tackle is fixed on the fall of the first; but here it is obvious, that whatever be the power of the 2d tackle, the height to which the weight might otherwise have been raised by the first, will be less, in the same proportion, as the purchase is increased by the 2d.

Again, very frequently the fall of the first tackle is applied to an axis in peritrochio, which increases the purchase very commodiously without the inconveniences last-mentioned; but then the machine is rendered cumbersome, and consequently less fit for a moveable apparatus.

All those impediments Mr. S. has avoided, by combining the two methods, above described, in one. The pulleys are here placed in each block in 2 tiers; several being on the same pin as in the first method, and every one having another under it, as in the 2d; as also that, when the tackle is in use, the 2 tiers that are the most remote from each other, are so much larger in diameter than those that are nearest, as to allow the lines of the former to go over the lines of the latter without rubbing.

From this construction arises a new method of reeving the line on the shieves: for here, whatever be the number of shieves, the fall of the tackle will always be on the middle shieve, or on that next the middle, according as the number of pulleys on each pin is odd or even.

To do this, the line is fixed to some convenient part of the upper block, and brought round the middle shieve of the larger tier of the under block, from thence round one of the same sort next to the centre one of the upper block; and so on till the line comes to the outside shieve, where the last line of the larger tier falls on the first shieve of the smaller, and being reeved round those, till it comes at the opposite side, the line from the last shieve of the smaller tier again rises to the first of the larger, whence it is conducted round till it ends on the middle shieve of the upper block on the larger tier; as will appear more plain, by inspection of figure 5, pl. 7.

In this method all the lines are clear of one another, and the blocks are kept parallel. The model which he showed the Society, and from which he made the draught, is a composition of 20 shieves, 5 on each pin. With this model, which may easily be carried in the pocket, he had raised 600 weight. But with a tackle of this sort, properly executed in large, one man will easily raise a ton, and a greater number in proportion. He had tried several numbers of shieves as far as 36; but 20 seemed to be the largest number that will answer well in practice. A very commodious tackle of 12 might be executed in wood, in the same manner that common blocks are made.

In constructing a tackle of 20 for 3 tons, the larger tier of shieves should not be less than 8 inches, the running line need not be thicker than half an inch diameter, and the iron pins need not be so thick.

LXXXIII. On some Vegetable Balls. By Wm. Dixon, Esq. F. R. S. of Loversall near Doncaster in Yorkshire, with Remarks on them by Mr. William Watson. p. 498.

These balls seem to be plants of a very particular kind. They were taken up in a fresh-water lake, on a large common in the East Riding of Yorkshire, about 12 miles west of Hull. The lake is from 100 to 200 acres in size, according to different seasons, and empties into the Humber; which is pretty salt, and has sometimes infected it a little at very high tides. The water is very bright, and the bottom in many places is quite covered with these balls, like a pavement, at different depths. These now sent were about 6 inches under water; and many are left quite dry every summer. On this communication Mr. Watson observes, that the vegetable here mentioned, he has never seen before; neither had he been able to find it described in any of the botanic writers he consulted. The matter of which it is composed, is that of a conferva; and should therefore have had a place under that genus in Dillenius's Historia Muscorum. They are of a deep green mossy colour, are hollow, of an irregularly spherical figure, and of different sizes, from an inch an half to 3 inches in diameter. They are covered with very short villi externally, and the thickness, from their external to their internal surface, is about a quarter of an inch; their texture is most compact the nearest to the surface. He denominates them globose conferva. Mr. Ray, in his History of Plants, vol. 1. p. 83, describes a plant, which he found in Sicily, something like this now sent by Mr. Dixon.

LXXXIV. Of the Copper-Springs in the County of Wicklow in Ireland. By the Rev. Henry Kenroy, D. D. p. 500.

These mines lie in the southern part of the county of Wicklow, on each side of the river Arklow, and about 7 miles west from the town of that name, among hills that rise to the height of small mountains. The mine, which was formerly wrought, is that of Ballymurtogh, on the south bank of the river. It yielded vast profit to the undertakers; but it has been disused for some years past. This is amply compensated by the far richer mines of Crone-Bawn, on the north side of this river.

Crone-Bawn is a hill of 2 miles in circumference, and about 1000 feet in height, in the form of a large inverted bowl. The bowels of this hill are full of rich mines. But the principal works lie on the east side, about half way up the hill, where several shafts are sunk, from 50 to 70 fathoms deep. The first mineral met with is an iron stone. Beneath this they arrive at a lead ore, which seems mixed with clay, yet yields a large quantity of lead, and some silver. Under this lies a rich rocky silver ore, which sparkles brightly, and yields 75 ounces of pure silver out of a ton of ore, besides a great quantity of fine lead. Having pierced some fathoms through this, they arrive at the copper ore; which is very rich, and may be pursued to a vast depth.

In order to carry off the water from the mines, there are levels carried on a great way under ground, from the lower part of the hill. Out of these levels issue large streams of water, most strongly impregnated with copper. An accidental discovery, which happened not long ago, is likely to make these streams more beneficial than all the rest of the mines. Some of the workmen having left an iron shovel in the stream, found it some weeks after incrusted with copper, insomuch that they thought it converted into copper. This gave the hint of laying bars of iron in these streams, which is done in the following manner:

Oblong pits are dug 10 feet long, 4 wide, and 8 deep: the bottom laid with smooth flags; the sides built up with stone and lime, with wooden rude beams across the pits to lay the iron bars on. Chains of these pits are continued along the stream, as far as the directors please; for the water never abates of its quality, if it were conveyed from pit to pit through a thousand. Soon after the iron bars are laid in these pits, they contract a copper rust, which by degrees entirely eats away the iron. The copper, which is in the water, being thus continually attracted and fixed by the iron, subsides to the bottom of the pit. To hasten this dissolution, the iron bars are sometimes taken up, and the rust rubbed off them into the pit. In the space of 12 months the whole bar is commonly dissolved, if the iron be soft; for steel or hard iron will not do. The stream is then turned off the pits; and the men with shovels throw up the copper, which lies on the flag at the bottom, like reddish mud. This mud, being laid in a heap, as soon as dry becomes a reddish dust. It is then smelted into copper.

This being the apparatus, the product is thus. One ton of iron in bars produces a ton and 19½ cwt. of this copper mud or dust. Each ton of this mud produces, when smelted, 16 cwt. of the purest copper, which sell at 10l. per ton more than the copper which is made of the ore. There are about 500 tons of iron now laid in these pits; and the proprietors may, with proportionable advantage, lay in many thousands. The water that runs from these mines, enters the river Arklow on New Bridge; and is of so corrosive a nature, that no fish can live in this large river from hence to the sea.

LXXXV. Extract of a Letter to Dr. Maty, F. R. S. from Geneva, concerning the Introduction and Success of Inoculation in that City.* p. 503.

In September 1750, the practice of inoculating the small-pox was first introduced into Geneva. The example was set by a young lady; and was, the next year, followed in the hospital of foundlings, where it was admitted by an order of the governors, and authorized by the magistrates. Their method of doing it was generally the same as that now commonly used in England; whence instructions were sent to Geneva, when they first began to inoculate. Yet 3 persons were inoculated in a new manner. These were blistered slightly, by means of a small vesicatory applied to that part of the arm, where the incision is usually made. The blister occasioned by this plaster was opened, and a pledget dipped in the pocky matter was applied to the excoriated part. In one instance the incision was made only in one arm; the success of which was the same, as when it had been made in both. Some pocky matter was made use of, which had been kept 3 weeks; and some that had even been kept 4 months, without any apparent difference in the effects from that which was fresh; unless it was owing to this, that, in one instance, the small-pox came out 4 days later than the usual time.

The experience, which they have hitherto had in Geneva, has suggested to them a conjecture, that the incision ought to be made deeper when the matter has been kept some time. All who have yet been inoculated in Geneva, have recovered; and the far greater number of them have had but an inconsiderable number of pustules.

LXXXVI. A Letter from James Parsons, M. D., F. R. S. to the Rev. Mr. Birch, Sec. R. S. concerning the Formation of Corals, Corallines, &c. p. 505.

[As it is now perfectly well ascertained that corals and corallines are really the fabrication, or at least the natural and necessary accompaniments, of animals of the polype tribe, this paper may be considered as of no importance.]

* Dr. Matthew Maty, an eminent physician, was born in Holland in 1718, and took his doctor's degree at Leyden. In 1740 he settled in England; and in 1750 he commenced a work, published every 2 months, called Journal Britannique, which gave an account of the chief productions of the English press. In 1758 he was elected F. R. S. and in 1765 he succeeded Dr. Birch, as Secretary to that learned body. He died in 1776, leaving one son, the Rev. Paul Henry Maty, who also was afterwards Secretary of the R. S. Dr. Maty was likewise one of the librarians of the British Museum; and he wrote the Memoirs of the Earl of Chesterfield, prefixed to that nobleman's miscellaneous works, in 2 vols. 4to.

LXXXVII. A further Account of the late Plague at Constantinople, in a Letter from Dr. Mackenzie, of the 23d of April, 1752. p. 514.

As a corollary to his former account sent to Dr. Mead, Dr Mackenzie subjoins that, on January 3, 1752, there was an accident of the plague, when the thermometer was at 53. Jan. 24, another accident, therm. 52. Jan. 26, an accident at Buiukdere, therm. 51. Feb. 8, accidents at Cassim, Pacha, and Phanar, therm. 52. Feb. 10, an accident in Galata, therm. 55; patient recovered. Feb. 15, another accident in the same house, therm. 53. March 8, an accident in Galata, therm. 56; and not one accident afterwards, though at the above date the thermometer was at 50, and had been at 44 the 16th instant; so that they had great hopes to get clear, if no infection should be conveyed to them from any other quarter.

Prosper Alpinus observes, that the etesian winds at Cairo remove the plague entirely; so that they fear nothing after these winds begin. And Dr. M. was assured that all the plagues which had been at Smyrna and Constantinople for the last 20 years, had been most violent during the season of the etesian winds; still allowing that were it not for the etesian winds, the plague would be more violent in the hot months.

LXXXVIII. A Letter of Mr. James Short, F. R. S. to the Royal Society, concerning the Inventor of the Contrivance in the Pendulum of a Clock, to prevent the Irregularities of its Motion by Heat and Cold. p. 517.

Soon after the invention of pendulum-clocks, justly ascribed to the celebrated Huygens, it was found that they were liable to considerable inequalities in their motion; which were imagined to rise from the pendulum, in its vibrations, describing an arc of a circle; and consequently that the larger vibrations must be slower than the shorter ones. In order to remedy this imperfection, Mr. Huygens wrote a treatise, called Horologium oscillatorium, a piece of geometry which does honour to the last century, in which he demonstrates, from the properties of the cycloid, that the vibrations of a pendulum, moving in a cycloid, would be performed in equal times, even though the vibrations were unequal. Pendulums therefore were made to vibrate in a cycloid; but great inequalities were still observed in the motion of clocks.

We do not read of any attempts after this, to regulate the motion of clocks, till the year 1726, when Mr. George Graham delivered into the Royal Society a paper, which is published in the Phil. Trans. N° 392, in which he says, that it having been apprehended, that the inequalities in the motiou of clocks arose from a change of length in the pendulum, by the influences of heat and cold, he, about the year 1715, made several trials, in order to discover, whether there was any considerable difference of expansion between brass, steel, iron, silver, &c. when exposed to the same degrees of heat; conceiving that it would not be very difficult, by making use of two sorts of metals differing considerably in their degrees of expansion and contraction, to remedy in great measure the irregularities, to which common pendulums are subject. He says also, that from the experiments he then made, he found their differences so small, as gave him no hopes of succeeding that way, which made him leave off prosecuting this affair any more at that time: that, some time after, having observed an extraordinary degree of expansion, by heat, in quicksilver, he thought of a proper manner of applying a column of it to the pendulum of a clock, in order to prevent the inequalities arising from its different lengths by the effects of heat and cold; which succeeded accordingly, and is what is now called Mr. Graham's quicksilver-pendulum. Mr. Graham, in the same paper, takes notice, that, though the pendulum of a clock was to remain invariable, yet there would still be some irregularities in the motion of the clock, arising from the friction of the different parts of the clockwork, and from the different degrees of foulness.

In the year 1725, Mr. John Harrison,* of Barrow in Lincolnshire, made several experiments on wires of different metals, to find their different degrees of expansion and contraction: for he thought, that by a proper combination of wires of two different metals, differing considerably in their expansion and contraction, he might be enabled to keep the centre of oscillation of a pendulum always at the same distance from the point of suspension. In consequence of these experiments, he made a pendulum consisting of one steel wire, at the end of which is the ball or weight, and on each side of this wire 4 wires alternately brass and steel, so disposed and contrived, as to raise the pendulum the same quantity as it is lengthened by heat, and to let down the pendulum in the same proportion as it is raised by cold. He made also a drawing of a clock, in which the wheels are disposed in a different manner from those then in use; which drawing Mr. S. has seen, signed by himself in the year 1725. Two of these clocks with pendulums, as described above, were finished in the year 1726. In these clocks Mr. Harrison has made a particular sort of pallets, so as to be almost entirely free from friction; for though he had thus happily succeeded in his

* Mr. John Harrison, an ingenious clockmaker, was born in 1693, at Foulby in Yorkshire, and bred to his father's business, that of a carpenter. Having a good mechanical turn, particularly for wheelwork, he constructed some wooden clocks, the accuracy of which was so much admired, that in 1728 he came to London with a drawing of a timekeeper, which he showed to Dr. Halley, who recommended him to Mr. Graham, from whom he received great encouragement to prosecute his design. In 1735 he visited London again with a complete machine, with which he was sent on a voyage to Lisbon by the board of longitude, to make trial of its properties. From that time he went on improving his invention, and at length received the reward of more than 20,000l. allowed by parliament for the discovery of the longitude. He died in 1776, at 83 years of age.

contrivance to prevent the inequalities in the motion of the clock, arising from the different lengths of the pendulum by the effects of heat and cold, yet he found there were considerable errors still remaining, occasioned by the friction of the pallets, as in the common way. He also suspended the pendulum on the wall of the house, entirely independent of the clock and clock-case: for he had observed considerable alterations in the going of the clock, when the pendulum is suspended as in the common manner. His pendulum vibrates in an arc of about 15 degrees, with a ball of about 3 lb. between cycloidal checks, which he himself found were necessary, though he had never heard of Huygens's book, till after he had made them. He has also disposed the force of his pendulum-wheel on the pendulum, by his sort of pallets, in such a manner, that the vibrations of the pendulum will not be affected by the different resistance of the air. On the whole, this clock is made in such a manner, as to be almost entirely free from friction; in consequence of which he uses no oil, and therefore there is no necessity ever to clean the clock. When he settled in London, he sent for one of these clocks from the country, and set it up in his house in Orange-street, in the year 1739, where it has stood ever since, and in all that time has never varied above one minute from the truth. He can depend on it to a second in a month.

About the year 1729, Mr. Harrison made his first machine for measuring time at sea, in which he likewise applied this combination of wires of brass and steel, to prevent any alterations by heat and cold. In 1746, he went on board one of his majesty's ships of war with this machine to Lisbon, and returned, where this machine was publicly shown. Since that time, he has made two more of these machines or clocks for keeping time at sea, in both which he has likewise this provision, to prevent the effects of heat and cold. An account of these and of the many contrivances which Mr. Harrison has made use of in them, for answering their intended purpose, and an account of the success of his voyage to and from Lisbon, is contained in a speech of the President Martin Folkes, Esq. on his delivering to Mr. Harrison the gold medal of Sir Godfrey Copley; which speech is inserted in the minutes of the Society in 1749.

Mr. John Shelton, who was the principal person employed by Mr. Graham in making astronomical clocks, informs, that Mr. Graham, in 1737, made a pendulum consisting of 3 bars, viz. one of steel, between two of brass, and that the steel bar acted on a lever, so as to raise the pendulum, when lengthened by heat, and to let it down, when shortened by cold. This lever, which is very strong, rests on a roller, made moveable, so as to adjust the arms of the lever to their true proportion. The whole was made to be as free from friction as possible in such a construction. Mr. Graham made observations, by transits of the fixed stars, of the motion of the clock with this sort of pendulum, and from the experience of several years, during which the clock was kept constantly going, he found that the clock was liable to sudden starts and jerks in its motion. Of this he informed Dr. Bradley, Mr. Bliss, Mr. S. and several other gentlemen. This clock still remains in Mr. Graham's house, in the possession of his executors.

Mr. S. had been informed, that one Mr. Frotheringham, a quaker, of Lincolnshire, caused a pendulum to be made, consisting of 2 bars, one of brass, and the other of steel, fastened together by screws, with levers to raise or let down the ball; and that these levers were placed above the ball. This clock Mr. S. had seen, and was told by the maker, Mr. John Berridge, that the pendulum of it was made in 1738 or 1739, and that the dial-plate of it was engraved at Mr. Sisson's house in 1738: and this clock is in the possession of Mrs. Gibson, in Newgate-street, who has had it ever since the year 1739.

In the Hist. of the Royal Acad. of Sciences at Paris, for 1741, there is a memoir of M. Cassini, in which he describes several sorts of pendulums for clocks, compounded of bars of brass and steel, and applies a lever to raise or let down the ball of the pendulum, by the expansion or contraction of the bar of brass. He has also given in the same memoir, a problem for finding the proportion which the two arms of the lever should have, to answer the intended purpose; and also a demonstration of it.

In June, 1752, Mr. John Ellicot gave in to the Royal Society a paper, containing the description of a pendulum, consisting of 2 bars, one of brass, and the other of iron, fastened together by screws, with 2 levers in the pendulum ball, so contrived as to raise and let down the ball, by the expansion and contraction of the brass bar; and also to adjust the arms of the levers to their true proportion.* He says, that he first thought of these methods of applying bars of brass and iron to prevent the irregularities of a clock, arising from the different lengths of the pendulum, by the effects of heat and cold, in 1732; and that he put his thought in execution in 1738.

In 1743, Mr. S. bought a clock of Mr. Graham, which he had kept going for 2 years before. This clock has a pendulum, compounded of wires of brass and steel, in the manner of Mr. Harrison's combination. It has also a provision in the ball, to adjust the wires, in case they happen to be too long. When Mr. S. first took notice of this contrivance or provision in the ball, he asked Mr. Graham the reason of it; who told him, that having observed some inequalities in the motion of the clock, he imagined that they arose from the wires being somewhat too long; and therefore added this contrivance, to adjust the length

* He has also given, in the same paper, another construction of a pendulum to prevent the effects of heat and cold, consisting of 2 bars, one of brass, and the other of iron; the brass bar acting on a lever, at the end of which is fastened the pendulum, the whole so constructed and contrived, as to raise the pendulum, when it is lengthened by heat, and to let it down, when shortened by cold.—Orig.

of the wires; but that when he had done this, he found inequalities still remaining; and therefore justly concluded, that they arose from the difference in the friction of the different parts of the clockwork, occasioned by the differences in the fluidity of the oil, &c.

From what has been said above, it appears, that the improvement of clocks, by a contrivance to prevent their inequalities arising from the different lengths of the pendulum, in different seasons of the year, by the effects of heat and cold, was first thought of, and executed, by Mr. George Graham; and that the application of wires or bars of two metals, which have different degrees of expansion or contraction, to prevent the same inequalities, was also first thought of by Mr. Graham, and first executed by Mr. John Harrison, without the least knowledge of what Mr. Graham had done before him.

LXXXIX. On the Cause of Thunder. By Mr. Henry Eeles, dated Lismore, Ireland, June 18, 1752. p. 524.

Mr. Eeles's opinion on the cause of thunder is, that it is by electrical explosions among the clouds, the fire of lightning and electricity being of the same nature, as had been long before proved by the experiments of Mr. Franklin in America. After the explosion, then the echo of it from the other clouds is the cause of the continued or distant rumbling noise. Mr. E. adds, that he intends afterwards to show, that this fire is a most considerable agent in nature. First, that the ascent of vapour and exhalation is principally owing to it, and that our atmosphere, by that means, is kept more homogeneal than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matter are kept suspended at their usual height merely by this fire. Secondly, that this fire is the cause of the reflection, refraction, and inflexion of light. Thirdly, that it is the cause of that secondary attraction and repulsion, which Sir Isaac Newton has taken notice of. Lastly, he will give some hints of the great use of this fire in animal life, and in vegetation.

XC. On Mons. Daviel's Method of Couching a Cataract. By Thomas Hope, M. D. p. 530.

Dr. H. states that he had heard of a new method of performing the operation for the cure of the cataract, but did not care to say any thing of it, until he had seen it himself, and had inquired into the success of it. M. Daviel, a surgeon of Paris, was the first who, in 1745, began to put it in practice, and had at last brought it to perfection; of which he gave a memoir to the Academy of Sciences, of 115 operations, 100 of which succeeded. Dr. H. saw him perform it on 2 persons, of which the following is a description.

After having placed the patient in a right light in a chair, he places himself over-against, and somewhat higher than the patient: an assistant holds the head steady, another keeps the upper eye-lid open; he, with his left hand, keeps open the lower eye-lid. He then takes an instrument like a lancet, of a myrtle-form point, a little crooked upwards, and fixed in a handle, and, making the patient look upwards, he pierces the cornea transparens at its lower circumference, just where it joins the sclerotica, conveys the point of the instrument between the cornea and iris upwards, beyond the pupil; he enlarges this opening on each side by the same instrument: he then takes out this instrument, and introduces another of the shape of a narrow lancet, made round at the point, fixed in a handle: with the cutting sides of this he enlarges the opening. Taking out this, he introduces a pair of crooked scissars, enlarges the opening on each side by different snips, always as near as he can to the circumference of the cornea transparens, till he has made the opening round two-thirds of the cornea transparens: he then takes out the scissars, and, with a small instrument like an ear-picker, he raises the cornea, and having in his right hand a cataract-needle, broader and stronger than the common, and pointed like a lancet, he cuts the capsula of the crystalline through the pupil; then, pressing gently the globe of the eye with his finger from below upwards, the crystalline slips out of the capsula, and drops out of the eye. On the first puncture, the aqueous humour coming out, the cornea and iris join together: and it requires great dexterity, and a very steady hand, to introduce the instruments so as not to wound the iris, which would endanger the eye.

Though the operation lasted above 2 minutes, the patient never complained of any pain; and said he felt nothing but a tickling. By which it appears the cornea is not much more sensible than the nail of one's finger. And this operation, which seems so cruel to a by-stander, does not give so much pain as couching in the usual manner. It is to be preferred to couching in many respects. It may be performed at all times, and in all kinds of cataracts, whether they are come to maturity or not. It also avoids many inconveniencies and accidents, which often baffled the success of the best operations; such as the rising again of the cataract, violent defluxions and inflammations, which often destroyed the eye, the hurting of the vitreous humour, which seldom failed in couching, &c. In both the operations, which Dr. Hope saw, the patient, immediately after, could distinguish all large objects in the room.

M. Daviel says, that he has found by experience, that all those instruments are necessary: and as to the extent of the incision, he says that he seldom makes it above one-half of the circumference of the cornea transparens; and that a smaller opening would not suffice to let the crystalline slip out easily; the diameter of which, in general, not being above a line less than that of the cornea, and in some cases within half a line, insomuch that, in order to make it pass through the pupilla, he has been obliged to give a snip of the scissars to the iris, which he says is attended with no bad consequences.

In answer to what is said, that it has been practised before, and that Taylor formerly performed it, he endeavours to prove, that it never was, excepting in cases where the crystalline had, by some accident, slipt through the pupilla into the anterior chamber. In regard to the operation, there is some mention made of it among the Arabians, as what they had heard of; but the operation is not described particularly any where. One convincing reason, that it never was carried into practice among the ancients, is, that had they made the extraction of the cataracts, they must have found it to be the crystalline humour, and not remained in the error they have all fallen into, that the cataract was a membrane formed in the aqueous humour.

In regard to Taylor, he may have attempted, but never did carry it into practice; else he would not have failed to have published it in the numberless productions he has given. Dr. H. knows that, in 1743, he followed him in Edinburgh for 6 months, where he performed above 100 operations of the cataract by couching; but never once attempted this way, nor ever mentioned it, but in the case where the crystalline is lodged in the anterior chamber; which operation has been described in many authors. So that he thinks, Mr. Daviel may be truly said to be the first, who had brought this method into general practice for the cure of a cataract.

Dr. H. thinks, the greatest risk one runs in this operation, is the pushing out of the humours of the iris through the opening, which forms a staphyloma; and he finds this has been the case in some of those that have failed; and it is not easy to contrive a bandage on that part, to make a compression equal to the resistance of the cornea before it was opened.

XCI. Letters of the Abbé Mazeas, F.R.S. to the Rev. Stephen Hales, D.D., F.R.S. on the Success of the late Experiments in France, concerning the Analogy of Thunder and Electricity. Translated from the French by James Parsons, M.D., F.R.S. Letter 1, *dated St. Germain, May* 20, 1752, *N.S.* p. 534.

The Philadelphian experiments which Mr. Collinson communicated to the public, having been universally admired in France, the king desired to see them performed. Therefore the Duke d'Ayen offered his majesty his country-house at St. Germain, where M. de Lor, master of experimental philosophy, should put those of Philadelphia in execution. His majesty saw them with great satisfaction, and greatly applauded Messieurs Franklin and Collinson. These applauses of his majesty having excited in Messieurs de Buffon, D'Alibard, and De Lor, a desire of verifying the conjectures of Mr. Franklin, on the analogy of thunder and electricity, they prepared themselves for making the experiments.

M. d'Alibard chose, for this purpose, a garden situated at Marly, where he placed on an electrical body a pointed bar of iron, of 40 feet high. On the 10th of May, 20 minutes past 2, afternoon, a stormy cloud having passed over the place where the bar stood, those that were appointed to observe it, drew near, and attracted from it sparks of fire, perceiving the same kind of commotions as in the common electrical experiments. M. de Lor, sensible of the good success of this experiment, resolved to repeat it at his house in the Estrapade at Paris. He raised a bar of iron 99 feet high, placed on a cake of resin, 2 feet square, and 3 inches thick. On the 18th of May, between 4 and 5 in the afternoon, a stormy cloud having passed over the bar, where it remained half an hour, he drew sparks from the bar. These sparks were like those of a gun, when, in the electrical experiments, the globe is only rubbed by the cushion, and they produced the same noise, the same fire, and the same crackling. They drew the strongest sparks at the distance of 9 lines, while the rain, mingled with a little hail, fell from the cloud, without either thunder or lightning; this cloud being, according to all appearance, only the consequence of a storm, which happened elsewhere. From this experiment they conjectured, that a bar of iron, placed in a high situation on an electrical body, might attract the storm, and deprive the cloud of all its thunder.

I do not know, Sir, whether Mr. Franklin's letters were before your consideration on earthquakes: if they were, we are obliged to Mr. Collinson for his communication of Mr. Franklin's notions; if they are not, you deserve the honour of the discovery; and whoever it be, it is still to the R. S. we owe the communication of this ingenious thought, which the experiments of M. d'Alibard and M. de Lor have confirmed. These 2 learned men deserve that esteem of our nation which their talents have a long time procured them.

Letter 2. *Dated St. Germain, June* 14, 1752.—M. le Monnier, having prepared to repeat the same experiments, avoided that inconvenience in the resin cakes being wetted by the rain. He placed, in the garden of the hotel de Noailles, a wooden pole, of about 30 feet high, at the end of which was fixed a large glass tube, which received at the other end a long tin pipe; and this pipe received again, in its turn, a pointed bar of iron, of about 6 feet high. The glass tube was instead of the cake of resin, to hinder the communication of the electricity from the tin pipe to the pole. A wire was carried from the bar of iron, which rested on a silken cord, about 50 paces from the pole; but rain coming on, the wire was conducted into the house. We perceived the commotions of the electrical matter from the first clap of thunder; it produced sparks, and there were certain intervals, when the commotions were so strong, that they were accompanied with very sharp pain. It seemed as if the commotion was the greater, the nearer the thunder was to the bar.

Letter 3. *Dated St. Germain, June* 20, 1752.—On the 26th of this month there was a storm at two different times: the 1st was at 3 in the afternoon, and the second at half after 6. This storm, which came from the south-west, was very inconsiderable: there were but 2 or 3 claps of thunder, either at 3 or at 6 o'clock; and there was a considerable interval between the lightning and the clap, which showed that the thunder was at a great distance. Yet the effects of the electricity were very violent, which we attribute to M. le Monnier's ingenious apparatus; which was as follows:

It is certain that the greater the quantity of these bars, the greater is the quantity of electricity furnished by the magazine. In the last experiment there was a tin pipe of 7 feet long, and about 5 inches diameter. It was the 1st magazine: the 2d consisted of 6 great bars of iron of 6 feet long each, placed in parallel order on glass bottles. All these magazines communicated with the iron wire, that descended from the little bar at the top of the great pole, described in the last letter.

The 26th of this month, at 3 afternoon, very lively sparks were excited, and M. le Monnier set fire to spirits of wine. At 6 o'clock the Abbé M. went up to a proper place, in order strictly to observe the intervals between the commotions and the electricity. The clouds extended from the south and west to the zenith of the pole, and the lightning came from a very distant part; and, in proportion as the clouds came nearer, the electricity was felt with very smart shocks, but without light or regularity; for sometimes none were felt for 2 or 3 minutes, and it was commonly with every flash of lightning that the commotion was felt. But when the clouds had covered a considerable part of the heavens, the commotions of the electricity succeeded very quickly with noise and sparks; though the thunder could scarcely be heard, because of its distance. It may hence be judged how strong the commotions would be, if the clouds, which produced the thunder, were nearer the bar.

On June 29 there was another storm; but the Abbé was not present at the experiments made in the garden, being employed in a like experiment in his chamber. He placed at his window, which was about 35 feet from the ground, a bar of iron of 12 feet long, which received a very sharp iron wire of 6 feet high; the whole advanced into the street, by means of a wooden pole laid parallel to the horizon; at the end of which was a glass tube filled with resin, to receive the iron rod. The wire that hung from the extremity of the pole entered into the chamber, and from thence into a gallery of 30 feet long. The electrical magazine was in his chamber, and the iron wire, after several turnings, was again brought thither. He had disposed of this wire in such a manner, that if the storm should come in the night, or if it happened by day, he had it in his power to observe all he proposed, without quitting his bed or his business.

The storm came at 5 in the evening; and though he had not yet time enough to form a sufficient magazine of electricity, he had nevertheless very satisfactory signs. The person who held the iron wire felt a commotion; and at the same instant silken ribands were attracted by the electrical magazine. There came on a great shower of rain and hail, which wetted the resin in the glass tube that supported the bar; and after that there were no more signs of electricity. The same thing happened in the garden; where the silken cords, which in several places interrupted the communication of the electrized bodies with the non-electrics, having been wet, sensibly diminished the desired effect. The electricity however was very strong before the rain fell; and the commotions were felt at about a foot distance: but the storm only passed by, and lasted no more i the whole than 2 or 3 minutes.

Letter 4. Dated St. Germain, July 12, 1752.—M. le Monnier, who performed the experiments, was convinced that the high situation, in which the bar of iron was commonly placed, is not absolutely necessary to produce the effects of electricity: for a tin speaking trumpet suspended on silken cords, about 5 or 6 feet from the ground, has produced very particular signs of electricity.

A man placed on a cake of resin, and holding with his hand a wooden pole, of about 18 feet long, round which an iron wire was twisted, was so well electrized while it thundered, that very lively sparks were drawn from his face and hands.

Having taken away the communication of the electrical magazine with the iron wire, which hung from the great wooden pole (this magazine consisting, as mentioned in the last letter, of 6 great bars of iron, placed horizontally on glass bottles, about 4 feet from the ground), this magazine was strongly electrized, when the stormy cloud passed in the zenith.

A man standing on the electrical cake in the middle of the garden, and simply holding up one of his hands in the air, attracted with the other hand wood-shavings, which were held to him on a piece of lead. Whence it evidently follows, that the matter which is the cause of all the surprizing phenomena, which electricity affords us, fills the atmosphere in the time of a storm; that it penetrates us; that we breathe it with the air; and that the height usually given to the iron bar, only serves to intercept the far greater quantity of the electrical matter.

At the time that M. le Monnier made his experiments, the Abbé, in his turn, tried to perfect the manner of bringing the electricity into his chamber. He therefore increased the length of his wooden pole, which went out of his window, and at the same time that of his iron rod, which was perpendicularly fastened to its end. The greater the length and height that these two were, the stronger was the electricity in the chamber.

Towards 11 in the morning, the heavens began to be covered to the south-west, with some claps of thunder and lightning at a great distance. The Abbé had just time to go to the garden, where he found the Duke d'Ayen, who had prepared every thing for the experiments. An iron wire descended from the top of the pole, and rested on the hot-house of the garden: this wire was supported by a silken cord, and was terminated by a tin cylinder, of about 3 inches diameter, and 3 feet long. The electricity of this cylinder was such that, when a finger approached it, 2 or 3 very lively sparks at a time were produced, with a sparkling noise, like that of the nails of one's fingers crackled against each other. Then the Duke d'Ayen took the first shrub he met in the hot-house, which happened to be that from which the labdanum is produced: he placed it with its pot on a cake of resin, and fastened the iron wire to one of its branches. This shrub was instantly electrized, so that whitish sparks issued from every leaf, with the same kind of crackling just mentioned; but the trunk of this shrub had a much stronger electricity; whether at that instant the electricity of the cloud was more strong, (for it varies every moment) or that the force of the whole electricity, expanded through the leaves, became concentrated in the trunk of this shrub.

The duke then took one of his silver watering-pots, which was 2½ feet high; he filled it with water within an inch of the brim, and placed it on the electrical cake, dipping into it a wire of lead, which communicated with that wire which came from the top of the pole. Of all the electricity tried till then, this was incomparably the strongest; there were 20 sparks; and on advancing the finger towards it, the shock affected the arms and breast with great violence.

Letter 5. *Dated Paris, August* 21, 1752.—A phenomenon, which I have always thought worthy of strict observation, is the diminution of the electricity of thunder, when rain comes on during the storm. This diminution was remarked at St. Germain, every time I was a witness to M. le Monnier's experiments; and the same effect is, within this little while, confirmed to me by the learned Mr. Euler, in communicating to me the observations of M. Ludolf.

I left St. Germain the 12th of July to come to Paris, at 7 in the evening. At the instant of my arrival, I saw the heavens covered with clouds, and the lightning foreboded thunder, which soon was heard. I went up into the gallery of the Hotel de Noailles, which is very high, and distant from the neighbouring buildings: my pole was 10 feet high; at the end of which a glass tube was made fast; and to this a very sharp iron spire, from the middle of which a wire of about 20 feet long came down, and rested on a long glass tube fixed to the balustrade, which environed the gallery. My apparatus was scarcely ready, when it thundered, and the clouds broke by this first clap, and poured down a continual large quantity of rain, which lasted near 2 hours, without the least discontinuance of the thunder.

I felt no commotion in putting my finger towards the wire, nor could I draw any sparks from it. I was on the point of giving it over, when the wire happened to touch the leads and the balustrade of the gallery; and it instantly produced as many sparks, as it touched places on the balustrade and leads. I then took the wire in my hand, and threw it strongly against the bars of iron; and as the wire extended, and successively touched the bars, it always produced the same effect. There were prodigious multitudes of these shining sparks, like those produced by the finger in common experiments. I only wanted an electrical magazine to accumulate electrical matter in, which would have produced me all the usual phenomena.

The Abbé then communicates the observations that Mr. Ludolf made at Berlin. 1. That the sparks drawn from the wire were half an inch long; and they caused so horrible a shock, that the entire body of the person who attracted them, was shaken; but the small sparks produced only a light sensation in the fingers. 2. It is also remarked, that this electricity communicates itself to all bodies elsewhere, that are susceptible of it, provided they are placed on electrical bodies, while they are made to communicate by a wire. 3. When there was plenty of rain, they scarcely remarked any thing of the force of the electricity, though the lightning and claps of thunder were very strong. 4. At every clap of thunder the electricity seemed extinct, and returned not till after about 30 seconds, and sometimes longer. 5. When the wire was surrounded with drops of rain, it was observed that only some of them were electrical, which was remarkable by the conical figure they had; while the others remained round as before. It was also perceived, that the electrical and non-electrical drops succeeded almost alternately; which made them call to mind a very singular phenomenon, that happened some years ago to 5 peasants, who passed through a corn-field near Frankfort on the Oder in a storm. The thunder killed the 1st, the 3d, and the 5th, without injuring the 2d and the 4th. 6. The storm of the 1st of August was very considerable, with very great rain every minute they remarked 3 or more flashes of lightning; in the mean time some electrical sparks were observed on the wire. They put upon a chain, which communicated with the wire, a thread, the 2 ends of which hung down; which showed electricity by mutually repelling each other; for at every flash of lightning they approached each other suddenly, as if they had been pushed one against the other by some force. 7. Sometimes the electricity continued in the wire with great strength to 45 minutes, after the thunder and lightning had entirely ceased, &c. Conformable to the 6th observation of Mr. Ludolf, the Abbé says he has often observed, that in presenting dust or dried snuff to the end of a tin cylinder, which hung to the wire in such experiments, this dust was strongly attracted, as soon as the wire showed any signs of electricity. But when the electrical matter came to be accumulated in this cylinder, the dust was powerfully repelled as by a strong blast, insomuch that the quantity of molecules repelled was much greater than of those attracted at the same time.

And with respect to this successive attraction and repulsion, the Abbé mentions an experiment he was informed of, without knowing that the author of it was Mr. Franklin. The dishes of a pair of scales were suspended to the balance by silken cords; the two dishes were electrized, and a very sharp needle was presented to one of them. The scales immediately lost their equilibrium; and that dish under which the needle was held was attracted. The direct contrary happened, when an obtuse or round body, such as a leaden bullet, was put on the point of the needle, for then the dish was repelled. If this experiment be true, it strongly imitates what happens in the clouds, when they are in equilibrio in the atmosphere: and it gives room to conjecture, that it would be much less dangerous to terminate the tops of steeples with obtuse bodies, than with pointed spires, on which the thunder falls, sooner or later, when they are very high.

XCII. On Extracting Electricity from the Clouds. Translated from the French, by the Abbé Nollet, F.R.S. Dated Paris, June 6, 1752. N.S. p. 553.

The Abbé, after having taken notice of the discovery of M. d'Alibard in France, in regard to extracting electricity from the clouds during a thunder-storm, in consequence of Mr. Franklin's hypothesis, observes that he is more interested than any one to come at the facts, which prove a true analogy between lightning and electricity; since these experiments establish incontestably a truth, which he had conceived, and which he ventured to lay before the public more than 4 years ago. Examine the 4th volume of his Leçons de Physique, p. 314, and you will find what follows: 'If any one should take upon him to prove, from a well connected comparison of phenomena, that thunder is in the hands of nature, what electricity is in ours; that the wonders which we now exhibit at our pleasure, are little imitations of those great effects which frighten us; and that the whole depends on the same mechanism; if it is to be demonstrated that a cloud, prepared by the action of the winds, by heat, by a mixture of exhalations, &c. is opposite to a terrestrial object; that this is the electrized body, and at a certain proximity from that which is not; I avow that this idea, if it was well supported, would give me a great deal of pleasure; and in support of it how many specious reasons present themselves to a man who is well acquainted with electricity! The universality of the electric matter, the readiness of its action, its inflammability, and its activity in giving fire to other bodies; its poverty in striking bodies externally and internally, even to their smallest parts; the remarkable example we have of this effect in the Leyden experiment; the idea which we might truly adopt in supposing a greater degree of electric power, &c. all these points of analogy, which I have been some time meditating, begin to make me believe that one might, by taking electricity for the model, form to oneself, in regard to thunder and lightning, more perfect and more probable ideas, than what have been offered hitherto, &c.'

To demonstrate, that glass is not absolutely impermeable to the electric fluid, the Abbé offers the following experiment: Let the neck of a small thin phial A, fig. 15, pl. 6, be placed in that of the receiver B; and lute it in such a manner, as that the air cannot pass through their joining. Exhaust the receiver, and pour the little phial 3 parts full of water, and conduct the electricity into it by means of an iron wire, suspended to the conductor. Make the experiment in a dark place, and for the greater surety fix the receiver to the plate of the air-pump, not with wet leathers as usual, but with soft cement. You will see the electric matter pass, as through a sieve, through the small phial into the receiver, and present itself in an infinite number of luminous streams, of extraordinary beauty; and if you do not take care you will be smartly shocked, as in the Leyden experiment, by laying one hand on the receiver, and touching the plate of the air-pump with the other.

To prove that in the Leyden experiment the electrical virtue, or power of giving a shock, does not reside only in the glass, make the following experiment: Electrize a phial two-thirds full of water; pour this water into another thin phial, placed on a glass stand; plunge an iron wire into it, and attempt, while the phial is in one hand, to draw a spark with the other: it is certain that if this is done with a little readiness, you will make the Leyden experiment with this water.* Possibly you may not always succeed with water; but with mercury, under the same treatment, it never fails. Whence proceeds the power of giving the shock to the second glass, if it is not by means of the water, which it has received?

Electrize a bolt-head of glass, void of air, and sealed hermetically; you may make use of it for the Leyden experiment, and you will succeed. Is there not then a communication between the exterior and interior surface of the glass? And is it not evident further, that the electric matter, which is perceived running within like a torrent of fire, passes through the glass?

* Some years ago I showed this experiment to several members of the R. S., and not only produced the Leyden experiment with it, but by pouring the electrized water into a basin, held in one hand of an assistant standing on cakes of wax, who, on his presenting a finger of his other hand to some warm spirit of wine in a spoon, held in the hand of a person standing on the floor, set it on fire. I then considered this experiment as a proof of the electricity being accumulated in the water. W. Watson.—Orig.

When you force a hole through a piece of paper or pasteboard, attend to one thing, which I constantly observe. If you electrize the plate of glass, AB, fig. 15, underneath, and that, by means of a thick iron wire somewhat bent, D, you draw the spark through a piece of pasteboard, C, placed on the metal, with which the glass is coated, the hole will appear invariably larger underneath, than on the top of the pasteboard; and this hole will have an impression at the place where the iron wire shall have been supported. These 2 effects leave no room to doubt, but that the stroke of fire was directed from the glass to the conductor, E, by the bent iron wire. Besides, if the electric fire proceeds from the upper surface of the glass, which receives the electricity from the under surface, it necessarily follows, that it must have passed through the whole thickness of the plate of glass; and consequently that the glass is not impenetrable to the electric fluid.

The electrical experiments, which have been made here during the thunder, are now sufficiently verified. Dr. le Monnier, assisted by his advantageous situation, has sufficiently experienced, first, that a bar of iron, pointed or not, is electrized during a storm: 2dly, that a vertical or horizontal situation is equally fitting for these experiments: 3dly, that even wood is electrized: 4thly, that by these means a man may be sufficiently electrized to set fire to spirit of wine with his finger, and repeat almost all the usual experiments of artificial electricity; for thus the Abbé denominates that which is excited by friction.

Seeing therefore that these experiments succeeded so well, he attempted them at Paris with a tube of tin, 18 feet in length, and of an inch and half in diameter; half of which tube he put out of the window, while the other half was placed on, and fastened to, silk lines: and though he lived in the lowest part of Paris, and his apartment in the Louvre is covered with an immense building, both in height and extent, at any time when the thunder was but moderate, he perceived signs of electricity. The sparks were more frequent after the lightning than after the thunder; and it even seemed that the clap of thunder put a stop, for a very short time, to the force of the electricity.

Mons. Cassini de Thury, who was desirous of observing these effects with the apparatus which they had erected on the terrace of the observatory, made the same remarks; and he has had a greater opportunity of observing them, because the effects there were more considerable, on account of the situation. He even remarked very evident signs of electricity, though there was neither lightning nor thunder, but only the sky covered with such thick clouds as seemed to forebode a storm.

Mons. le Roy, a member of the Academy of Sciences, who lives near the Abbé, has repeated also a great number of these experiments and observations by only making use of a pole of wood 25 feet long, about which he turned an iron wire in form of a screw.

XCIII. Extract of a Letter from Mr. Mylius of Berlin, to Mr. W. Watson, F.R.S., on the before-mentioned Subject. Dated at Berlin, August 26, 1752. p. 559.

March 16 last, at a little past 8 in the evening, we had here a slight earthquake, which manifested itself by its shaking the ground, the windows, and by opening some doors. This we have had no example of before in our country; and it was perceived at the same time at Stavanger in Norway. I have made experiments of collecting the electricity, during a thunder-storm, with great success, in company with Professor Ludolf. He had erected an iron bar, of 12 feet long, which was fastened on a pole of wood, 50 feet in height, with 2 tubes of glass covered with tin. The upper end of the iron bar was sharp pointed, and near the lower end was fastened a very long iron wire, which being carried into a summer-house, gave great sparks, as the thunder was approaching; and these sparks caused sometimes as violent a shock through the body, as the experiment of Leyden. It was also continually observed, that the effects were greatest, when the lightning was nearest; and that for some moments after the lightning, the effect ceased, but returned and increased by degrees.

XCIV. Mons. Faget's Remarks on the Use, &c. of the Styptic, purchased by his most Christian Majesty. Communicated by James Theobald, Esq. F.R.S. p. 560.

About the end of the year 1750, Mr. Brossard, a surgeon from Berry, came to Paris, to propose the use of a remedy, which he had discovered for stopping the blood after amputations, and which he asserted to have found effectual in several amputations of the arms and legs. At his request, some gentlemen of the Academy of Surgery were deputed, in whose presence he was to make some new experiments in stopping the blood on different animals, and in all which he succeeded, by stopping it in the largest arteries after amputation. But the success of this remedy might yet be considered a little dubious, because in many animals, as in dogs particularly, the great arteries stop of their own accord; and rarely any dogs die from an hæmorrhage, because their blood is more disposed to coagulate, and by that means stop the discharge. For this reason the experiments made on animals not being thought satisfactory, and yet being convinced that no ill effect could follow the application of this remedy on human kind, Mr. Brossard was permitted to use it at the Hospital of the Invalids, in an amputation of the leg, which succeeded perfectly well.

Some time after this 2 waggoners were run over by a waggon loaded with stone, and each of them had one leg broken in a miserable manner. These 2 men being brought to the Hospital of the Charity, Mons. F. saw no other hopes of success but in amputating the legs; and therefore he requested Mr. Brossard would be present, and give a proof of this new application, which they applied in the following manner: As soon as the leg was cut off, he slackened the tournequet, to discover the vessels; and Mr. Brossard applied, on the orifices of the 2 arteries, 2 pieces of his astringent, fastened one on the other with a ribband. After the application was made, Mr. F. straitened the tournequet, and passed the 2 ends of the ribband, which was fastened to the upper piece of the astringent, on the stump over the knee, and applied a linen bag, filled slightly with the same astringent in powder, on the whole wound; and over all applied the common dressings in the like case. After the dressing was finished, he slackened the tournequet, and 2 hours after took it entirely away. Eight and forty hours after this, they took off the dressings, and not the least drop of blood followed from the vessels: and they again applied 1 single piece of the astringent on the 2 vessels; and he dressed the other parts of the wound with pledgets of lint, with common digestive, a styrax plaster, and the usual bandage.

The 3d day the astringent fell off of itself in the time of dressing; and the patient, after that time, was dressed in the common manner. The same was done to the other patient, after the amputation, as to this.

The first of these men died on the 5th day, and the other on the 9th: but there did not appear, through the whole, the least tendency to an hæmorrhage. Thus the remedy fairly produced its effects, as to the stopping the blood.

However, in order to determine the manner in which this astringent produces its effects, he examined the blood-vessels of those 2 patients after their death, and found them contracted and straitened, as if they had been tied, and in the largest of them a conical coagulation of the blood, which was an inch and half long: and after having taken out this coagulation, it was with difficulty that he could introduce the point of a very small probe into the orifice of that vessel. The patient, who died on the 9th day, had the arteries contracted in the same manner; but with this difference, that the coagulation was at least 4 inches long.

Mr. Morand has employed this remedy with success in applying it to a wound, made by a sword in the bending of the arm: and Mr. F. himself had made use of it, with great success, on occasions where the temporal and intercostal arteries had been opened. In the last-mentioned cases, he applied only 1 piece of the styptic on the opening of the artery; and this generally fell off at the first dressing, that is, 48 hours after the application, without the least appearance of an hæmorrhage, or other ill symptoms, which could raise any objection to this styptic; for those patients were all recovered.

There had been lately made, at the Hospital of the invalids, 2 experiments of this astringent in amputations; and in both the success has been equal to all that can be desired. The surgeon, in these cases, used only the 2 pieces, applied one over the other, without using the powder in the bag, as before; and dressed the whole wound with lint and the common bandage.

Thus then at last there appears to be discovered a remedy beyond our hopes, and which art has never yet equalled. The application of fire was the cruel resource of the ancients; and Paré believed himself inspired when he discovered the use of the ligature. But, alas! how many accidents are there, which arise from the use of those 2 manners, and which too often terminate in the death of the patient! Happy for us, that those accidents now appear to be no longer to be feared, by the lucky discovery of this styptic, the first experiments of which have so greatly promised success!

It may be remarked, that, if this astringent succeeded only in coagulating the blood, it had produced nothing extraordinary; for these coagulations would not have been sufficient to have stopped the hæmorrhage, directly after the operation in amputations: but its excellency lies in contracting the arteries so closely, that it hardly lets a little probe into the aperture of the artery, and by this means forms, as it were, a perfect ligature, much more certain than the usual one; as this is not made in any one point of the cylinder of a vessel. Thus this application exceeds every thing which has hitherto been produced by the operation of our hands.*

This singularity in the operation of this remedy supposes another in the vessels, which is the great contractility of the fibres of the arteries. These indeed do naturally contract of themselves; but not to two-thirds of their diameter; nor to that state in which they are straitened by the effect of this astringent; because, by that the whole aperture is almost entirely taken off in the largest vessels; and it is easy to imagine their effects in the smallest.

It may be observed, that it is not in the dead parts of bodies, that this contraction can be made: it requires the assistance of the vital principle, and operates on the fibres by certain articles contained in it, which dispose the animal body, by its irritation, to shorten its fibres, and reduce the tissue, which they compose, into a less volume.

This remedy is nothing else but the agaric of the oak. The best kind of it is found on the parts of oak trees, where the large limbs have been cut off; and it very often resembles a horse-shoe in its shape. This agaric is distinguished into 4 parts: the rind; the 2d part, which is preferable to the other; the 3d part

* In a subsequent number of these Trans. we shall take occasion to remark that notwithstanding all that has been advanced in favour of this and other styptics, the best method of stopping a hæmorrhage when a large artery is divided, is to have recourse to a ligature.

serves for the stopping the blood in the smaller vessels, as well as that part which touches the tree. This last was what was powdered, and applied in the little bag, as in the operations of the charity.

The 2d part is what Mr. F. makes use of in amputations, cut into pieces. It must be beaten by a hammer till it is soft; and this is its whole preparation. Every part is prepared alike. The best time of collecting it Mr. Brossard has found to be in the autumn, in fine weather, after great heats.

XCV. A Letter from Benjamin Franklin, Esq. to Mr. Peter Collinson, F.R.S. concerning an Electrical Kite. Dated Philadelphia, Oct. 1, 1752. p. 565.

As frequent mention is made in the public papers from Europe, of the success of the Philadelphia experiment, for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, &c. it may be agreeable to the curious to be informed, that the same experiment has succeeded in Philadelphia, though made in a different and more easy manner, which any one may try, as follows:

Make a small cross, of two light strips of cedar; the arms so long, as to reach to the 4 corners of a large thin silk hankerchief, when extended: tie the corners of the handkerchief to the extremities of the cross: so you have the body of a kite; which being properly accommodated with a tail, loop, and string, will rise in the air like those made of paper; but this, being of silk, is fitter to bear the wet and wind of a thunder-gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp-pointed wire, rising a foot or more above the wood. To the end of the twine, next the hand, is to be tied a silk ribband; and where the twine and silk join, a key may be fastened.

The kite is to be raised, when a thunder gust appears to be coming on, and the person who holds the string must stand within a door, or window, or under some cover, so that the silk ribband may not be wet; and care must be taken, that the twine does not touch the frame of the door or window. As soon as any of the thunder clouds come over the kite, the pointed wire will draw the electric fire from them; and the kite, with all the twine, will be electrified; and the loose filaments of the twine will stand out every way, and be attracted by an approaching finger.

When the rain has wet the kite and twine, so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle. At this key the phial may be charged; and from electric fire thus obtained spirits may be kindled, and all the other electrical experiments be performed, which are usually done by the help of a rubbed glass globe or tube, and thus the sameness of the electric matter with that of lightning completely demonstrated.

XCVI. A Letter from Mr. W. Watson, F. R. S. to the Royal Society, concerning the Electrical Experiments in England on Thunder Clouds. Dated Dec. 20, 1752. p. 567.

After the communications received from several correspondents in different parts of the continent, acquainting us with the success of their experiments last summer, in endeavouring to extract the electricity from the atmosphere during a thunder storm, in consequence of Mr. Franklin's hypothesis, it may be thought extraordinary that no accounts have been yet laid before the Society of our success here from the same experiments. That no want of attention therefore may be attributed to those here; who have been hitherto conversant in these inquiries, he states, that though several members of the Royal Society, as well as himself, did, on the first advices from France, prepare and set up the necessary apparatus for this purpose, they were defeated in their expectations, by the uncommon coolness and dampness of the air here, during the whole summer. They had at London only one thunder storm; viz. on July 20; and then the thunder was accompanied with rain; so that, by wetting the apparatus, the electricity was dissipated too soon to be perceived on touching those parts of the apparatus which served to conduct it. This in general prevented verifying Mr. Franklin's hypothesis; but Mr. Canton was more fortunate, as appears by the following letter from him to Mr. Watson, dated from Spital-square, July 21, 1752.

"I had yesterday, about 5 in the afternoon, an opportunity of trying Mr. Franklin's experiment of extracting the electrical fire from the clouds; and succeeded by means of a tin tube, between 3 and 4 feet in length, fixed to the top of a glass one, of about 18 inches. To the upper end of the tin tube, which was not so high as a stack of chimnies on the same house, I fastened 3 needles with some wire; and to the lower end was soldered a tin cover to keep the rain from the glass tube, which was set upright in a block of wood. I attended this apparatus as soon after the thunder began as possible, but did not find it in the least electrified, till between the 3d and 4th clap; when applying my knuckle to the edge of the cover, I felt and heard an electrical spark; and approaching it a 2d time, I received the spark at the distance of about half an inch, and saw it distinctly. This I repeated 4 or 5 times in the space of a minute; but the sparks grew weaker and weaker; and in less than 2 minutes the tin tube did not appear to be electrified at all. The rain continued during the thunder, but was considerably abated at the time of making the experiment."

Mr. Wilson likewise of the Society, to whom we are much obliged for the trouble he has taken in these pursuits, had an opportunity of verifying Mr. Franklin's hypothesis. He informed Mr. W. by a letter from near Chelmsford in Essex, dated Aug. 12, 1752, that on that day about noon, he perceived several electrical snaps, during, or rather at the end of, a thunder storm, from no other apparatus than an iron curtain-rod, one end of which he put into the neck of a glass phial, and held this phial in his hand. To the other end of the iron he fastened 3 needles with some silk. This phial, supporting the rod, he held in one hand, and drew snaps from the rod with a finger of his other. This experiment was not made on any eminence, but in the garden of a gentleman, at whose house he then was.

Dr. Bevis observed, at Mr. Cave's at St. John's gate, nearly the same phenomena as Mr. Canton.

Trifling as the effects here mentioned are, when compared with those which we have received from Paris and Berlin, they are the only ones that the last summer here has produced; and as they were made by persons worthy of credit, they tend to establish the authenticity of those transmitted from our correspondents.

XCVII. On the Success of Inoculation at Salisbury, By Mr. Brown, Apothecary there. p. 570.

From the 13th of August to the beginning of February had been inoculated, in this city and neighbourhood, 422 persons. On 5 or 6 of these it had no effect, though on one the experiment was tried a second time. Of this whole number 4 died; one of which was a patient of Mr. B.'s, who, he thinks, did not do justice to this method; for the day on which the operation was performed, the patient's blood had been heated violently by exercise, and suddenly chilled again, by putting on clean linen, just before the operation was performed; which he apprehends, was receiving the infection in an inflamed state of blood; but with this he was not the least acquainted, till about 6 hours before the patient's death.

END OF THE FORTY-SEVENTH VOLUME OF THE ORIGINAL.

I. Of an extraordinary Stream of Wind, which shot through part of the Parishes of Termonomungan and Urney, in the County of Tyrone, on Wednesday, Oct. 11, 1752. *By Wm. Henry, D. D., Rector of the Parish of Urney.* p. 1. *Vol. XLVIII.*

The air for the whole day was serene and calm; sometimes a gentle breeze from the S. E. About 4 in the afternoon, the sky seemed to open; and there was a flash of lightning from the S. E. Half an hour after, thunder was heard as at a great distance, from the same point. About 5 the sky was a little overcast with clouds, but the air continued in a dead calm. Suddenly a violent rushing noise was heard; the sky seemed to open, and emitted a flash of lightning; but no noise of thunder; and a stream of wind instantly ensued, the violence of which nothing could resist.

This stream of wind, so far as can be traced by its effects, arose from a glin called Allgolan, and continued its course for 3 miles from S. E. to N. W. The violent current of it seemed to be confined to a space about 16 feet in breadth, and the whole body of the air in motion did not exceed 60 feet, as may be computed from some of the particulars which happened in the little village of Lisnacloon in the parish of Termonomungan, and the edge of the parish of Urney. At the distance of a mile to the S. E. of this village, it cut a line through several clamps of turf, which were standing in a bog, and threw down all the clamps in this line. Thence it crossed the river Derge, in the same line, and dashed up the water with great noise and violence. Thence, in the same line, and at the space of half a mile, it took the village of Lisnacloon, carrying away fences, the roofs of houses, and the tops of stacks. It then burst with incredible violence through a cow-house, and cut a passage of 16 feet quite through it, and carried some of the ribs of the house before it 400 yards into the field; the rest of the house was a little ruffled. A woman who was gone into the cow-house a minute before, was knocked down by one of the ribs falling. She declared that it was a dead calm the minute before; when, on a sudden, she saw a flash of lightning, and heard and felt the violent storm, but heard no thunder. A man being in the same field, but out of the line, in which the stream of wind passed, felt no wind, but heard a mighty rushing noise, and saw the timber, thatch, turf, and dust of the houses, fly by him, at the distance of 40 yards. He saw a flight of rooks dashed down in the same field. In this village are several other inhabited houses, both on the north and south sides of the course of this stream, none of which were in the least ruffled. The air continued still among these houses; and the inhabitants stood astonished, on seeing the sudden devastation so near them.

After passing this village, the stream was continued in the same line, but with less violence, to a large hill in the parish of Urney, called Muckle, and on the north side of the hill, at the distance of a mile from Lisnacloon, burst open the door of a weaver, and broke down a web in his loom. As at this last place it entered a large bog, which is extended for 3 miles, it could be traced no farther.

The time, in which this stream passed through the village of Lisnacloon, was about 5 minutes. It was succeeded immediately by a torrent of rain.

II. An Account of a Book, intitled, P. D. Pauli Frisii Mediolanensis, &c. Disquisitio Mathematica in Causam Physicam Figuræ et Magnitudinis Telluris Nostræ. Milan* 1752. *By Mr. J. Short, F. R. S.* p. 5.

It may be laid down as a rule in mixed mathematics, "That the determination of no physical quantity be carried further than the observations, or other mechanical measures, can bear;" lest there follow this incongruity, of the conclusion being more extensive than the premises. It would be absurd, for instance, in the resolution of a triangle, to compute an angle to the exactness of seconds, or a side to centesms of an inch, when perhaps the instruments used can measure no angle less than 10 minutes, or a side only to the exactness of a foot. The conclusions of arithmetic and geometry are indeed rigorously true, but they are only hypothetical; and whenever the quantities, that enter any practical question, can only be measured within certain limits, it would be in vain to look for an answer perfectly accurate. The error of the instrument becomes itself one of the data, and we must content ourselves to find the limits which the quantity sought cannot well exceed, or fall short of, by such rules as Mr. Cotes has given in his excellent treatise on the subject.

In like manner, when any physical theory is deduced from observations, its accuracy will still be in proportion to that of the observations on which it is founded. Sir Isaac Newton, in computing the ratio of the earth's axis to its equatorial diameter, confines himself to a reasonable approximation, and to 3 places of figures (229 to 230); because, whether that ratio is deduced from the different lengths of isochronous pendulums in different latitudes, or from the measurement of distant degrees of a meridian, or from both, the elements of the calculus can scarcely furnish a greater degree of exactness. And of the same judicious caution, we have many other examples in the works of that incomparable author. On the other hand, when observations and theories are brought together and compared, nothing can be justly inferred against a theory from its disagreement with the observations, unless that disagreement is greater than can be fairly imputed to the imperfection of instruments, and to the unavoidable mistakes of an observer; especially if the difference should be sometimes in excess, and at other times in defect; or, as some of the observations should entirely vanish.

Though these rules, manifestly well-founded, have been followed by all the best writers, our author observes, that several ingenious men, both in France and in Italy, have deviated from them; particularly in treating of the famous question concerning the figure of the earth. Some, with Messrs. Clairaut and Bouguer, attributing too much to the observations that have been made, and

* Paul Frisi was born at Milan about the year 1729.

taking them for absolutely exact, have concluded Sir Isaac Newton's reasonings on that subject to be faulty; while Father Boscowich, a Jesuit at Rome, making them quite loose and uncertain, thinks no argument at all can be drawn from them, concerning the earth's figure: far less in confirmation of the Newtonian theory. In opposition to these two extremes, equally contrary to reason, as they are to each other, Frisi writes the treatise now before us; in the introduction to which he shows, 1. That, though the ratio of the axis of the earth to its equatorial diameter is, from M. de Maupertuis' operations in Lapland, and afterwards in France, that of 177 to 178; and by the theory only 229 to 230; yet the difference is no more, than what might arise from a mistake of about 60 toises in the measure of either of the two degrees, that are compared, or of 30 toises in each of them. Or, suppose the measure of the arcs to be exact, the same difference might be owing to an error of 4 or 5 seconds in the astronomical part. And such errors, or others equivalent to them, in a course of so many combined operations, our author considers as difficult to be avoided. But he adds, if the observations of M. de Maupertuis, and his fellow academicians, seem to differ from the theory, those of Messrs. Bouguer and de la Condamine exactly agree with it; according to whom, a degree at the equator, containing 56753 toises, and in latitude 49° 22′ 57183 toises, the difference of the axis and equatorial diameter comes out to be $\frac{1}{229}$.

In answer to Boscowich, and those who make no account of the observations, our author allows, that if they were such as Cassini, and some other academicians, made in France, of the measure of a parallel of latitude, they could not be much depended on; that method being liable to several obvious inconveniencies. But he insists that, with the excellent instruments which were used, and considering the distinguished skill of the observers, as well at the polar circle as in France, and at the equator, the error on one degree of the meridian could not exceed 60 or 70 toises, which is a degree of exactness not only sufficient for the determination of the first question, viz. whether the spheroid of the earth is flat or long; but likewise to found an agreement between the observations and the theory, as near as can be expected or desired.

The work itself is divided into 10 chapters:

(1) De observationibus circa telluris figuram hactenus institutis. (2) De principiis et hypothesibus quibusdam. (3) De rotatione corporum, et vi centrifuga. (4) De mutationibus ex motu circulari ortis. (5) De attractione corporum rotundorum. (6) De comparatione gravitatis in variis homogeneæ sphæroidis locis. (7) De figura terræ. (8) De gradibus meridiani et parallelorum. (9) De loxodromiis nautarum, de parallaxi lunæ, et aliis ex eadem theoria pendentibus. (10) De theoriæ et observationum consensu.

In chap. 1, we have a short history of the inquiries that have been made into the magnitude and figure of the earth, down to the present times; and the preference is justly given to the measurements of Mr. Norwood in England, A. D. 1635, and of the members of the French Academy of Sciences since that time. From these he gathers, that within less than 60 or 70 toises, the lengths of a degree of the meridian are as annexed.

Lat.		Toises
0^0	$0'$	56753
45	0	57100
49	22	57183
53	0	57300
66	20	57400

Chap. 2 contains an account of the principles on which this theory is founded, viz. the universal gravitation of matter, and the diurnal rotation of the earth. Our author mentions also the hypothesis of the earth's being originally in a fluid state; but rejects it as precarious and improbable. He allows however, that with regard to the present question, it is all one whether it was first a fluid or not, seeing the ocean is circumfused just in the same manner, and to the same altitude, as if the whole was still a fluid. Chap. 3 and 4 are employed in the doctrine of centrifugal forces, and their effect in changing a fluid sphere into the form of an oblate spheroid. In the former of these chapters, the author resolves, as usual, the centrifugal force of a particle into two others; one, that acts directly contrary to the gravitation of the particle; and the other a force in a direction perpendicular to it. And this last he considers again as acting laterally on the contiguous particles impelling them towards the equator. But the quantity of this force, when greatest at the octant, he computes to be only $\frac{1}{668163}$ of the force of gravity; and therefore, says he, it may be safely neglected. In fact, after the spheroid is come to be in a permanent state, and all its parts in equilibrio, there is no longer any such lateral force at all; it being now entirely satisfied by the gradual contraction of the earth's axis.

The general contents of the following chapters are sufficiently expressed in their titles already given. Nor can we be more particular, without entering into a detail of algebraical operations, which would be improper for this place; and which is the less necessary, as the same things have been treated of by several other authors. This does not however in the least detract from the merit of Frisi, who discovers throughout this work much acuteness and skill, joined with all the candour and ingenuity that become a philosopher. And as he has not yet exceeded his 23d year, it may be expected, that the sciences will one day be greatly indebted to him; especially as we find him actually engaged in composing a complete body of physico-mathematical learning.

But there is in his 6th chapter, a criticism on one of Sir Isaac Newton's demonstrations, in which we cannot agree with him. And as this demonstration has proved a stumbling-block, not only to Frisi, but to many other learned men, we shall be obliged to consider that part of it, which has been mistaken, at some length, by the help of the scheme, fig. 1, pl. 8. In which let the ellipsis A*p*B*q*A, whose axes AB, *pq*, are in any given ratio, as of m to n, have the circles *apbq*, and APBQ, inscribed and circumscribed to it: and if the figure revolves on the axis PQ, there will be generated an oblate spheroid A*p*B*q*A, with 2 spheres, the greater circumscribed to the spheroid, and touching it in its equator ABA, and the lesser inscribed and touching it in the poles *p*, *q*; the solid content of the spheroid being the first of the 2 mean proportionals between the solidity of the exterior sphere, and that of the interior.

But if the figure revolve on the axis AB, there will be generated a prolate spheroid A*p*B*q*A, inscribed in the exterior sphere at the poles A, B; and circumscribing the interior sphere at the equator *pqp*, its solidity being the second of the above mean proportionals. So that if O and P stand for the solidities of the oblate and prolate spheroids, and S, *s*. for the two spheres; $S : O : P : s \div\!\div$ are in the continued proportion of $m : n$. And $S : P$, or $O : s :: m^2 : n^2$. As $S : s :: m^3 : n^3$.

Or we may with Sir Isaac Newton consider the genesis of these solids as follows. 1. Let the sphere APBQ be uniformly compressed in the direction of its axis PQ, till that axis is diminished to *pq*, and the sphere changed into the oblate spheroid. 2. Let this spheroid be equally compressed in the direction of that diameter of its equator, which is perpendicular to *pq* and AB, or to the plane of the figure; and it will degenerate into the prolate spheroid, whose poles are A and B. 3. Let this last be compressed in the direction of its axis AB, till it is changed into the sphere *apbq*; and, in each of these compressions, the solid space which the body contains, will be diminished in the ratio of m to n.

Now, as the determination of the earth's figure depends not only on that of the ratio of the centrifugal force, by which a body tends to recede from the axis of rotation, to the power of gravity; but also on the decrement of gravitation, arising from the body's being in that rotation actually removed to a greater distance from the centre; it is not enough that we know, from the experiments with pendulums, the centrifugal force at the equator to be about $\frac{1}{289}$ of the force of gravity. We need further two distinct propositions; one to determine the attractive force of a spheroid at its pole; and the other to determine its attraction at the equator. The first of these we have in Princip. lib. 1, prop. 91, and the second has been supplied by several authors. But Sir Isaac, who seldom does any thing in vain, found that he could, by one of his artifices, make that 91st proposition serve likewise to determine the attraction at the equator, by the following argument.

Let G be the attraction of the exterior sphere at A; and let the decrement of that attraction, when the sphere is diminished into the oblate spheroid A*p*B*q*, be d; and δ the decrement of this last attraction, when the oblate spheroid is diminished into the prolate, whose poles are AB; then is d nearly equal to δ; the difference of the axes of the generating ellipse being small. For the attractive matter that is taken away, has, in both cases, the same ratio to the matter that is left; and its position, with respect to that which is left, is in both cases nearly the same; and therefore the successive attractions will be nearly in continued proportion, $G : G - d : G - d - \delta$ ∺. Or multiplying and rejecting d^2 as inconsiderable, $Gd = G\delta$, and $d = \delta$.

Thus, if the attractions of the sphere APBQ, and of the prolate spheroid, at its pole A, be 126 and 125 respectively; the attraction of the intermediate oblate spheroid at its equator will be $125\frac{1}{2}$: and how nearly this approaches to the truth, may be seen from an exact computation of those attractions. For if the axes of the generating ellipse be 101 and 100, and the attractive force at the surface of the sphere 126; the attraction at the pole of the prolate spheroid will be 124.9838; and that at the equator of the oblate 125.5077; which exceeds the arithmetical mean between the two former, only by .0068; that is, by about $\frac{1}{18410}$ part of the attraction of the oblate spheroid at the equator.

This reasoning is more shortly expressed in the Princip. lib. iii, prop. 19, as follows. "Gravitas in loco A in sphæroidem, convolutione ellipseos A*p*B*q* circa axem AB descriptam, est ad gravitatem in eodem loco A in sphæram centro C radio AC descriptam, ut 125 ad 126. Est autem gravitas in loco A in terram media proportionalis inter gravitates in dictam sphæroidem et sphæram; propterea quod sphæra, diminuendo diametrum PQ in ratione 101 ad 100, vertitur in figuram terræ; et hæc figura, diminuendo in eadem ratione diametrum tertiam, quæ diametris AP, PQ perpendicularis est, vertitur in dictam sphæroidem; et gravitas in A, in utroque casu, diminuitur in eadem ratione quam proxime."

In which the expression "eadem ratione" occurring a second time has misled F. Frisi and others, to think that this last ratio is also that of the axes, or of 101 to 100; whereas the identity of ratios here asserted, is to be referred only to the words "utroque casu;" the ratio itself being not that of the axes, or of m to n; but the half of that ratio (whatever it is found to be by prop. 91, lib. i) which the attraction of the sphere has to the polar attraction of the inscribed spheroid.

This inadvertence, however, of his own, Frisi charges on Sir Isaac Newton; and files it up, as the 6th of the errors, which he says have been discovered in the Principia.... "Ita dum stabilitæ in 19 lib. 3 propositioni terrestrium axium proportionis fulcimentum et patrocinium quærimus, aliud in propositione eadem sophisma sese offert, quod eorum, quæ in principiis mathematicis Newtoni nacta (i. e. detecta) sunt hactenus, sextum est, &c." But we may take it off the file again; and for the present leave the other 5, till they are considered of at more leisure.

In his 10th and last chapter, our author sums up the evidence, and finds that all the good observations that have been made, as well by pendulums as by actual mensuration, concur with the theory, in making the ratio of the earth's axis and equatorial diameter to be as 229 to 230. This is indeed a sufficient confirmation of the theory of gravitation: but it must be observed, that the coincidence is not perhaps quite so perfect as Frisi imagines. That ratio corresponds well enough to the exactness to which the first elements of the calculus can be obtained; the length of a second pendulum, and that of the earth's equatorial diameter, from which the centrifugal force ($\frac{1}{289}$) is deduced. But, if we suppose that force to be accurately $\frac{1}{289}$, and compute more rigorously, we shall find the ratio in question to be very nearly that of 225 to 226; agreeing still with the observations as well as can be desired; and showing, at the same time, the inimitable art of Sir Isaac Newton in the contrivance and use of approximations; seeing the strictest calculation raises the equator not the third part of a mean geographical mile above what he had found by his method.

I sent, says Mr. Short, Frisi's book to my learned friend the Rev. Mr. Murdock, who has fully considered the question concerning the figure of the earth; and who, after having perused the book, and discovered the above mistake of Frisi, sent me the above theorem, and its demonstration. He likewise sent me the following theorems, which, he says, he had communicated to M. de Bremond, in the year 1740, when he was translating his treatise on sailing: but M. de Bremond dying soon after, those who had the care of publishing the translation, printed it incorrectly in several places; particularly the theorems for the prolate spheroid: on which account, he says, if they are thought worth preserving, they may be inserted in the Phil. Trans.

Postscript.—Theorems for computing the ratio of the attractive force of a spheroid, at its pole or equator, to that of the inscribed sphere.

1. In an oblate spheroid, the ratio is,

$$\text{Pole } 1 + \frac{1}{m^9 - 1} - \frac{m^2}{(m^2-1)^{\frac{3}{2}}} \times \text{A} : \tfrac{1}{3}$$

$$\text{Equator } \frac{m^2}{(m^2-1)^{\frac{3}{2}}} \times \text{A} - \frac{1}{m^2-1} \quad : \tfrac{2}{3}$$

2. In a prolate spheriod, the ratio is,

$$\text{Pole } 1 - \frac{m^2}{m^2-1} + \frac{m}{(m^2-1)^{\frac{3}{2}}} \times l : \tfrac{1}{3}$$

$$\text{Equa. } \frac{m^2}{m^2-1} - \frac{m}{(m^2-)^{\frac{3}{2}}} \times l : \tfrac{2}{3}$$

In which $m : 1$, as the greater axis of the generating ellipse is to the lesser. A is a circular arc, to the radius 1, whose tangent is $\sqrt{m^2 - 1}$, or its reciprocal if $m^2 - 1 \sqsubset 1$. And l is the natural logarithm of $\frac{s}{v}$; s being the sine of the arc, whose co-sine is $\frac{1}{m} \times \sqrt{m^2 - 1}$, and v the versed sine of the same arc.

Note, The first two theorems, by substituting t for $\sqrt{m^2 - 1}$, coincide with those of Mr. Maclaurin for the oblate spheroid, in his dissertation on the tides.

III. Concerning the Year of the Eclipse foretold by Thales. By the Rev. Mr. George Costard, Fellow of Wadham Coll. Oxford. p. 17.

Riccioli supposes that the eclipse foretold by Thales happened the year A.C. 585;

and quotes both Theon and Cleomedes in confirmation of the opinion. Theon perhaps had Cleomedes's words in view; but neither of these authors have circumstances enough to determine what eclipse in particular they meant. The passage of Theon is in his chapter concerning the moon's parallax, where he says that Hipparchus, being in doubt whether the sun had any parallax at all, supposed, in the first book of his treatise concerning magnitudes and distances, that the earth, in respect of the sun, was only a point; whence, by means of an eclipse there set down by him, he framed two distances of the sun, a less and a greater. All then that is here said is, that the eclipse made use of by Hipparchus, was at the Hellespont; but at Alexandria in Egypt a little more than 5 digits only. But he has neither given the æra of Nabonassar, the place of the luminaries, nor any one circumstance besides, by which we might form any conclusion what year this eclipse was in.

Cleomedes, who perhaps saw the same treatise of Hipparchus, is as uncircumstantial as Theon. He says only, that the diameter of the moon's shadow at the earth is something more than 4000 stadia. By the quantity of obscuration he mentions, this seems to have been the same eclipse with that quoted by Theon from Hipparchus; but as the place of observation in both these authors appears to have been Alexandria in Egypt, it must have been after that place was built. Consequently it was probably observed there by Hipparchus himself, and therefore could not have been the eclipse foretold by Thales. Besides, if this eclipse was total on the banks of the Hellespont, I know not what reason there is for supposing, that the battle between the Lydians and the Medes was fought there. It should rather seem, that the engagement was on the confines of the two kingdoms: consequently in a more southern latitude, and in a longitude more to the east of Alexandria, this eclipse could not have been total; nor therefore (as Herodotus said it did) turn day into night.

Sir Isaac Newton, in his chronology, likewise supposes the eclipse meant to have been that in May, A. C. 585. But in this perhaps he rather follows others, than adopted it after any examination of his own. That treatise never had the finishing hand of its great author, and it is well known now in what manner it came abroad.

According to Riccioli, this eclipse was central at the Hellespont, and at Sardes fell out at 6 in the afternoon; and therefore is rejected by Mayer, in the Petersburg acts, as being too late in the day.

According to my computation, the apparent time of the true conjunction was at Greenwich, May 28, $4^h\ 35^m\ 15^s$; the beginning of the general eclipse $2^h\ 3^m\ 30^s$; the end of the same $7^h\ 1^m\ 46^s$. And by calculating the path of the penumbra's centre over the earth's disk, it pretty plainly appears, that the centre of the shadow passed so far from any place, where we can reasonably suppose the battle between the Lydians and Medes to have been fought, that this can hardly have been the eclipse mentioned by Herodotus.

Father Hardouin, in his chronology of the Old Testament, rejects this eclipse, as not happening, he says, in the reign of Cyaxares, but in that of Astyages; not on the 4th year of the Olympiad, but a month before it began; as falling out too late in the day; the greatest obscuration being scarcely half an hour before sun-set; and not total or central, or $12^{\circ} 56'$ digits, as Riccioli makes it, but almost 9. Though Pliny therefore says this eclipse was Olymp. XLVIII, 4, and A. V. C. CLX, yet six MSS. he observes, in the French king's library, have CLXX, and so most printed copies. He thinks therefore, that instead of CLXX, the number should be CLVII, which he says is Olympiad XLVIII, 4, and the year before Christ 597; when there was an eclipse of the sun, on Wednesday July the 9th, at 6 o'clock in the morning.

This eclipse Petavius also prefers; though he makes the digits eclipsed only $9{,}22'$: which is strange enough, as it could not have been by any means the cause of such a darkness as is described by Herodotus. But F. Hardouin supposes, that this battle was fought on the banks of the river Halys in Cappadocia, and in latitude north 40°; where, says he, this eclipse must have been central and annular.

According to Dr. Halley's tables, the year before Christ 597, the apparent time of the true conjunction at Greenwich, was July $8^{d}\ 21^{h}\ 50^{m}\ 9^{s}$; the beginning of the general eclipse $19^{h}\ 8^{m}\ 16^{s}$, and the end $9^{d}\ 0^{h}\ 49^{m}\ 2^{s}$.

And from the course of the centre of the penumbra, it appears that this eclipse, at Sardes, or any where else where we can suppose this battle to have been fought, could not have been great enough to turn day into night; and therefore does not answer the description of Herodotus.

Archbishop Usher rejects both these eclipses, as inconsistent with his chronology; and supposes that intended to have been A. M. 4113, An. Nab. 147, before Christ 601, Olymp. XLIV, 4. Sunday July 20 $3^{h}\ 25^{m}$ before noon, digits eclipsed 9. But this also is greatly defective as to quantity. But though this is insufficient for the purpose, yet there was one 2 years before this, or the year before Christ 603, that will be found by good tables entirely satisfactory. Petavius indeed makes the digits eclipsed only $7.20'$; but, according to Dr. Halley's tables, the apparent time of the true conjunction was at Greenwich, May $17^{d}\ 20^{h}\ 42^{m}\ 19^{s}$. The place of the luminaries $1^{s}\ 19^{\circ}\ 12'$, and the moon's latitude north $25'\ 17''$.

Beginning of the central eclipse	$19^{h}\ 13^{m}\ 27^{s}$
End of the central eclipse	22 3 47

And if modern maps and geographers may be depended on, the centre of the shadow passed over the kingdom of Barca and Africa, and crossed the Mediterranean between Candia and Cyprus, and then over Antiochetta and Erzroum, and a little to the south of Kars. Which agrees with what is said in the Petersburg Acts, p. 332. If any allowance is to be made for the moon's acceleration, or any other cause, the track here given will be a little different. As Mr. C. cannot make several ancient eclipses, that he has tried, succeed to his mind, without some such supposition, he has done the same with regard to this, viz. 45^m, at Mr. Whiston's rate of 1^m in 54 years, or thereabouts. Then agreeably to this he finds, that the centre passed more to the south than the former, and went near Tripoli, Aracta, Nisabin, and Ardbil.

It is much to be wished, that Herodotus had told us where this battle was fought; that by this means we might have known which of these 2 paths to have preferred. However, as he has not, and there is nothing in either of them that is inconsistent with the history, Mr. C. concludes, from a number of other circumstances besides, that this really was the eclipse foretold by Thales. He was not a little pleased on looking into his papers, to find that Bayer and he agreed so exactly in the very year, and he was a stranger to what he had said on that subject, till he saw his account in the volume of the Petersburg Acts.

IV. The Case of Anne Elizabeth Queriot, of Paris, whose Bones were Distorted and Softened. By Ambrose Hosty, M. D. of Paris. p. 26.

Anne Elizabeth Queriot, aged 35, native of Paris, was married in the year 1746, was brought to bed in 1747, and for the first time complained of great weakness in the small of her back, loins, and thighs, and could scarcely walk.

A 2d lying-in, a year after, removed her complaints for about 6 weeks; after which they returned. In the year 1749, being $2\frac{1}{2}$ months with child, she was seized with a loss of blood, and miscarried. Two months after, she fell on her left side; which gave her great pain in the leg, thigh, and hip of that side, and made them swell: but there was neither fracture nor dislocation. Her pains after some time abated; but the weakness of her limbs continued. She was a 3d time with child, and, in the beginning of her pregnancy, had a 2d fall; which revived her former pains, and caused new pains all over her body, with a swelling, as before. This confined her to her bed, yet her pregnancy terminated favourably, after which the swelling went off; but her limbs were so weak, that she could not bear upon her feet.

In about 6 months after her last lying-in, her pains returned worse than before; and about the same time, an abundance of white chalky sediment appeared in her urine; and the fore-finger of her right hand was observed to be distorted towards the little finger: which was the first appearance of the dissolution that ensued. Soon after the lower extremities began to turn upwards gradually, and almost in a parallel line with her body, and continuing, till in 9 months her lower limbs were turned upwards. All the bones were affected, especially the thorax, which had lost its natural form and capacity, and she was altogether miserably distorted.

This miserable state was attended with exquisite pains; and, according to the seat of them, the patient used to say, 'Now such a part works.' Sometimes they abated, and then she felt so sore, as not to bear being touched: and during this ease from her pains, a quantity of the aforesaid sediment passed by urine, though little or none in her sufferings. It was quite cretaceous, and, reduced into a fine powder, fermented gently with acids. She could bear no covering, but a few napkins, both from inward heat, and to avoid loading her breast. Notwithstanding her preternatural posture, the evacuations by stool and urine were regularly and easily performed. Her flesh seemed dead and œdematous, the skin rough and scaly; so that a mortification was often apprehended. She had a cough, a laborious respiration, and sometimes a spitting of blood, from the coarctation of her breast, all its bones plying inwardly. She was capable of no other motion than turning her head on both sides, stirring her left arm in the shoulder-joint only, and separating her fingers, but not bending them. She had her menses regularly, till about 3 months before her death. She generally had a low fever, inward heat, sweats, and restlessness. Her fever ran very high in August, attended with delirium, headach, raving, and subsultus tendinum. A little before her death, came on a deafness, a dimness of sight, a scalding of her eyes, and a constant dropping; violent pains in her head; in short, a great weakness in all the organs, which showed how much the head was then affected.

The distortion of her limbs went on so fast in August and September, that almost every 3d day something new was observed; especially the left foot, during that time, came down gradually near 18 inches from under her ear, where it lay before. It was also observed in August, that her neck grew visibly smaller, the thorax much narrower. And then the napkins, on which she spit, grew black in the washing, and stained as from the mercurial ointment, though Dr. Hosty could not suspect it, as he could not learn she had ever used any mercury. In a month after, he observed the same thing on all the linen, that touched her skin. He got a napkin rubbed with soap, then dried, and afterwards washed. This method had almost taken off the stains, as it does those from the mercurial ointment. Her linen stained all the washing, like linen impregnated with it. Those spots appeared on the linen a mixture of a cretaceous matter and grease.

Since this remark was made, none of the white sediment was seen. This, and the apparent nature of the stains, made him believe that it was then discharged by spittle, and the pores of the skin, and mixed with oily particles of her fluids, which had acquired a quality analogous to that of mercury, of staining all linen. He was also apt to think, that this sediment was the earthy matter, that gives the bones their solidity and hardness, which had been dissolved by the same vitiated quality of the fluids, and evacuated by the emunctories already mentioned.

After great sufferings, she died the 9th of November. Her body was opened in the presence of some of the most celebrated anatomists and academicians of Paris. The operation was begun on the left tibia, cutting on the fore part of it, from below the knee to its basis. It was wonderfully altered; more or less soft in all its length; in some points entirely dissolved, and its sides not thicker than the gristle of the ear. The spongy substance of its extremities supple, yielding to the least pressure. The reticular matter was quite destroyed. The peroné was entirely dissolved in the middle, and only slight marks of its extremities remained. Instead of marrow, they found in all the bones a red thick matter, like coagulated blood mixed with grease. The rotula was entire, but very soft and spongy; the condyles of the femur the same. All the cartilages were found in their natural state. The head of the humerus was much diminished and flattened; its middle part very small, pliable softened in all points, yet in some friable. The cubit and radius suffered the same alterations with the humerus. By stretching all her limbs they laid them straight; but they soon after returned to their former curve. The phalanges of the fingers were not so much softened, but were easily cut, and bent like whale-bone. The femur was rather a fleshy body than a bone; its cavity was filled with a reddish suet, instead of marrow, which, accumulated in different points, bulged out the fleshy sides. The capacity of the pelvis was much diminished; the bones, that compose it, were softened, thickened, and contracted. The spine kept its natural form; the vertebræ soft and supple. The sternum, and all the cellular bones, seemed solid, but could bend, and were easily cut. The ribs, though softened, were still friable. Some of them, towards the sternum, were doubled over each other. The clavicles seemed almost cartilaginous. The shoulder-blades were much thicker than natural, less broad, and entirely disfigured. The 2 protuberances called acromion and coracoides almost joined. The skull-bones were easily cut in slices, and were twice as thick as in their natural state. Both plates were joined in one, and no traces at all of a diploe. Their substance abounded with an extremely diluted serum, easily squeezed out by a gentle pressure of the fingers. The sutures almost obliterated: the bones of the basis and face shared in the calamity. The teeth hard as usual. The dura mater was incorporated with the bones. The brain not softer than ordinary: its right hemisphere was by one-third larger than the left; and hence, perhaps, the weakness of her left side, often manifested by pains, achs, defluxions, heaviness, falls on that side, and every illness which she had from her infancy, beginning in some part of it. When young, she fell on her head down 2 pair of stairs. The membranes, that separate the 2 hemispheres of the brain, were much thicker than commonly.

In fine, all her bones were so soft, that the scalpel with very little force ran through the hardest of them; even the rocky apophysis of the ear-bone, so called from its excessive hardness. Nothing extraordinary was found in the viscera; but their size diminished by the compression, and a universal cachexy.

There could be no cause assigned of this woman's disorder, as she gave no signs plain enough to prove either a scurvy, pox, or king's-evil, either hereditary, or acquired; her parents having lived healthy, the one to the age of 80, and her mother being then alive, aged 60, and in good health. She had 3 children, who died of disorders common to their age. One, 4 years old, died of the measles.

This, it is added, is a rare case, but there have been some similar cases, which are cited in the Abridgment of the Phil. Trans. in the remark upon the like case presented to the R. S. by Mr. Silvanus Bevan. This differs from the other examples, by the sediment of the urine, the stain on her linen, the preternatural situation of her limbs. Something very singular was, that she did not blow her nose perhaps once a month, even in her health; always slept with her mouth opened, and her tongue hanging out. The manner in which such dissolutions of bones are accounted for, in the above-mentioned remark, seems the most rational and satisfactory, that can be given.

V. Of a Roman Altar, with an Inscription on it, lately found at York, and communicated to the Society of Antiquaries by Mr. Francis Drake, F.R.S. Also a Brief Explication of the Inscription by John Ward, LL.D., and V.P.R.S. p. 33.

This altar was found, with other remains of antiquity, by some workmen, in opening a deep drain down the centre of a large street, called Micklegate, in the city of York. Its height with the pedestal, on which it stands, and which is made hollow to receive it, is 14¼ inches. But the breadth varies in several parts of it, according to their different form. On the top is an apex, with a volute on each side, and on the front a pediment over the inscription. It is elegant for the workmanship, and well preserved. Mr. Drake has sent up a draught of it in its just proportion, with the inscription upon it; as also another copy of the inscription, taken off from the stone, by pressing wet paper into the letters, and then delineating both them and the stops with a pencil. The inscription itself in words at length, as Dr. W. thinks it may be read, is as follows:

Matribus Africis, Italicis, Germanicis,

Marcus Minucius Ande.
miles legionis sextæ victricis,
gubernator legionis sextae,
votum solvit libentissime merito.

By this inscription, and many others of the like sort, found in Britain and other countries, it appears that these female deities, under the name of Matres, were worshipped in most parts of the Roman territories. Writers are not agreed in their opinion concerning these female deities, who were worshipped under the character of Matres. Spon supposes they were deified women, who, while living, were thought to have the gift of prophecy. The reasons for which opinion have been given at large in Horsley, p. 201. But Selden applies them all to the Dea Syria, or Mater Deorum; whom St. Augustin, as he observes, takes for Juno, and says, Tot esse Junones, quot sunt simulacra. Agreeably to which notion, we find several altars in Gruter inscribed Junonibus, in the plural number. And Plutarch takes notice of the worship paid to the Deum Matri at Enguium in Sicily; which Cicero seems to allude to, when he says, Matris magnæ fanum apud Enguinos est. As we meet with several inscriptions, which have on them the title of Matronæ, to whom they are addressed, Spon thinks, not improbably, that these Matronæ were the same deities as the Matres, or Matræ, as they are sometimes written, who were indifferently worshipped by each of those titles, of which he has given a variety of instances.

The first two names of the person who dedicated this altar, were doubtless Marcus Minucius; but the third, as here abbreviated, is uncertain. This Marcus Minucius describes himself by 2 characters or employments; first, as a soldier of the 6th legion, which was honoured with the title of victrix; and then as pilot of the same legion, the epithet victrix not being repeated the 2d time, as unnecessary. The title of gubernator, or pilot, Dr. W. does not remember to have met with in any other Roman inscription. And notwithstanding the 1st line is placed at some distance from the rest; yet it may, he thinks, connect with them, without supplying the word sacrum. It appears, by an inscription in Gruter, and republished by Dr. Gale, that this legion was transported from Germany to Britain in the reign of the Emperor Adrian, under the command of Marcus Pontius; who is there called, Tribunus militum legionis sextæ victricis, cum qua ex Germania in Britanniam transiit. The inscription therefore on this altar at York, may refer to that voyage; and intimate to us, that Marcus Minucius, by whom it was erected, was then pilot to the legion. It is probable indeed, as Horsley observes, that on its first arrival it made no stop in the south, but marched directly by the usual route to Adrian's vallum; since there are several inscriptions on and near the wall, both in Northumberland and Cumberland, where this legion is mentioned. And in the following reign of Antoninus Pius we find, by several other inscriptions, that it was in Scotland, and had a share in building the wall there. Not long after it might very probably be stationed at York, where Ptolemy places it, who lived under the next emperor Marcus Aurelius, as we learn from Suidas. The legion therefore being thus settled, Marcus Minucius might then think it a proper time to pay his vows, formerly made to those deities, whom he addresses in the inscription cut upon this altar.

VI. Of several Persons seized with the Jail-fever, working in Newgate; and of the Manner in which the Infection was communicated to one entire Family. By John Pringle, M.D., F.R.S. p. 42.

In the month of October 1750, a committee of the court of aldermen was appointed to inquire into the best means for procuring in Newgate such a purity of air, as might prevent the rise of those infectious distempers, which not only had been destructive to the prisoners themselves, but dangerous to others, who had any communication with them; and particularly to the courts of justice on the trial of malefactors; of which a fatal instance had occurred that year at the Old-Bailey sessions, when the lord-mayor, 2 of the judges, and an alderman on the bench, with several other persons then present, were seized with a malignant fever, and died.

The Rev. Dr. Hales and Dr. P. being consulted by the committee on the point referred to them, and having visited the jail in company with those gentlemen, it was agreed, that, considering the smallness of the place, in proportion to the number of the prisoners, it would be proper to make a further trial of the ventilator, and to have it worked by a machine, in the manner of a windmill, to be erected for that purpose on the leads of Newgate.

This scheme was laid before the court of aldermen, and approved of, but not put in execution till near 2 years after. For on the 11th of July 1752, Dr. Hales acquainted Dr. Knight and Dr. P. that several of the tubes were finished, and that the machine had been going about 6 weeks; therefore, being desirous to see the effects, he had appointed Mr. Stibbs the carpenter, employed in that work, to meet him that day at Newgate, and desired him to go along with them. They went accordingly, and having visited several of the wards, they were all very sensible, that such as were provided with ventilating tubes were much less offensive than the rest that wanted them; and Dr. Hales and Dr. P. could perceive a considerable improvement in the air of the whole jail, since the time they had been first there with the committee. Some of the wards were so free from any smell peculiar to such places, that Dr. P. was persuaded, were Dr. Hales's design completed, and a person appointed to regulate the sliders of the tubes, and to keep the machine in order, the usual bad consequences from foul and crouded jails, might in a great measure, if not wholly, be prevented in Newgate.

One of the wards allotted for the women had a small room adjoining to it, in which they usually slept. Both places seemed at that time well aired, though the latter was close, and, he thinks, without either window or chimney. The prisoners informed them, that before this ward received the tube, this sleeping-place had been very offensive, but that soon after it became sweet; and though on the first working of the ventilator they had been more sickly than before, they soon recovered their health, and had preserved it ever since. Now from this account it must not be inferred that any danger will arise from a sudden change of bad air for good; since this accident may be better accounted for from another circumstance, which they were then likewise told of; viz. that this ward of the women had been supplied by a ventilating tube before those in the lower story, where the air being in a more corrupted state, it had passed from thence through the seams of the floor, and other passages, to replace that, which was drawn off by the tube in the ward above: but that after the bad air was exhausted, the benefit of the fresh air soon appeared, by the better health of the prisoners.

But as it was not his design in this paper to set forth all the advantages that might be expected from the ventilator, he would leave that subject to be treated of by the ingenious inventor of it; and would only take notice, that the tubes from the several wards, uniting in one great trunk, convey all the putrid steams by that channel into the atmosphere, through a vent made for that purpose in the leads of Newgate; and that though the wind was moderate during the time they staid in the jail, yet they observed, that the ventilator threw out a considerable stream of air, of a most offensive smell. Before they parted, Mr. Stibbs informed them, that Clayton Hand, one of his journeymen, while he was employed in setting up the tubes, was seized with a fever, and carried to St. Thomas's Hospital, after lying some days ill at his own house. Apprehending that this man's sickness might be owing to the air of the jail, Dr. Knight and he having the curiosity a few days after to go to St. Thomas's to make the inquiry, they found the patient sitting in one of the courts, recovered of his fever, though still weak, and he gave this account: that on first finding himself indisposed, he had left off work for some days; but on growing better he had returned to Newgate. That soon after happening to open one of the tubes of the old ventilator, which had stood there for 3 or 4 years, such an offensive smell issued from it, that being immediately seized with a nausea and sickness at his stomach, he was obliged to go home, and that the night after he fell into a fever, in which he lay about 8 days before his friends carried him to the hospital. That becoming soon delirious, he recollected no other symptom, succeeding these mentioned, besides frequent retchings to vomit, a trembling of his hands, and a constant head-ach. This man had taken no medicine before he came into St. Thomas's, and since that time was attended by Dr. Reeves; but as that gentleman was not then present, they were informed by the apothecary, that Clayton Hand had been admitted in the advanced state of a continued fever, attended with a stupor, and a sunk pulse, and that the fever had not left him till several days after his admission. The nurse's account was, that he had all along lain like one stupified, and that after the fever went off, he had continued for some time very dull of hearing. They could learn nothing certain about the precise duration of the fever, but from what the patient and his attendants told them, they collected, that he must have been ill between 2 and 3 weeks. So that from all these marks, they made little doubt, but that this person had been ill of the true jail-distemper; and were confirmed in their opinion by the following circumstance.

In company with the convalescent was one Thomas Wilmot, another of Mr. Stibbs's journeymen, who had likewise worked in Newgate, and whom they remember a few days before to have seen in that place, very active, and in perfect health. This man told them, he had come to see his companion, but as he apprehended himself in danger of falling into the same fever, he should therefore be glad of their advice. On examination they found his tongue white, his pulse quick, and that he complained of a pain and confusion of his head, with a shaking of his hands, and a weakness in his limbs. He said his disorder had come on gradually, since the time they saw him in Newgate, but that he was then so very ill, he could work no longer. From which account it appeared to them, that this man had also caught the infection; but as the fever seemed not to be quite formed, they had hopes of stopping its progress: and with this view they advised him to take a vomit, and on the following night a sudorific. He followed the prescription, and the effects shall afterwards be mentioned. After Wilmot had told them his own case, he informed him of the indisposition of 3 more of his companions, who had been likewise employed by Mr. Stibbs in Newgate: on which they took their direction, visited them, and found them all ill of the jail-distemper. The first was Michael Sewel, who lodged in the Swan-yard near Newgate. This man had been 10 days confined to his bed, without taking any medicine. He was then delirious, and had the petechial eruption: but observing that he lay in a close, ill-aired, and dirty room, without any attendants but his wife, then suckling a child, they believed he had no chance to recover where he was, and therefore recommended his case to Mr. Stibbs, who procured his admission that day into St. Thomas's Hospital; where he also recovered.

The 2d was Adam Chaddocks, who lay at a green-shop in the little Old-Bailey. He was taken ill on the same day with the former, and had used no medicine.

He had likewise the petechial spots on his breast and back; and though he was not altogether insensible, was affected with a stupor attended with a sunk pulse, and other symptoms of the distemper. His landlady, who took care of him, informed them, that he had been troubled with retchings to vomit, and a head-ach from the beginning, and that for some days past he had been seized with a looseness, and that his stools were very offensive. As the room this person lay in was large and well aired, they did not think it necessary to remove him, but recommended him to the care of Dr. Pate, physician of St. Bartholomew's hospital, who attended him till he recovered.

The 3d was John Dobie, apprentice to Mr. Stibbs, a lad about 15 years of age, who lived with his parents in a court by the White Bear in Canon-street. They saw him on the same day with the other two, which was the 14th of his sickness, and the 12th since he took to his bed. His mother told them, that some of the journeymen working in Newgate had forced him to go down into the great trunk of the ventilator, in order to bring up a wig, which one of them had thrown into it; and that, as the machine was then working, he had almost been suffocated with the stench, before they could get him up. That on coming home he complained of a violent head-ach, a great disorder in his stomach, with retchings to vomit, which had never entirely left him. They found him extremely low, with a sunk pulse, a delirium, and an unusual anxiety or oppression about his breast. This last symptom they ascribed to the opiates he was then taking for a looseness, which had come on 2 or 3 days before they saw him. He being in no condition to be moved, and being besides well attended by his mother, and in a well aired chamber, they prescribed to him there, and repeated their visits, till he was quite free of the fever. It was observable, that before he was taken ill, he had been twice let down into the great trunk of the ventilator, when the machine was standing still, without complaining of any ill smell, or receiving any hurt; but that the last time, when the machine was working, he immediately cried out he was ready to be suffocated; and the 2 men who helped him out, by receiving the foul steam from the trunk, were both set a vomiting so violently as to bring up blood.

On the 23d of August, Thos. Wilmot, above-mentioned, called on Dr. Knight, and told him, that after taking the vomit and sudorific, he had immediately recovered; but begged him to see his wife, who then lay ill of a fever, at his house in Snow's-fields, Southwark. The doctor suspecting that this woman's indisposition might be owing to the contagion received from her husband, acquainted Dr. P. with it, and carried him to see her. There they were informed, that Wilmot's daughter, a girl of 8 years old, who lay with her parents, had been seized with a fever, soon after her father's recovery; that she had been ill about a fortnight, and they believed had spots on her breast, but that she had recovered without any medicine. That her mother had not only nursed her, but continued to lie with her; and that some time after the girl's recovery, the mother began to complain, and soon after fell into a fever; and that it was the 12th day since she was confined to her bed. This woman having the petechiæ, a stupor, with deafness, and a sunk pulse, there was no doubt of her being likewise infected with the distemper, and probably by her daughter. As she had been without any assistance, they advised her husband to send for Mr. Breach, apothecary, in the Borough, who having served in the hospital of the army during the war, was well acquainted with the nature of such fevers: and having left directions with him, they did not return till after the crisis; which happened on the 16th or 17th day from the time she was confined to her bed.

Some time after this, Mr. Breach the apothecary informed them that he was again employed in Thomas Wilmot's family; for that Eliz. Marshall, his sister-in-law, after nursing his wife, was taken ill of the same kind of fever, and desired their assistance. This person they found in the same bed, and in the same condition, in which they had seen her sister some time before; and in the room with her, in another bed, a son of Wilmot's, a boy of 9 years old, ill of the same distemper. The former had been attacked on the 15th of September, and the latter the day before. The woman's fever ran out the ordinary length of 16 or 17 days, but the boy's came some days sooner to a crisis, and was all along of a milder nature. She recovered very slowly, complaining of great weakness, deafness, and a confusion in her head, the ordinary consequence of these malignant fevers.

One day, in his return from this house, Dr. P. called at St. Thomas's hospital, to inquire for one William Thomson, a lad of about 16 years of age, who, as Wilmot then told him, was another of Mr. Stibbs's journeymen, and had been taken ill by working in Newgate, since the 3 he had mentioned before. This lad was recovered, but not yet dismissed. He said, that on finding himself growing ill, he had left his work, and kept at home for about a week, complaining of a pain in the hinder part of his head, and his back, of a trembling of his hands, and of restless nights; that his feverish indisposition increasing, he had been obliged to take to his bed, where he lay about 8 days before he was sent to the hospital. The apothecary added, that he had continued under their care about the same number of days before the turn of his fever; that his pulse had been extremely low all that time, and that they believed him to be in the utmost danger. He added, that the wife of Michael Sewel (the second patient they had received, of those who had been employed in Newgate) some days after her husband's admission, came to seek advice for herself, and that her complaints had been the same with Wilmot's at the time they saw him: he added that he had given her some medicines, but had heard nothing of her since.

On the last day of December Mr. Breach informed Dr. P. that about a month before, he had been called to attend Thomas Wilmot; but as he died before he saw him, he could give no other account of his sickness, than as they told him, viz. that he had long been in a bad state of health, and that at last he became feverish, and went off with a looseness.

In the beginning of this month (January 1753) the widow applied to Dr. Hales and Dr. P., in order to have the distress of her family attested, and laid before the lord mayor, in hopes of having some provision made for them. On which occasion they learned, that Thomas Wilmot her husband, after taking the sudorific, so far recovered as to work at his business; but though he did not return to Newgate, yet his strength would not permit him to continue at work above a day or two at a time; still complaining of a head-ach, and pains across his breast, or, as he expressed it, about his heart, of a feebleness of his limbs, a shaking of his hands, and a constant drought. That notwithstanding these ailments, he went daily, till a week before he died, when he grew very weak, and more feverish, had sometimes profuse sweats, and at other times a looseness, and that both these excretions, and also his breath, were remarkably offensive. That at last he was seized with convulsions, and having 3 fits in one day, he died in the last of them. Mrs. Wilmot added, that her youngest son James, a boy of 4 years of age, was after the father's decease seized with the spotted fever, of the same kind with what had prevailed in the family, but that he recovered; and that her own mother Eleanor Megget, who did not live in the house, but came often to see and attend them, was also taken ill of a fever, but without spots, and died about 10 days after her husband. She concluded with telling them, that the distress of her family had been the greater, by her being deprived of all assistance from their neighbours, who having thus seen the whole family, one after another, seized with this fever, were as much afraid to come near them, as if they had been infected with the plague.

Besides these 6 persons, that were taken ill by working in Newgate, and whom Dr. P. saw, there was another, called Rust, as Mr. Stibbs informed him, but whom he never visited. So that, besides Wilmot's whole family, and Sewel's wife, who received the contagion at second-hand, there were 7 originally infected in the jail, out of 11 only, who were employed by the master-carpenter in that place. Now as most of these 7 were attacked within a few days of each other, and by the same kind of fever, it is not to be doubted but that the distemper was owing to the corrupted air of Newgate. From all which it appears, how requisite it is, that the public should take such measures, as may prevent the like accidents arising from foul and crouded jails; or indeed from any place, where a multitude of people are long, closely, and nastily kept; and which in all probability can never be obtained without a constant change of air.

Then follows a letter from Mr. Stibbs, stating the number of men seized with the jail fever, and what proportion they bore to the whole number, employed by him in setting up the ventilator in Newgate.

In this letter Mr. Stibbs gives the names of all the workmen that worked on the ventilator in Newgate, and how many had the jail distemper. One of the principal men, whose name was Wilmot, died, and all the rest recovered. He was informed by the person, whose name was Jackson, that took care of the windmill and ventilators, that but 1 person had died within 2 months; whereas before the ventilator was used, there died 6 or 7 in a week; so that it appeared very plain, that the ventilator caused the foul stagnated air to circulate, and thereby consequently was drawn out of the several wards.

Mens names: Mr. Sewel,* Mr. Hand,* Mr. Wilmot* (died), Mr. Letts, Mr. Chaddock,* Mr. Rust,* Mr. Morris, Mr. Bates, Mr. Thompson,* Burton, apprentice, Dobie,* ditto.

N. B. All those marked * had the jail distemper.

VII. Of the great Alterations which the Islands of Scilly have undergone since the Time of the Ancients, who mention them, as to their Number, Extent, and Position; in a Letter to the Rev. Thomas Birch, D. D. Secr. R. S. by the Rev. Mr. Wm. Borlase, M. A., F. R. S. p. 55.

The inhabitants of these isles are all new-comers; not an old habitation worth notice; nor any remains of Phenician, Grecian, or Roman art, either in town, castle, port, temple, or sepulchre. All the antiquities here to be seen are of the rudest Druid times; and, if borrowed in any measure from those eastern traders before-mentioned, were borrowed from their most ancient and simple rites.

We are not to think, however, but that Scilly was really inhabited, and as frequently resorted to anciently, as the old historians relate. All the islands (several of which are now without cattle or inhabitant) by the remains of hedges, walls, foundations of many contiguous houses, and a great number of sepulchral barrows, show that they have been fully cultivated and inhabited. That they were inhabited by Britons, is past all doubt, not only from their vicinity to England, but from the Druid monuments; several rude stone pillars; circles of stones erect; kist-vaens without number; rock-basins; tolmêns; all monuments common in Cornwall and Wales, and equal evidences of the antiquity, religion, and origin of the old inhabitants. They have also British names for their little islands, tenements, and creeks.

How came these ancient inhabitants then, it may be asked, to vanish, so as that the present have no pretensions to any affinity or connection of any kind with them, either in blood, language, or customs? How came they to disappear, and leave so few traces of trade, plenty, or arts, and no posterity, that we can learn, behind them? Two causes of this fact occurred while Mr. B. was at Scilly, which may perhaps satisfy these inquiries: the manifest incroachments of the sea, and as manifest a subsidence of some parts of the land.

The sea is the insatiable monster, which devours these little islands, gorges itself with the earth, sand, clay, and all the yielding parts, and leaves nothing, where it can reach, but the skeleton, the bared rock. The continual advances which the sea makes on the low lands, are obvious, and within the last 30 years have been very considerable. What we see happening every day may assure us of what has happened in former times; and from the banks of sand and earth giving way to the sea, and the breaches becoming still more open, and irrecoverable, it appears that repeated tempests have occasioned a gradual dissolution of the solids for many ages, and as gradual progressive ascendency of the fluids.

Again, the flats, which stretch from one island to another, are plain evidences of a former union subsisting between many now distinct islands. The flats between Trescaw, Brêhar, and Sampson, are quite dry at a spring tide, and men easily pass dry-shod from one island to another, over sand banks, (where, on the shifting of the sands, walls, and ruins are frequently discovered) on which at full sea there are 10 and 12 feet of water. History confirms their former union. "The isles Cassiterides, says Strabo, are 10 in number, close to one another; one of them is desert and unpeopled, the rest are inhabited." But see how the sea has multiplied these islands: there are now reckoned 140; into so many fragments are they divided, and yet there are but 6 inhabited.

But no circumstance can show the great alterations which have happened in the number and extent of these islands more than this, viz. that the isle of Scilly, from which the little cluster of these cyclades takes its name, is no more at present than a high rock, of about a furlong over, whose cliffs hardly any thing but birds can mount, and whose barrenness could never suffer any thing but sea-birds to inhabit it.

It has been mentioned before, that on shifting of the sands in the channel, walls and ruins are frequently seen; there are several phenomena of the same nature, and owing to the same cause, to be seen on these shores. Here then we have the foundations, which were probably 6 feet above high-water mark, now 10 feet under, which together make a difference as to the level of 16 feet. To account for this, the slow advances and depredations of the sea will by no means suffice; we must either allow, that the lands inclosed by these fences have sunk so much lower than they were before; or else we must allow, that since these lands were inclosed, the whole ocean has been raised 16 feet perpendicular; which last will appear much the harder and less tenable supposition of the two. Here then was a great sudsibence; the land between Sampson and Trescaw sunk at least 16 feet, at a moderate computation. This subsidence must have been followed by a sudden inundation, and this inundation is likely not only to have destroyed a great part of the inhabitants, but to have terrified others who survived into a total desertion of their shattered islands. By this means, as I imagine, that considerable people, who were the Aborigines, and carried on the tin trade with the Phenicians, Greeks, and Romans, were extirpated. These islands are no longer what they were anciently, fertile in tin; nor are there any remains of such and so many ancient workings as could maintain a trade, so coveted by some of the ancients, and so industriously concealed by others. There are no mines to be seen in any of these islands, but only on one load (so we call our tin veins) in Trescaw island, and the workings here are very inconsiderable, and not ancient. It must therefore be matter of wonder where the Phenicians, Greeks, and Romans, could have found such a plenty of that useful metal. Whatever resources they had from Cornwall, formerly reckoned probably among the Cassiterides, great part of their tin must doubtless have come from these islands; but where it was found is uncertain. Nothing now appears above ground which can satisfy such an inquiry. The story of the Phenician vessel mentioned by Strabo to have purposely run ashore, and risqued the men as well as lost the ship, rather than discover to the Romans the trade to these isles, is well known, and proves beyond all doubt the commerce to have been very advantageous. That the natives had mines, and worked them, appears from Diodorus Siculus, lib. 5, ch. 2, and from Strabo, Geog. lib. 3, who informs us, that Publius Crassus sailing thither, and observing how they worked their mines, which were not very deep, and that the people loved peace, and at their leisure navigation, instructed them how to carry on this trade to better advantage: that is, seeing their mines but shallow, yet well worth working deeper, taught them how to pursue the metal to a greater depth. The question then is, what is become of these mines? and how shall this question be answered, but by confessing that the land, in which these mines were, is now sunk, and buried under the sea?

Tradition seems to confirm this; there being a strong persuasion in the western parts of Cornwall, that formerly there existed a large country between the Land's-end and Scilly, now laid many fathoms under water. The particular arguments by which they support this traditon, may be seen in Mr. Carew's Survey of Cornwall, p. 3, and in the last edition of Camden, p. 11.

But though there are no evidences, to be depended on, of any ancient connection of the Land's-end and Scilly, yet that the cause of that inundation, which destroyed much of these islands, might reach also to the Cornish shores, is extremely probable; there being several evidences of a like subsidence of the land in Mount's-bay. The principal anchoring place, called a lake, is now a haven, or open harbour. The mount, from its Cornish name,* we must conclude to have stood formerly in a wood, but now at full tide is half a mile in the sea, and not a tree near it.

VIII. Of Mr. Appleby's Process to make Sea water Fresh. By W. Watson, F.R.S. p. 69.

Mr. Appleby's process is as follows: Into 20 gallons of sea water put 6 oz. of a fixed alkali, prepared with quick-lime as strong as lapis infernalis, and 6 oz. of bones calcined to whiteness, and finely powdered. With a slow fire, draw off in a common still 15 gallons.—Mr. Appleby conceives, that the alkali here employed, is the best adapted to prevent the bituminous matter in sea water from rising by the heat in distillation.

The experiments of Mr. Clark, chemical operator at Apothecaries' hall.—Into a spoonful of the distilled sea-water he put 20 drops of a solution of silver in aquafortis: he likewise did the same with the like quantity of common water distilled. There appeared no change in either, and both retained their transparency. This demonstrates, that the distilled sea-water is by the process entirely freed from marine salt, or its acid spirit. For, if we take a spoonful of common distilled water, and add the least particle of sea-salt, with the point of a penknife, and then drop into the mixture 1 or 2 drops of the solution of silver, it will appear turbid and milky.

From the number of animal bodies constantly perishing in the sea, it may reasonably be suspected, that a volatile urinous spirit may be retained in this distilled water; and this is evident from the following experiment: into a spoonful of distilled sea-water drop 10 drops of a strong solution of sugar of lead, and the mixture immediately becomes turbid and milky. Into another spoonful of common distilled water, with 2 drops of spirit of sal ammoniac, and 10 drops of a solution of sugar of lead; and this mixture had the same appearance with the foregoing.

If into a spoonful of common distilled water be dropped 1 drop of oil of tartar per deliquium, and then added 10 drops of a strong solution of corrosive sublimate, the mixture will immediately become turbid and brown, and with a few drops of the solution of silver, it will be precipitated, and turn milky. It is therefore a volatile alkali, and not a fixed one, that is contained in this water.

The solution of silver will not discover a volatile alkali contained in water, but very plainly a fixed one. A solution of sugar of lead will not discover a small quantity of marine salt or spirit, till we add more. A solution of sublimate will manifest both a volatile and fixed alkali.

* Guavas lake, signifying the grey rock in a wood.—Orig.

IX. On the Antiquities dug up from the Ancient Herculaneum, dated from Naples, Nov. 18, 1752. *By Sig. Camillo Paderni. Translated from the Italian.* p. 71.

The things, of which Sig. Paderni says he had the charge, are many, and extraordinary, consisting of metals: that is, bronzes, silver and gold of all kinds, of excellent workmanship. Beautiful cameos and intaglios. Glass of all sorts. Various productions of the earth; such as, grain, beans, figs, dates, nuts, pistachios, almonds, rice, bread. Colours for painting. Medicines in pills, and other forms, with their marks. A phial of oil. Gold lace, perfectly well preserved, and extremely curious, on account of its being made with massy gold, spun out, without any silk, or other yarn. Soap, bran, and a variety of other things, which it were tedious here to enumerate.

There were found many volumes of papyrus, but turned to a sort of charcoal, and so brittle, that, being touched, it fell to ashes. Yet by his majesty's orders, he made many trials to open them, but all to no purpose; excepting some scraps containing some words, by which it appeared in what manner the whole was written. The characters are made with a very black tincture, exceeding the darkness of charcoal.*

There were found also very lately 3 beautiful statues of marble, and one of them excellent; 6 heads of bronze, of which there was one that gave hopes of finding the statue it belongs to. It is a young Hercules, of a kind of work that has no fellow in the way of metal, having the hair finished in a surprising manner. Also several little figures of metal; a sistrum very neat and well preserved; and not a day passes but they bring some curiosities newly found.

X. A Translation and Explanation of some Articles of the Book intitled, Theorie de la Figure de la Terre. By Mons. Clairaut of the Royal Academy of Sciences at Paris, and F. R. S. p. 73.

Mr. Short, in his account of Father Frisi's Disquisito Mathematica in Causam Physicam Figuræ et Magnitudinis Telluris Nostræ, having reported that philosopher's sentiments on my reflections, says M. Clairaut, on the same matter, without taking the trouble to examine whether they were founded on the truth or not, I find myself under the necessity to lay before the Royal Society the passages of my book, which, having been misunderstood by F. Frisi, have occasioned the misconstruction made of my sentiments, either on the trust I give to the actual operation made for discovering the figure of the earth, or Sir Isaac Newton's theoretical inquiries about the same subject.

* Since this was written some successful attempts have been made to unravel these ancient manuscripts, which treat of philosophy, music, &c.

The expressions of Frisi, referred to by Mr. Short, are as follow:

"Quia tamen plerique omnes hucusque, aut nihil pro figura telluris determinanda ex iis observationibus deduci posse cum geometra celeberrimo Ruggero Boscovik autumârunt, aut exinde cum ill. Clairaut, Bouguer, aliisque, contra incomparabilem virum ac prope divinum Isaacum Newton insurgentes, admirabilem ipsius theoriam facto minus respondentem dixerunt, assignatamque in prop. 19, lib. 3, Princip. Mathem. terrestrium axium proportionem à vera absonam omnino esse, alios mihi observationibus parum, alios nimis tribuere visum est, omnes ferme oppositis erroribus peccâsse, ubi res neque aurificis lance, neque molitoris, ut aiunt, statera librandæ sunt."

This, when compared with the propositions of my theory, which they relate to, will appear, I hope, quite incoherent; and I cannot show it more clearly than by translating the last chapter of my book, to which Frisi refers the reader. For the better understanding of that chapter, it is proper to know, that the chief results of the precedent inquiries are these theorems:

1. Supposing the earth in its former state composed of several fluids of different densities, and settled all in equilibrium by the laws of gravity and centrifugal forces, the surfaces separating the different mediums will always affect the form of a curve, which is so near to the ellipsis, that it may be supposed so, without any error of the least moment. 2. That, in the case of the denser fluids being nearer to the centre, as hydrostatics require, the spheroid will always be less flat than in the homogeneous one, and vice versa. 3. And as to the diminution of the gravity from the pole to the equator, it will always follow the opposite rule, viz. if the spheroid be denser towards the centre, the gravity will decrease in a less ratio than in the homogeneous spheroid, and vice versa. 4. That if δ represent the fraction found out for the difference of diameters, $\frac{1}{115} - \delta$ will express the total diminution of the gravity from the pole to the equator, not only in the case of the spheroid supposed originally fluid, but in any supposition of variation for the densities and proportion of the diameters of the beds, provided they be elliptical.

These premised, let us proceed to the last chapter of the theory of the earth's figure; in which the principles laid down in the preceding chapters are compared with the observations.

§ 68. For the diminution of the gravity from the north to the south.

It has been seen in the preceding chapter, that when a spheroid is not supposed homogeneous, the diminution of the gravity from the pole to the equator will be greater than in the case of homogeneity. Hence, if my theory holds in our globe, the whole decreasing of the gravity will be equal to $\frac{1}{230}$ or greater, and never less; since the ratio of 230 to 231 will (§ 21) express the ratio of the action of gravity at the equator and pole, when the spheroid is homogeneous.

And this conclusion of my theory quite agrees with experience; for, from all the observations relating to the gravity made in several places of the globe, either by actual measures of the second pendulum, or by the difference of duration of the same pendulum's vibrations, it appears, that the gravity decreases from the north to the south in a greater ratio, than it would be, if the total diminution from the pole to the equator were only $\frac{1}{231}$.

§ 69. For the proportion of the two diameters.

Supposing, as in the precedent chapter, the earth originally fluid, it follows, from the § 65, that the ratio of the two diameters cannot exceed that of 230 to 231; since (§ 20) 230 to 231 is the ratio in the case of the homogeneous spheroid; and as the mensurations of the gravity cannot agree with the supposition of the homogeneity, the diameters of the earth ought to be in a ratio less than 230 to 231.

Without adhering to the supposition of the earth's being formerly fluid, but admitting, as in the chap. 3 and 4, all generality possible in the variation of density and ratio of diameters of the beds or strata laid down from the centre to the surface, there will still happen a difference of the diameters less than $\frac{1}{230}$. For, by § 50, the total diminution of the gravity from the pole to the equator being subtracted from $\frac{1}{115}$, the remainder is the difference between the diameters. Now the diminution of the gravity having been found greater than $\frac{1}{230}$, the ellipticity, or difference of diameters, ought to be less than that fraction, and consequently the ratio of diameters less than 230 to 231.

That consequence of my theory is not so happy as the preceding; for the degree measured in the north, compared to that of France, give the two diameters as 177 to 178, which ratio is greater than 230 to 231 instead of being less, as the theory would require.

As the measures made in the north have been performed with great care and exactness,* their result seems at first to be preferred to that of my theory. But a reflection on the errors unavoidable in actual measures, and an examination of the limits of these errors, will show that, without violating the measures, they would be brought nearer the theory, and even agree with it. For, by a convenient calculation, it will be found, that a diminution less than 60 toises, made to the difference between the degrees of Paris and Tornea, would give the diameters in the ratio of 230 to 231. And if it be considered what is the smallness of an error of 60 toises, when divided in two operations, which require so great a number of astronomical and geographical observations, it will be thought that an error a little larger may be supposed, without disparaging either our operation, or Mr. Picard's; and thus theory and experience would agree.

* Those measures have been since found very erroneous.

Supposing, for example, that the difference between the degrees of Paris and Tornea has been found too great by 80 toises, the difference between the two diameters will come out about $\frac{1}{260}$, which, subtracted from $\frac{1}{115}$, gives $\frac{1}{200}$ for the diminution of the gravity from the pole to the equator. And such a conclusion would agree pretty well with the observations made in France and Lapland with the excellent clock of Mr. Graham.

However, though the errors to be supposed in the operations, to reconcile them with my theory, be in themselves small enough, I shall abstain from asserting that they have been committed. It is a fact not to be decided, till after the result of the observations which are expected from Peru. For the great difference which is to be found between the degrees of Quito and Tornea, is the only means of knowing whether the diameters be less or greater than 230 to 231.

Were the question only, to demonstrate the flatness of the earth, the measures of the degree of Paris and Tornea would be full sufficient; but to discover the true ratio of diameters, is what can be performed only by the comparison between the degrees whose mutual distance is the greatest.

Such a ratio once fixed, if it happen to be less than 230 to 231, it will be very easy, by the preceding theory, to imagine some hypothesis for the inside of the earth, which shall agree with both theory and observation, whether admitting the supposition of the original fluidity of the globe, or not.

But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231, I own, that not only the theory established in this second part of my book must be abandoned, but it would be very difficult to reconcile the measures of the pendulums with those of the degrees on Sir Isaac's system. And I dare say, that the success in that case would hardly depend on any natural hypothesis.

The subsequent 70th article containing only a proof, that the preceding theory agrees with any ratio between $\frac{1}{23}$ and $\frac{1}{9}$, for the quantity which expresses the excess of Jupiter's equator above its axis, there is no necessity for the translation of the arguments leading to a result so answering to the observations; and I pass to the conclusion of that article, which ends my book.

The preceding theory agreeing with all the measures of the pendulum, and observations of Jupiter's diameters, if besides it happen, that the measures expected from Peru give, when compared with those of Lapland, a difference of diameters less than $\frac{1}{230}$, this theory will have all possible confirmation, and the universal gravitation so well agreeing with the motions of the planets, will also agree with their figures.

Now I beg every candid reader to examine, whether, in that chapter quoted by F. Frisi, I have too much relied on the certainty of observations, and attempted to disparage Sir Isaac Newton's discoveries.

In the first place, I will ask F. Frisi if before the operations, which I depended on, were performed, I could establish any thing against their agreeing, or not, with Sir Isaac's proposition about the same matter?

He perhaps will answer, that my remark of the 69th art. "But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231," imports that I was not thoroughly convinced, that what care soever would be taken by the gentlemen sent into Peru, they never would be able to measure their degree with a sufficient exactness, to conclude, from its length, compared with that of the other degrees, whether the diameters were in a greater or less ratio than 230 to 231; and consequently he will think, that my being in suspense about it was an offence against Sir Isaac's theoretical determination. Then I request F. Frisi to tell me, why he is so good as to commend operations so void of use, as those which tended only to discover what was demonstrated before, and needed not to be confirmed, since it could not be invalidated.

Perhaps F. Frisi, in representing me as depending too much on the observations, relied on these expressions of the 69th art. 'As the measures of the gravity cannot agree with the supposition of the homogeneity:' and I confess, that it seems to me impossible to reconcile the great number of all the measures of that sort with the table which follows the homogeneity. For the simplicity of the means made use of in the performance of those measures cannot admit the errors, which should be supposed to bring them to Sir Isaac's theory: but as this theory is founded on the homogeneity, which is only a mere supposition; and as he has himself suspected, in his second and third edition, that the internal parts of the earth might be denser than those towards the superficies, I do not see how I oppose myself to that illustrious philosopher, when I assume the same hypothesis as he does. As I shall use all possible endeavour to understand F. Frisi's meaning, I hazard this conjecture. Seeing that I thought favourably enough of the exactness to be obtained in astronomy, when observations have been already made in great numbers, and with all possible care, to suppose them fit to let us know, whether the diameters are in a greater or less ratio than 230 to 231; and being informed afterwards, that the operation made in Peru led those who have made use of it to imagine the spheroid flatter than the homogeneous, he concludes, that I cannot help thinking like them, and accordingly indulges himself in exposing how much I over-rate the validity of observations, and how little I know the submission due to a proposition of Sir Isaac; which, I must say by the bye, that great man has never himself given as impossible to be opposed by experience. But yet I would ask of F. Frisi, why he will guess at my sentiments, while I have not given room to know them on that point? How can he know whether, since the examination of all the measures, I have not found any way to reconcile them with the theory? Which I say in no manner as a hint I intend to make any corrections in those measures, but merely to show the little foundation which F. Frisi had to represent me as he has done.

However difficult it may be to account for F. Frisi's expressions, I shall hazard yet another conjecture. His great zeal for Sir Isaac, for which he is certainly to be commended (if not blinded by that zeal) has hindered him from distinguishing between the different ways of opposing that great man's sentiments. Perceiving then, that my calculations (§ 50, part 2) had led me to a result quite different from Sir Isaac's assertion, (Prop. 20, lib. 3), he was offended at my boldness to such a degree, that he was unable to examine impartially what I said; and instead of discussing a mathematical question quite independent of any actual measure, wherein if I were mistaken, he would have forced every geometrician to condemn me, he has supposed that I have built my argument on an operation which was not performed at the time when I wrote.

This conjecture would appear to me the true cause of F. Frisi's error, if it were not inconsistent with a proceeding of his towards Sir Isaac, which I will venture to relate. After F. Frisi has examined himself the 19th problem of the third book of the Principia, which is much less complicated than that I spoke of, the truth of which is incontestable, he finds, by his own mistake, a disagreement with the result of that proposition, and charges that illustrious author, without the least apology, with an error, which, says he, (quite from the purpose) is the 6th, that has been found in the same work, and also gives an enumeration of the 5 others, though they are not at all concerned in the question.

I cannot forbear saying, that the manner in which I have proposed my remarks on the 20th proposition of Sir Isaac, has nothing of that slight way of treating so great a man; and as my utmost wish is to be judged on that account by the Royal Society, I shall relate what were my objections; which I cannot effect in a more concise and clear method, than by giving the translation of the article which contains it.

§ 51. Of the 2d part of the theory, &c. 'In which is seen what had induced Sir Isaac Newton to think, that the planets, when denser at the centre than at the surface, ought to be flatter than in case of homogeneity.'

'Some years ago I gave, in the Philos. Trans. N° 449, the theorem of the preceding article; and on this occasion I mentioned a passage of Sir Isaac contrary to it. Not having at that time looked into the 2d edition of his Principia, I could not know what had engaged that illustrious philosopher to think so; and far from suspecting any mistake in his proposition, I was contented to think, that the difference between our conclusions arose from a different way of conceiving the inside of the earth; and I imagined, that he had happened to fall on such a disposition of parts, as would answer to his assertion. I then followed only his commentators, and especially Dr. Gregory, showing, that his explanation of Sir Isaac's conclusion was wrong, as grounded on a proposition which did not hold in the present case. For that proposition (which is, that the gravity at any point of the earth is inversedly as the distance from the centre) only holds when the earth is homogeneous; and consequently ought not to be made use of, when the density is greater towards the centre than at the superficies.'

'Since I have discovered that the theorem, the demonstration of which I had given in the Philos. Trans. for the case of beds supposed of the same ellipticity, holds in an infinity of other suppositions, I have taken greater care to discover what could have induced Sir Isaac to think that the earth is flatter, as the gravity is more decreasing from the pole towards the equator; and I believe I have found it out in the second edition of the Principia, and it is, for having built on the same argument as Dr. Gregory.'

'In p. 386, after having observed, that the experiments gave a diminution of two lines to the second pendulum from the pole to the equator, he argues thus: Since, says he, the case of homogeneity afforded only $1\frac{87}{5000}$ to 4, the difference $7\frac{1}{5}$ miles between the two diameters (which followed from the same hypothesis) is to be magnified in the ratio of $1\frac{87}{1000}$ to 2, and it will come out $31\frac{7}{12}$ miles for the real difference. For, pursues he, the retardation of the pendulum at the equator denotes the diminution of the gravity in that place; and the lighter the matter is there, the higher will it rise to be equiponderant with that of the pole.'

'Further, p. 387, examining the measures of the degrees of latitude made in France by M. Cassini, by which the earth is higher at the pole than at the equator by about 95 miles, he pretends, that accordingly the pendulum should be longer at the equator than at the pole by about half an inch. And all that shows the opinion which Sir Isaac was of, that in any case whatever, the equilibrium requires a gravity inversely proportional to the length of the columns, which proportion, as I hope to have evinced, is only demonstrated in the case of homogeneity, and is not true in general. Thus, what I argued in the Phil. Trans. against Dr. Gregory, holds also against Sir Isaac.'

By all that I have said, every body may judge, whether differing from Sir Isaac's sentiments on a point, which I had for so long a time examined, I did not express my disagreement with him in as decent a manner as any one should, when speaking of so great a man. And in case the R. S. thought some alterations were to be made in the form of my remarks, I declare that I shall execute it, as may be prescribed to me by that illustrious company. But I cannot help thinking, that unless those, who would examine my demonstrations, find some error in them, no alteration is requisite to be made in my expressions. I desire then either F. Frisi, or any geometrician who thinks the question worth his examination, to take the trouble of reviewing my calculations, and to believe me ready to acknowledge my error, when shown to me by a candid and impartial examiner.

Of a Storm of Thunder and Lightning, near Ludgvan in Cornwall. By the Rev. Mr. Wm. Borlase, M. A., F.R.S. Dated Ludgvan, Feb. 1, 1753. p. 86.

This storm was on Dec. 20 preceding. The first traces in the parish of Maddern, were an incision, or scratch, made in the turf, about 3 inches wide, and 2 deep, where the lightning coming up from the south-west, passing through the bank, and issuing out from the bank in 3 streams, which united again, and turned away to the north. About 10 paces to the north of these breaches, there are more marks of the same kind, but not in the same direction; for the lightning here came from the north-west, and, passing upwards, the furrow, which it had made, grew wider, and somewhat deeper, as it gained on the hill, especially where it met with bank or stone; and some banks were 5 feet wide, which had their tops untouched, but were pierced through as with a bullet. This second furrow was (as all the rest) not in a straight line, but in a vermicular direction, and with its turnings led to a karn, or ledge of flat rocks, striking off many splinters from it, and in some places making a perforation through it. There were made also furrows 10 inches wide, and a foot deep; besides which, were several places in the hill which had holes about a foot wide, and 6 or 8 inches deep, and several clods cut thin and clear off from the ground: which shows, that as this lightning went like darts through banks and stones, and tore up the ground in many places like a ploughshare, so in other places it spread into a horizontal thin edge, which scooped up and carried off the little unevennesses of the turfy ground. The whole workings of this lightning were in length about a furlong from west to east.

The first thunder-clap was succeeded, in less than a quarter of an hour, by another, which broke at a village, in the parish of Gullval, called Trythal, about a mile and half to the south-west of Moelfra hill, and was attended with the following melancholy accidents:

Thomas Olivey, a respectable farmer, had returned from the field, about a quarter before 12 o'clock, and had all his family round him in the kitchen, except his daughter, who was in the hall. There was a pan over the fire in the kitchen-chimney, full of boiling water. The farmer was sitting by the fire, and his wife on a bench before it; their only son, 23 years of age, was standing at the window, when it lightned much, and the first clap of thunder followed. This was so violent that the back door of the kitchen, which opened to the north, quivered. The farmer called to his son, and desired him not to stand so near the window, lest the lightning should hurt his eyes; on which the young man removed from the window, backwards, into the corner of the room, and sat down. The lightning came from the west-north-west, and falling on the stack of the kitchen-chimney, which was about 4 feet square, and as much in height, of hewed stone, carried it clear off from the house, and threw it into a pool of water 20 feet distant. In the chamber over the kitchen, directly beneath the top of the chimney, there was a little closet boarded in; all the boards were broken to pieces, the timbers of the roof shattered; as also the bedstead in that chamber; of the chamber-partition 2 planks were forced, a large cloaths-press thrown, and the south windows of the chamber-floor (excepting one casement) all broken, and blown out. From the top of the chimney, and chamber-floor, it descended into the kitchen below, where the family was: the farmer saw no lightning, nor heard any thunder, after the first clap before mentioned; but was struck senseless with the first flash, and thrown into the middle of the kitchen, and continued senseless for a quarter of an hour. As soon as he came to himself, he asked, who struck him? but had not the use of his arms; and felt an aching pain, shooting, as he described it, into his bones; and a brand-iron, which hung in the chimney, being thrown down into the pan of water, on the fire, had dashed the boiling water upon him to that degree, that his life was in extreme danger for more than a fortnight after. Mrs. Olivey was struck down before the hearth. Both her shoes, though buckled on as usual, were struck off her feet; but her feet not hurt: and being neither burnt nor senseless, was able to cry out for help, but could not move; for she had no use of her under-limbs for a day and a half.

The farmer's brother was at the end of a long table in the same room, and was only flung against the wall, about 3 feet distant, not hurt. Mrs. Olivey's sister was near the back door; a plank of this door was started, and beat in: she was struck senseless, and thrown 12 feet off against the settle, which stood against the south wall of the house.

The farmer's son had his coat and waistcoats (for he had two on) torn into shreds, so that it could hardly be distinguished where the pieces had formerly joined; his shirt had a rent 2 feet long down the back, and was scorched; his left shoe torn from his foot; and the little toe of that foot so nearly cut off, that it hung only by a bit of skin; and he was quite dead. But though reduced to this lamentable condition, as to his exterior, he was not moved from his seat, nor his face at all changed: his dog was lying at his feet, dead likewise, but never moved.

The farmer's daughter received the shock in the hall; was struck senseless, but revived soon; felt a trembling all over; her feet tickling, and partly be-numbed and stiff, as if sleeping; but perceiving in the room a cloud of smoke, and hearing her mother cry, she made haste into the kitchen, which she found full of smoke, stinking like brimstone. The lightning had left a mark quite across the clavel of the kitchen-chimney, about half an inch wide, in an undulating direction, broke through the partitions of the under floor, thrown down the shelves, carried out all the south windows, forced up the stair-case, blown out the north window, missed or spared a clock, which stood close by the window; and being somewhat spent, when it reached the hall, carried out the windows; moved not some Delft basins, which were in the south window, but forced the door of a beaufet, at the end of the hall, an inch and a half inwards; and shook the eastern wall of the house to the very foundation.

The clouds over Moelfra hill, and the village of Trythal (a space of a mile and a half) were so heavily charged with lightning, that here they broke, both the first and the second time, and the thunder-claps were within a few minutes of each other, as being produced but by two portions of one and the same congeries.

The general tendency of this lightning was as the direction of the wind at that time; that is, from the north-west to the east, but where the principal explosions were, as at the hill, and the house, many branches spread off in all directions. Nor were the shapes, in which it operated, less different than its motions. Sometimes, as it appeared to Mr. B. at Ludgvan, it was pointed as a dart; in some places edged as a scythe, now but one thin sheet or stream, then 2 or 3, and then one again. Now it fell as several separate balls of fire; but on the house as a large gush or torrent. It was all fire, yet of different powers, according to the impregnation of its several portions. Subtil and penetrating as the electrical fire, it affected, shocked, and permeated, all the human frame. Some parts of it only scorched wood, but did not melt iron, as with lightning is very common: some tore the leather and clothes; some cut and wounded, and some killed without wound or rent; and other parts of this lightning again, upon stone, wood, leather, clothes, and flesh, only rushed and forced with the power of air put into a violent agitation. All this happened in this place, and all in an instant: and though the clothes were somewhat singed, as well as torn, and the young man's skin round his waist was also scorched, yet, from the general effects of this lightning in both places, it was rather swift, and irresistibly piercing, than inflammatory. The house stands very high, without tree or hill near it.

XII. A Second Letter of the Rev. William Henry, D. D. concerning the Copper Springs in the County of Wicklow in Ireland. p. 94.

This is only a confirmation of the former account of procuring copper by means of iron bars laid in the stream of copper-water. He further remarks, that to prevent any dirt or mud from being carried out of the mines, by the streams, which are let into the pits, where the iron bars are deposited, the stream, as it issues out of the level, is collected into a large deep basin, where all the dirt subsides; and the clear water only is from the surface of the basin let out into the pits.

XIII. The Construction of the Logarithmic Lines on the Gunter's Scale. By Mr. John Robertson, F.R.S. p. 96.

The Gunter's scale (so called from its inventor Mr. Edmund Gunter, astronomy professor in Gresham-college, from March 6, 1619, till his death, Dec. 10, 1626,) is an instrument almost universally known, and amply described by many writers; therefore Mr. R. only shows on what principles the divisions of the logarithmic sines, tangents, and versed sines, are usually protracted.

The line of numbers on these scales consists of 2 equal lengths, commonly called 2 radii; the first containing the logarithms of numbers from 10 to 100; and in the second are inserted those between 100 and 1000, or such of them as can conveniently be introduced. These divisions are taken from a scale of equal parts; such, that 100 make the length of one radius; and from this scale, the divisions for the sines, tangents, and versed sines, are also taken. Now, from this construction of the line of numbers, it is plain that, as the numbers in one radius exceed those in the other, by one place in the scale of numeration; therefore the difference of their indices must also be unity; so that such numbers only, whose index differs by 1, can be estimated in a length of 2 radii: but in a length of 3 radii, numbers, whose indices differ by 2, may be read; and a difference of 3 may be reckoned in a length of 4 radii, &c. The tables of logarithmic sines, tangents, secants, and versed sines, are generally computed for a circle, whose radius is 10,000,000.

As the length of the Gunter's scale admits of no more than 2 radii, or of such numbers only whose index differs by unity; therefore, within this length, no more of the sines, tangents, or versed sines, can be introduced, than those whose index differs by unity: and as not only the greatest number among the sines and tangents, but also those more generally wanted, have the indices 9 and 8 differing by unity; therefore all the sines from 90° to 0° 34′, and all the tangents from 45° to 0° 34′, are those only which are put on these scales; the divisions answering to the lesser sines and tangents being omitted for want of room.

And this is the reason, why the sine of 90°, and the tangent of 45°, are limited by the same termination as the 2d radius on the line of numbers.

To construct the line of logarithmic sines.

From the scale of equal parts, take the numbers expressing the arithmetical complements of the log-sines of the successive degrees, and parts of degrees, intended to be put on the scale, descending orderly from 90°: then these distances successively laid from the mark representing 90° at the right-hand end of the scale, will give the several divisions of a scale of logarithmic sines. For, the ends of any scale being assigned, the progressive divisions of that scale are laid on it from that end which represents the beginning of the progression: or, the same divisions may be laid from the other end, by taking the complements of the terms to the whole length of the scale: consequently the arithmetical complements of the sines are to be laid from the division representing 90 degrees.

To construct the line of logarithmic tangents.

These are laid down in the same manner, and for the same reasons, that the sines were; the tangent of 45° standing against the sine of 90°. The divisions for the tangents above 45°, are reckoned on the same line from 45° towards the left hand; or any tangent and its co-tangent are expressed by the same division. Thus one mark serves for 40° and 50°; and the division at 30° serves also for 60°; that at 20° serves for 70°, &c. and the like is to be understood of the intermediate divisions. For, as the tangent of an arc, is to radius; so is radius, to the co-tangent of that arc. Therefore the tangent is equal to the square of radius divided by the co-tangent. And the co-tangent is equal to the square of radius divided by the tangent.

Now the radius being unity, its square is also unity. Therefore the tangent and co-tangent of any arc are the reciprocals one of the other. But the reciprocals of numbers are correlatives to the arithmetical complements of their logarithms. Therefore the logarithms of a tangent and its co-tangent, are arithmetical complements one of the other; and consequently will fall at equal distances from 45 degrees. Therefore, in the line of logarithmic tangents, the divisions to degrees under 45, serve also for those above; both being equally distant from 45 degrees.

To construct the line of logarithmic versed sines.

As the greatest number of degrees will fall within the limits of the scale, by beginning at 180°; therefore the termination of this line is at 180°, which is put against 90° on the sines: and though the numbers annexed to the divisions increase in the order from right to left, yet they are only the supplements of the versed sines themselves. Now subtract the logarithmic versed sines, of such degrees and parts of degrees as are intended to be put on the scale, from the logarithm versed sine of 180°; then the remainder taken from the foresaid scale of equal parts, and laid successively from the termination of this line, will give the several divisions sought.

Hence it appears, that the least versed sine, which can be introduced within the length of a double radius, falls between 10° and 20°, where the index changes from 1 to 2; which will happen about 11° 28′.

If a table of logarithm versed sines to 180° are wanting, they are easily made by the following rule: Take the logarithm sine of 30° from twice the logarithm sine of (N) any number of degrees; the remainder is the logarithm versed sine of (2N, or) twice those degrees." For it is a well-known goniometrical property, that the sine of any arc (A), is a mean proportional between radius (R) and half the versed sine of twice that arc.

Therefore, putting v for the versed sine, and s for the sine;

then $v\,2\text{A} = (\frac{2ss\text{A}}{\text{R}} = ss\text{A} \times -\frac{2}{\text{R}} = ss\text{A} \times \frac{2}{10} =)\; ss\text{A} \times \frac{1}{5}$; radius being 10.

Or the log. $v\,2\text{A} = 2$ log. $s\text{A}$ — log. 5.

But when radius is 10, the sine of 30° is 5.

Therefore the log. $v2\text{A} = 2$ log. $s\text{A}$ — log. sine of 30°.

Most of the writers on this subject give the following rule for laying down the divisions of this line: From the line of logarithmic sines, take the distance between 90° and any arc; that distance being twice repeated, from the termination of the line of versed sines, will give the division for twice the complement of that arc." Thus the distance between 90° and 20° on the sines twice repeated, gives the versed sine of 140°; or twice 70°, the complement of 20°. For the divisions, to be laid on this line, are the differences between the logarithm versed sine of 180°, and the logarithm versed sines of the successive arcs.

Now the difference between the logarithm versed sines of 180°, and of any arc 2A, is log. ver. sine 180 — 2 log. sin. A + log. sin. of 30°.

Or, 10,30103 + 9,69897 — twice log. sin. of A.

Or, 20,00000 — twice logarithm sine of A.

Or the arithmetical complement of twice logarithm sine of A. That is, the difference between the logarithm versed sine of 180°, and the logarithm versed sine of any arc, is equal to double the arithmetical complement of the logarithm sine of half that arc, rejecting the indices.

But, as these differences give the divisions to the supplements of the real versed sines; therefore the arithmetical complement of the logarithm sine of any arc being doubled, will give the distance of the division for the supplement of twice that arc on the line of versed sines.

Thus, for 70°, the logarithm sine is 9,97299

The arithmetical complement is 0,02701
Its double is 0,05402
Which is the supplement versed sine of twice 70 degrees.

Now, as the arithmetical complement of the log. sines of arcs, are the distances on the line of sines between 90°, and the divisions to those arcs; therefore the distances between 90° and any arc, being twice repeated, will give the division of the supplemental versed sine to twice the co-sine of that arc.

XIV. Concerning an Improvement of refracting Telescopes. By Mr. John Dollond. Dated Feb. 21, 1753. p. 103.*

It is well known, that the perfection of refracting telescopes is very much limited by the aberration of the rays of light from the geometrical focus; which

* Mr. John Dollond, F. R. S. was born 1706, in Spitalfields, London, where his father had settled as a silkweaver, having fled from France on account of the persecutions after the revocation of the edict of Nantz. Here our author was bred to his father's profession, which he continued during a great part of his life. But being of a very studious and philosophical turn of mind, his leisure hours, even while a youth, were chiefly employed in mathematical pursuits. And though by the death of his father, which happened in the infancy of our author, his education gave way to the necessities of the family, yet at the age of 15 he amused himself by constructing sundials, drawing geometrical schemes, and solving problems. Add to this, that an early marriage, and an increasing family, left him but little opportunity of pursuing his favourite studies. Yet even under the pressure of a close application to business, for the support of his family, the energy and perseverance of his genius urged him, by abridging the hours of rest, to extend his mathematical knowledge, making a considerable proficiency in optics and astronomy, to which he now chiefly devoted his attention; having in the earlier stages of life prepared himself for the higher parts of those branches by a competent knowledge of algebra and geometry. To these he added the study of anatomy, particularly that of the eye; and even made a considerable proficiency in the Latin and Greek languages.

His eldest son Peter he brought up to the same profession with himself; and for several years they carried on their manufacture together in Spitalfields; but the employment neither suited the expectations nor disposition of the son, who having received much information on mathematical and philosophical subjects from the instructions of his father, and observing the great respect which professional men had for his father's knowledge in optics, he determined to apply that knowledge to the benefit of himself and the family; and accordingly, under the directions of his father, he commenced optician. Success attended these efforts; so that in the year 1752, our author also, embracing the opportunity of pursuing a profession congenial with his mind, joined his son, and in consequence of his theoretical knowledge, soon became a proficient in the practical parts of optics. His first attention was directed to improve the combination of the eye-glasses of refracting telescopes; hence he proceeded till he produced telescopes furnished with 5 eye-glasses, which greatly excelled all former ones, and of which he gave an account to the R. S. in the paper above printed. He soon after made a very useful improvement in Savery's micrometers; for instead of employing two entire object-glasses, as Savery and Bouguer had done, he used only one glass cut into 2 equal parts, one of them sliding laterally by the other; by which contrivance Mr. Short was enabled to apply it to the reflecting telescope with much advantage; an account of which was communicated to the R. S. by our author, and printed in the same 48th vol. of the Phil. Trans. p. 178. Thus Mr. Dollond's celebrity in optics soon became universal; and in consequence the friendship and protection of the most eminent men arises from 2 very different causes; that is, from different degrees of refrangibility of light, and from the figure of the sphere, which is not of a proper curvature for collecting the rays in a single point. The object-glass is chiefly affected by the first of these; nor has there been yet any method discovered for rectifying that aberration so, as in the least to remove the indistinctness of the image arising from it. We are therefore reduced to the necessity of contracting their apertures, which renders it impossible to magnify much, without very long glasses.

of science encouraged his pursuits. Under such favourable circumstances Mr. D. engaged in the discussion of a subject which then interested the public philosophical mind of all Europe. Sir Isaac Newton had declared, in his treatise on Optics, p. 112, "That all refracting substances diverged the prismatic colours in a constant proportion to their mean refraction;" and drew this conclusion, "that refraction could not be produced without colour;" and consequently "*that no improvement could be expected in the refracting telescope.*" No one doubted the accuracy with which Sir Isaac had made the experiments; yet M. Euler and some others were of opinion that his conclusion went too far, and they maintained that in very small angles refraction might be obtained without colour. Mr. D. however was not of their opinion, but defended Newton's doctrine with much ingenuity and learning; as appears by the letters that passed between Euler and Dollond on that occasion, and published in this same 48th vol. of the Philos. Trans. p. 287; where Mr. D. contended that, "if the result of the experiment was as described by Newton, there could not be refraction without colour."

Mr. D.'s active and accurate mind however could not rest satisfied barely with an experiment made by another, which he could perform himself. Accordingly he began the examination himself in the year 1757, which he assiduously prosecuted till June 1758, when he found the result to be very different from what he expected, and from what Sir Isaac had related. He discovered "*the difference in the dispersion of the colours of light, when the mean rays are equally refracted by different mediums.*" The discovery was complete, and he immediately drew from it this practical conclusion, "That the object-glasses of refracting telescopes were capable of being made without being affected by the different refrangibility of the rays of light." His account of the experiments was printed in the Philos. Trans. vol. 50, p. 743, and the same year he was presented by the R. S. with the annual gold medal as a reward of his discoveries, though he had not yet become a member of the Society. This discovery however no way affected the points in dispute between Euler and Dollond respecting the doctrine advanced by Newton. A new principle was thus discovered, which had no part in the former reasonings, and it was reserved for the accuracy of Mr. D. to have the honour of making a discovery which had eluded the observation of the immortal Newton. This new principle being now established, Mr. D. was soon able to construct object-glasses correcting the different refrangibility of the rays of light, and the name achromatic was given to them by Dr. Bevis, though it seems that M. Lalande had said he conferred that name. Mr. D.'s improvement in refracting telescopes has been of great advantage in astronomy, having been applied to fixed instruments; by which the motions of the heavenly bodies are determined to a much greater exactness than by means of the old telescopes. Navigation has also been much benefited by applying achromatic telescopes to the Hadley's sextant.

In the beginning of the year 1761 Mr. D. was elected F. R. S., and appointed optician to the king. But he did not long enjoy these honours: for, on the 30th of November, the same year, a fit of apoplexy in a few hours terminated his life, at 55 years of age. Besides Mr. Peter Dollond, above mentioned, our author's family, at his death, consisted of 3 daughters, and another son, John, who with his elder brother Peter, carried on the optician's business with the greatest reputation to the time of the death of the younger brother John, which happened in the year 1805. And the business is still carried on by the elder brother Peter, in conjunction with his nephew Mr. Huggins ——— Dollond.

A larger account of the Dollonds may be seen in the Philos. Magazine, vol. xviii. p. 47, from which the above has been abstracted.

But the case is widely different with regard to the eye-glasses; for though they are very much affected by both the aberrations before mentioned, yet by a proper combination of several together, their errors may be in a great measure corrected. If any one, for instance, would have the visual angle of a telescope to contain 20 degrees, the extreme pencils of the field must be bent or refracted in an angle of 10 degrees; which, if it be performed by one eye-glass, will cause an aberration from the figure, in proportion to the cube of that angle: but if 2 glasses be so proportioned and situated, as that the refraction may be equally divided between them, they will each of them produce a refraction equal to half the required angle; and therefore, the aberration being in proportion to the cube of half the angle taken twice over, will be but a 4th part of that which is in proportion to the cube of the whole angle; because twice the cube of one is but $\frac{1}{4}$ of the cube of 2; so the aberration from the figure, where 2 eye-glasses are rightly proportioned, is but a 4th of what must unavoidably be, where the whole is performed by a single eye-glass. By the same way of reasoning, when the refraction is divided among 3 glasses, the aberration will be found to be but the 9th part of what would be produced from a single glass; because 3 times the cube of one is but one 9th of the cube of 3. Whence it appears, that by increasing the number of eye-glasses, the indistinctness, which is observed near the borders of the field of a telescope, may be very much diminished, though not entirely taken away.

The method of correcting the errors arising from the different refrangibility of light, is of a different consideration from the former; for whereas the errors from the figure can only be diminished in a certain proportion to the number of glasses, in this they may be entirely corrected, by the addition of only one glass; as we find in the astronomical telescope, that 2 eye-glasses, rightly proportioned, will cause the edges of objects to appear free from colours quite to the borders of the field. Also in the day-telescope, where no more than 2 eye-glasses are absolutely necessary for erecting the object, we find, by the addition of a 3d rightly situated, that the colours, which would otherwise confuse the image, are entirely removed: but this is to be understood with some limitation; for though the different colours, which the extreme pencils must necessarily be divided into by the edges of the eye-glasses, may in this manner be brought to the eye in a direction parallel to each other, so as, by the humours thereof, to be converged to a point in the retina; yet, if the glasses exceed a certain length, the colours may be spread too wide to be capable of being admitted through the pupil or aperture of the eye; which is the reason, that in long telescopes, constructed in the common manner, with 3 eye-glasses, the field is always very much contracted.

These considerations first set Mr. D. on contriving, how to enlarge the field by increasing the number of eye-glasses, without any hindrance to the distinctness or brightness of the image: And though others had been about the same work before, yet observing that the five-glass telescopes, sold in the shops, would admit of further improvement, he endeavoured to construct one with the same number of glasses in a better manner; which so far answered expectations, as to be allowed by such persons as are the best judges, to be a considerable improvement on the former.

Encouraged by this success, he resolved to try if possible he might gain some further enlargement of the field by the addition of another glass: and by placing and proportioning the glasses in such a manner, as to correct the aberrations as much as possible, without any detriment to the distinctness, he obtained as large a field, as is convenient or necessary, and that even in the longest telescopes that can be made.

These telescopes with 6 glasses having been well received, and some of them being gone to foreign parts, it seems a proper time to settle the account of its origin; which is one of the motives, that has induced him to give this short sketch of the considerations, that gradually led him to its construction. And as the subject has never been fully treated by any author, he intends as soon as may be, to draw up a more particular explanation of the aberrations of light by refraction.

XV. A Comparison of Different Thermometrical Observations in Siberia. By Mr. Wm. Watson, F. R. S. p. 108.

According to the thermometrical observations made by Mons. Demidoff, at Soliskamsky, on the borders of Siberia, latit. 59, in the year 1751, the greatest degree of cold was on Nov. 9, at 7 in the morning, when the thermometer, according to Fahrenheit's scale, stood at 34 degrees below 0; which is 66 under the freezing point. This degree of cold, though much greater than what is ever observed in these parts, is little, when compared with the accounts given by Professor Gmelin, in the introduction to the Flora Siberica. This gentleman, who was professor of chemistry and natural history at Petersburg, was sent with several other learned men, to inquire into the natural history of Siberia, and was attended by some students, a painter or two, a miner, and other proper attendants. He continued 9 whole years on this expedition, and the observations he made, extraordinary as some of them are with regard to their truth, are scarcely to be doubted. The mercury in his thermometer, graduated according to De L'Isle's scale, often sunk in winter, in very southern parts of this country, as near Selinga, in lat. 48, to near 226, which is equal to $55\frac{1}{2}$ below 0 in Fahrenheit's thermometer, and is $87\frac{1}{2}$ below his freezing point.

But the cold is often more intense than this, as appears by the experiments made at Kirenginshi, where its sharpness was so great that Professor Gmelin with difficulty staid at the door of his house between 3 and 4 minutes.

Feb. 10, 1738, at 8 in the morning the mercury stood at 240 degrees in De L'Isle; which is 72 below 0 in Fahrenheit. At the same place in 1736, Dec. 11, at 3 p. m. 254 in De L'Isle, almost 90 below 0 in Fahrenheit, Dec. 20, at 4 o'clock, p. m. 263 in De L'Isle = 99 $\frac{44}{100}$ below 0 n Fahrenheit.

Jan. 9, 1735, 12 at noon, 275 = 113 $\frac{65}{100}$.

Jan. 6, 6 in the morning, 280 = 120 below 0 in Fahrenheit, and 152 below his freezing point.

Such an excess of cold could scarcely have been supposed to exist, had not these experiments demonstrated the reality of it; and Professor Gmelin assures us, they were made with all possible exactness, and agree with many others made in different parts of Siberia by his direction.

It was not apprehended that a greater degree of cold existed any where than that artificial one produced by Boerhaave, by means of ice and concentrated spirit of nitre, which sunk the mercury 40 degrees below 0 i Fahrenheit's thermometer; and this was supposed to be the point, beyond which no animal could bear it: and Gmelin's account is the more extraordinary, as the French academicians under the polar circle mention the greatest degree of cold, observed by them, to be by Reaumur's thermometer 37 degrees, which nearly corresponds with 70 degrees below the 0 in Fahrenheit's.

XVII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the Company of Apothecaries for the Year 1752, *pursuant to the Direction of Sir Hans Sloane, Baronet.* p. 110.

[This is the 31st presentation of this kind, completing to the number of 1550 different plants.]

*XVIII. Observations on a Remarkable Coralline.** *By Mr. John Ellis.* p. 115.

This coralline he received from Mr. Collinson. It appears, from its size and firmness, to belong to a warmer climate than this, and is probably American. Some of the same genus, but of a different species, are found in our own coasts; but they are smaller, tenderer, and more transparent. There is one particularly, called by Dr. Dillenius, in the 3d edit. of Ray's Synopsis, p. 37, N°. 20, tab. 2, fig. 1, corallina pumila erecta ramosior: and in Buddle's Hortus siccus, in the late Sir Hans Sloane's collection, there is a specimen like it, but not so

* Cellaria neritina. Ellis. Sertularia neritina, Linn.

fully advanced in its ramifications: this he calls fucus minimus hirsutus fibrillis herbaceis similis, from Doody's Appendix to Ray's Synopsis, p. 330.

This curious sea production, which has the appearance of a plant, arises first from many small vermicular wrinkled tubes, by which it appears to have adhered, like ours, to rocks, shells, fucuses, or other submarine substances. These tubes, uniting, form a sort of stem which, as they rise, insensibly change into rows of cells: these stretch out into many regular dichotomous branches: each branch is made up of 2 rows of cells united together, and these cells placed in such a manner side by side, that each cell joins 2 others on one side, and the bottom of one is inserted in the top of the other. Their openings or faces look one way: they are nearly of an egg-shape, a little compressed before: the broadest part is uppermost, and bends a little forward: the top of each is fortified by 2 angular points or spines.

By attentively viewing many specimens of this genus of corallines, in the microscope, that was taken out of the sea at different seasons of the year, Mr. E. observed the progress of nature to be pretty nearly thus: the tubuli, or first beginning of the corallines in the younger state, are found full of a yellow soft substance, which soon decays; in the more perfect state they are clear and transparent. The cells, which communicate with these tubes, have in the spring black specks in each, which he takes to be the embryo of the future production. During this very tender and minute state, the opening of each cell is covered with an extremely fine transparent membrane, the use of which no doubt is to cherish and protect it. These specks in time swelling into spherical testaceous bodies, as they are often found in summer, burst through this membrane, and sit in the front of the cell, supported by an umbilical ligament, which is fastened to the bottom of the inside of each cell or matrix, till they come to maturity, which seems to be the case in the microscopical drawing, fig. A, pl. 8: where they appear to be rows of very small sea-snails, or rather testaceous bodies, of the shape of a nautilus, ready to drop off, and provide for themselves. In the same plate, fig. B, &c. is a microscopical drawing of one of the English corallines of the same genus, with the embryo specks in each cell.

He further adds, that he believes, if the curious, with good microscopes, at the sea-side, and at different seasons of the year, would strictly examine many of these beautiful sea productions, hitherto claimed by the botanists, they would find that several of the testaceous tribe proceed from some kinds of the larger corals, as well as that many owe their origin to the smaller corallines; and we are the more encouraged to try, since we observe that various shapes and stages of the same animal are no new thing in the laws of nature.

XIX. Of some Uncommon Fossil Bodies. By Mr. Henry Baker, F. R. S. p. 117.

The fossil bodies which Mr. B. sent to the R.S. with this paper, were such as he had never before met with, nor remembered any description of. He received them from W. Frankcombe, a young gentleman residing at Oxford, who was very diligent in searching after curiosities of this nature. He found them himself, but could not get them out of the bed they lay in without breaking them in many pieces: though he has glued those pieces so well together, that one may judge of them nearly as well as if they had not been broken.

Mr. B. caused drawings of them to be made, for the satisfaction of those who might never have an opportunity of seeing them; to which drawings he refers in his description of them.

Pl. 8, figs. 3, 4, 5, 6, show these curious fossil bodies at more than a 4th their real size. They are only 3 in number, though there are 4 figures, one of them being drawn in 2 positions. They are evidently of a bony substance, made black, most likely, and rendered brittle, by some mineral steams or juices, though not corroded by them. Two of these bodies (fig. 3, 4) have the greatest part of their outer surface studded, as it were, with pretty regular rows of tubercles, about the size of the heads of small nails, rising to a blunt roundish point, nearly $\frac{1}{12}$ of an inch above the surface they issue from. Many of them appear radiated very prettily from the base to the apex; and perhaps they have all been so, though in some the lines are not now seen, and may have been obliterated by time. These tubercles are of a fine shining glossy black colour, and of a much closer and harder substance than the bone from which they rise.

Fig. 3 represents one of these fossil bodies, whose length from end to end is $7\frac{1}{4}$ inches; on the sides from a to b its breadth is 2 inches. The width of that part where the teeth are placed at c about $\frac{7}{8}$ of an inch; but it gradually decreases, as does also the breadth of the sides, towards the smaller end, which was probably about an inch longer than it now appears, and terminated in a point. The tubercles are largest in the broadest part, and the farther they are from the teeth, near which they are small and flat; they likewise lessen towards the smaller end, which is rigid for about an inch, and without any tubercles.

The under part of this body is placed uppermost, for the sake of showing its teeth to the best advantage. There are 2 rows, running longitudinally, on a little rising in the middle, with no great regularity, and ending in one row of very small ones. The largest are about $\frac{1}{4}$ of an inch in length, hooked, of a shining black colour, having still the natural polish, and being extremely sharp and perfect. The sides of this fossil have swelled out, and been naturally more rounded than they are at present: for they plainly appear to have been crushed and compressed together by some foreign force.

Fig. 4 is a fossil body, 10 inches in length, one part of which is rounded, and the opposite part hollowed: this figure shows the hollow part, which from a to b is more than one inch and half over; the channel runs its whole length, and where deepest is $1\frac{1}{2}$ inch over, but it gradually becomes shallower and narrower towards the smaller end. The sides are $\frac{1}{4}$ of an inch in thickness.

Fig. 5 shows the same fossil body with the rounded part upwards. Its sides from a to b are 2 inches. Great numbers of black shining tubercles, of the kind described fig. 3, but in general larger, and with less variation in their size as to one another, are disposed in rows, pretty regularly in the manner shown in the picture. Many of them appear starry or radiated with several fine lines from the base to the apex, which lines rise a little, and in some positions to the light appear of a whitish colour. Two separate figures of these tubercles are given (p, q) to make this account the better understood. One is a side and the other a front view. They are shown magnified about 8 times.

Fig. 6 is a fossil body, much more solid and weighty than the former two. Its length is 10 inches. It is rounded on the upper part, where the sides in the broadest place are $1\frac{3}{4}$ inch: the under part has a hollow or channel $1\frac{1}{8}$ inch in depth, $7\frac{1}{2}$ inch long, $1\frac{1}{2}$ inch over, its bottom rounded. From a 3 inches and a half to b is quite solid, and at a in width $1\frac{1}{4}$ inch, whence it goes tapering to b, where it is broken off so blunt, as to show that it must probably have extended 4 or 5 inches farther. In this solid part c stand many small teeth in rows, but not quite regular; some rows having but 2, some 3, and others 4. They begin an inch distant from the channel, and went probably to the extremity that is broken off. They are black and shining like those in fig 3, but the points somewhat broken; though when whole they must have been less hooked, and much smaller than those. The rounded part of this fossil body has no tubercles like the other two, though it is plainly a species of the same kind with them, but is pretty strongly furrowed, and the ridges have the same black glossy polish as their tubercles.

Mr. Francombe writes, "that he met with these 2 bodies, fig. 1, 2, in a pit, on the right hand side of the road, as you ascend Shotover-hill from Oxford. The uppermost stratum in this pit consists of a yellow sandy earth; the next a brownish clay; then a regular stratum of large stony nodules, about 12 inches thick; then a dark blue clay, of about 10 feet; and immediately under, a rock of free-stone. About 2 feet above the free-stone were found the fossils, fig. 1, 2. The first was found at twice; the second in searching to complete the first, and both of them in many small pieces, as is evident from the bodies themselves, which he carefully joined with some thick gum-water. That the first is of its proper shape and figure plainly appears from the regularity of its tubercles; and the second is as he saw it himself in the stratum. In this clay are found bones of several kinds, oyster-shells, ammonitæ, crustaceous shells, selenitæ, and belemnitæ.

"The cliffs on the right hand side of Pyrton-passage over the Severn, Gloucestershire, afforded the body, fig. 6. This, says he, which was likewise found in a stratum of blue clay, not unlike that at Shotover, and also in several pieces, appeared different from the others in nothing, but in the want of tubercles, and I flatter myself will serve to throw no small light on the subject. His being not quite so conversant with these cliffs as with the pit at Shotover, prevents his speaking of them so particularly as he could wish."

The general appearance of these fossil bodies gives reason to conjecture, that they are bones belonging to the head or snout of some animal of the fish-kind, or perhaps of some sort of lizard, alligator, or crocodile.

The piece, fig. 3, whose sides are a little crushed, was found in Oxfordshire, with the piece fig. 4 and 5, and may probably have been part of the same head: and if so, it should seem from the 2 rows of teeth along its middle to have been the upper part of the head or snout: for some kinds of fishes have teeth in the palate or upper part of the mouth, but we know of none that have teeth along the middle of the lower part: there a tongue most commonly is placed, and the piece, fig. 4, has a hollow or channel well adapted to contain a tongue. The teeth in the palate of the lupus piscis, and likewise of some other fishes, are frequently found fossil, of various sizes and shapes, being what are called (very improperly) bufonitæ. When the 2 pieces 3, 4, are brought together, their size, figure, and appearance, greatly strengthen the above conjecture: and it is worthy observing, that the teeth are hooked inward, to prevent the prey when taken from escaping.

The piece, fig. 6, found in Gloucestershire, serves likewise to confirm the same opinion: for the toothing in the middle almost proves that part to have been the palate of some animal; an animal of the same genus too with fig. 3 and 4; though its having no tubercles, and being more solid, show it to have been of some different species.

Mr. B. remembered not any fossil bodies like these, mentioned by authors, nor could he point out any animal to which they might with certainty be imputed. Animal substances, before unknown, are met with frequently in the bowels of the earth; for the inhabitants of seas and rivers have been hitherto so imperfectly described, that we know but little of their internal structure; and many sorts we have never seen or heard of.

XX. An Abstract of a Discourse entitled, The History of the Emperor Tetricus, explained and illustrated by Medals; written in French by Mr. Claude Gros de

Boze, keeper of the medals in the French King's Cabinet, &c. By John Ward, LL. D., V. P. R. S. p. 124.

As the emperor Tetricus governed some years in Gaul, his reign makes part of the history of that country. But the accounts given of him by ancient writers being very confused and imperfect, this learned antiquary has attempted to clear them up from medals. And as to those of Tetricus himself, the gold ones, as he observes, are in general exceedingly scarce; and no medallion of this emperor in any metal was known, till very lately, when he procured one in gold, for the French king's cabinet, which is represented, fig. 2, pl. 8. But though M. de Boze professes only to give the history of the Emperor Tetricus; yet such was the unsettled state of the Roman affairs at that time, he thought it necessary for him to introduce it, by reciting a variety of incidents relating to other persons, which prepared the way for his advancement to that dignity. It would be unprofitable however to reprint here details of ancient writers. Suffice it therefore to observe, that after relating the historical events preceding the elevation of Tetricus, it is shown that he was a governor of Gaul as a Roman province, and that on occasion of the death of Marius, the emperor of the western part of the Roman empire, he was elected his successor, according to M. de Boze, about the beginning of the year 268 of Christ.

The Ædui, who applied to Claudius for his assistance, opposed at that time the government of Tetricus; whose medals give us a more noble idea of him, from the use he made of his victories. For in some of these he is represented not as a warrior, but in a state of peace and plenty, with the legend SALVS AVGVSTORVM, intimating that moderation in success is the true grandeur and safety of princes. And in others are seen the figures of several temples erected by him, some of them in a circular form like the Pantheon, with the legend PACI. And to these happy times Mr. de Boze refers the curious gold medallion mentioned above, which represents Tetricus as crowned with laurel, and dressed in the toga palmata, or consular robe, which was also worn in triumphs. In his right hand he holds an olive branch, and a scepter, with the Roman eagle on the top, in his left, and round his image is this inscription, IMPERATOR TETRICVS AVGVSTVS. But the reverse, if it has one, must remain unknown; since the medal is so fixed in the gold box, which contains it, that they cannot be separated without endangering both. And the radiated circle of gold, with which it is encompassed, is designed only to adorn and enlarge it.

After some notice of the struggles of the contending parties in the Roman empire, M. de Boze adds, that in the mean while Tetricus, who remained unmolested, was constantly employed in studying the welfare and prosperity of the Gauls. And as both the situation, and natural fertility, of the country are very well suited to promote commerce; those advantages were greatly improved by him, as well by repairing the roads as making new ones, the care of which works was committed to his son; some proofs of which yet appear from inscriptions on the milliary pillars, erected to mark out the distance of the ways. The legends also on some of their coins, struck in honor of Tetricus, plainly express the happiness which the Gauls enjoyed under his auspicious government; such as VBERTAS, LÆTITIA, FELICITAS PVBLICA, and the like.

However, the arts of intriguing and caballing, which had been carried to the greatest height in Gaul by Victorina, gave Tetricus a continual uneasiness, either to detect or suppress them. And therefore on the return of Aurelian from the conquest of Zenobia, whom with her two sons he sent to Rome in great pomp; when Tetricus could no longer bear with the insolence of his own soldiers, he wrote a letter to him, in which he used this expression: Eripe me his, invicte, malis. And afterwards on the arrival of Aurelian near Chalons in Campania, drawing out his forces, as if he designed an engagement, he surrendered to him both himself and his whole army. By this means Aurelian being then, as the historian expresses it, princeps totius orbis, celebrated a most splendid triumph at Rome; in which not only Zenobia with her two sons, but likewise Tetricus and his son, were exposed to public view among the other captives, to denote the subjection both of the eastern and western empire.

But Trebellius Pollio informs us, that he afterwards treated Tetricus with the highest honour, often calling him colleague, sometimes fellow soldier, and at other times giving him the title of emperor. His estate also was restored to him, and his house, which had been demolished, was rebuilt on mount Cœlius, changed into a palace, and dedicated with solemnities like a temple. Aurelian was himself invited to this ceremony, and having entered the grand hall, was surprised to see himself represented there, as delivering to Tetricus and his son the senator's robe with other marks of dignity, and receiving from them a civic crown and scepter. And afterwards, Aurelian thinking himself in a condition to avenge the outrages committed by the Persians under Sapor, on the Roman empire, he entered on that expedition; leaving the government of the greatest part of Italy to the care of Tetricus, with this complaisant expression; Sublimius habendum regere aliquam Italiæ partem, quam trans Alpes regnare.

No historian has settled the time when Tetricus died. But M. de Boze, after relating several circumstances in favour of his opinion, places it about the end of the year 275 of the present era.

XXI. An Account of a Treatise intitled, Flora Sibirica, sive Historia Plantarum Siberiæ, tomus secundus. Extracted and Translated from the Latin of Professor Gmelin, by W. Watson, F. R. S. p. 141.

This 2d vol. of the Flora Sibirica, contains 240 pages 4to, exclusive of the preface, and 98 copper plates, very curiously engraved. It was printed at Petersburg in the year 1749.

An account of the first volume of this valuable work was communicated to the Royal Society, by Dr. John Fothergill,* and has been published in their Transactions. From its title, we were only promised an account of the plants of Siberia; but Dr. John George Gmelin, its author, at that time professor of chemistry and natural history at Petersburg, and now at Tubingen, has gone much further, and has given us a great number of new and useful observations concerning the natural history of that vast region. The abundance of matter, and the limits of an extract, obliged Dr. Fothergill to confine himself, principally to the geographical and meteorological part of the work; but as the contents of this 2d vol. are chiefly botanical, Mr. W. takes a review of the 1st vol. to introduce with propriety an account of the contents of the 2d.

The Flora Sibirica contains the plants, growing spontaneously in a region of vast extent, bounded by the Uralensian mountains on the west, the ocean of Kamtschatka on the east, the Mare Glaciale on the north, the countries of Kalmucks and Mongales, and the confines of China, on the south. Our author has, among the productions of these countries, interspersed a few plants, collected by the botanist Gerber, near the rivers Don and Wolga, and in the Ukraine; partly because many of the same kind grow in Siberia, and partly from a desire that these curious plants should no longer be concealed from the public. He has given no plant a place which he himself has not examined, at least in a dried state, and of which he was not satisfied respecting its generical character.

The plants of Kamtschatka were collected by two of their company, detached for that purpose, who sent to our author from time to time large collections and descriptions of such natural bodies as occurred to them. In digesting the plants into classes, the author has followed the method of Van Royen of Leyden, who considers, that all plants may be ranged into 20 classes; and in consequence of this system, he has given 5 classes in his 1st vol. viz. those which Van Royen intitles, palmæ, lilia, gramina, amentaceæ, and umbelliferæ; and 3 classes in the 2d vol. viz. compositæ, aggregatæ, and tricoccæ; the remaining 12 classes therefore are probably to be published hereafter. The author has generally adopted the genera of Linnæus; some indeed he has taken from Haller; but wherever he thought it expedient to differ from these great men, he gives his reason; and when he finds a plant, which cannot properly be ranged under any genus already established, he forms a new one; in the explanation of which, after the manner of Linnæus, he omits nothing essential to it.

To the different species, discovered in this expedition, P. Gmelin has affixed names, after the manner of Linnæus, Haller, Van Royen, and the more modern

* See Phil. Trans. vol. xlv, p. 248; vol. ix, p. 491, of these Abridgments.

botanists, which are such, as that from the name of the species the plant may be known. But in what relates to the plants before discovered, he adopts the names given them by the botanists just now mentioned, and scarcely ever forms a new one; as he thinks a name already received, though but an indifferent one, should be retained in preference perhaps to a better; lest the number of synonyms, already too great, should be augmented. To these he usually adds the synonyms of the Bauhins and Tournefort; and sometimes, for the sake of their figures, those of Morrison, Dodonæus, Plukenet, and Lœselius; and also those of the Russian botanists, Messerschmid, Bauxbaum, and Amman. He has also throughout the work carefully separated the varieties of plants from their genuine species, and has laid down the places of their growth, the names given them by the inhabitants, and their application of them to the various purposes of life. The figures of the plants were taken from the life, and are, as far as possible, represented in their natural proportion; but from these must be excepted those of Gerber, collected near the Don and the Wolga, and some others collected by Dr. Lerche, physician to the Russian embassy in Persia, near Astracan, and even in Persia; these were delineated from dried specimens: and wherever the figure does not, to our author's satisfaction, represent the plant intended, by the neglect of the painter or engraver, he apprises you of it, and endeavours to remedy this defect in his descriptions.

The venereal disease has made no inconsiderable progress among barbarous, as well as among the more polite and civilized nations; and our author has given two methods of treating that distemper among the inhabitants of Siberia; from which, in some degree, an idea may be formed of the state of medicine in those parts of the world. One method is, a decoction of a species of cirsium,* which grows in those parts, and is first described by the author; in this decoction, when the pains are violent, they add some leaves of a species of chamærhododendron, which produces effects similar to opium, by relieving the pain, and sometimes bringing on a delirium. If they are not cured by this decoction, which often happens in an aggravated state of the disease, they then boil a small quantity of sublimate of mercury, with some fat, in a spoon over a candle, mix it with the beforementioned decoction, and let the patient swallow it. It is no wonder that this rude method should destroy the patient, and put an end to his life by severe torture, which frequently happens. The other method of cure is a more reasonable one, and is effected by administering a cup full or two of the decoction of a species of iris† every morning, detaining the patient in bed. Of

* "Cirsium inerme foliis scabris, lanceolatis, inferioribus ex sinuato dentatis, squamis superioribus calicum subrotundis, membranaceis." Flor. Sibir. tom. ii. p. 72.—Orig.

† Iris foliis linearibus, corollis imberbibus, fructu trigono, caule tereti. Lin. Hort. Cliffort, p. 19. Flor. Sibir. tom. i. p. 27. Iris pratensis angustifolia, non fœtida, altior. C. B. P. p. 32.—Orig.

this they give a greater or less dose in proportion to its operation, which is both by vomit and stool. After having taken it a week, it ceases to have the effect of evacuating; nevertheless they continue it another week; during which time the patient is laid on a heap of fresh burdock leaves, and his body is also covered with the same, which must be renewed every day. This method is said to cure the disease radically.

Russians, Tartars, and other nations in these parts, eat as food, either boiled in milk, or roasted in the embers, various species of the roots of lilies. The Tartars collect and dry the roots of the dens canis* of the botanists, and boil them either with milk or broth, and consider them as very nutritious food. This root certainly is in every respect nearly related to salep.

The Siberian hunters, who kill various animals for their fur, are obliged to go in search of them into the most desert parts of the country, and remain there during their dreadful winters. It often happens that from the intenseness of the cold, the leaven, which ferments their bread, is spoiled, and ceases to be of use. In this case they collect the inner bark of the larch tree, which is very juicy and sweet, and cut it into small pieces, and digest it over the fire in warm water. They then add to it some rye flour, bury the whole in the snow, and let it remain there 12 hours; in which time the fermentation begins, and the fæces, which fall to the bottom, make excellent leaven.

Both the Russians and the people of Kamtschatka made great use of the sphondylium † vulgare hirsutum of Caspar Bauhin and Tournefort; or, what we usually call cow-parsnep. According to Gmelin, the plant in question differs from that species frequently met with in the pastures of Germany and England, only in its being much larger. This difference of size the Russian kind constantly preserves, when planted in the botanic garden. What we generally meet with here in England seldom grows higher than 3 feet, whereas the Siberian plant is double that size. Our author has given us a very exact description of it. This plant, which has never yet been applied to any useful purpose in these parts of the world, is of very great importance to the Russians and people of Kamtschatka. They indeed apply it to very different uses; the former distil their brandy ‡ from it; the latter dry it to eat in winter.

Dodonæus § relates, that the inhabitants of Poland and Lithuania make a kind of liquor, which the poor people use as beer, from the fermented leaves and seeds of the sphondylium.

* Erythronium. Linnæi Hort. Cliff. p. 119. Flor. Sibiric. tom. i. p. 39.—Orig.

† Heracleum foliolis pinnatifidis. Lin. Hort. Cliff. p. 103. Flor. Sibir. tom. i. p. 213. Sphondylium. Rivin. tab. iv.—Orig.

‡ Spiritum ardentem.—Orig.

§ Dodon. Stirp. Histor. p. 304.—Orig.

When Steller, whom Gmelin always mentions with esteem, was at Tobolski in the year 1738, he was informed, that 2 years before they were afflicted there with pestilential carbuncles, which were so contagious as to seize those who approached the person affected. The disease first began in horses and oxen, and afterwards seized the human species. A red spot first was perceptible under the armpits, or in the thigh, attended with great itching; and in a few hours grew to a very large tumour, joined with a burning heat of the part affected; these symptoms were attended with a very acute fever, entire loss of strength, violent pains in the head, and redness of the eyes. An old country practitioner, famous in these parts for his judgment, cured persons labouring under this severe disease in a short time. He used first to the carbuncle the powder of an herb,* of which is given a complete history and figure in this work, made into a thin poultice with dregs † of beer; this poultice, gently warmed, was applied to the part affected, and the patient confined to his bed, who was at liberty to take whatever nourishment he liked, except milk, brandy, or the flesh of pikes. During this time the patient drank plentifully of a decoction of this herb, collected during the time of its flowering; though the powder, applied as above, was prepared from the leaves, before the flower-stalk was produced. The carbuncle, from this treatment, generally broke in 24 hours, and the symptoms greatly abated. The wound was sprinkled with sal ammoniac, and healed in a short time. This disease affected the cattle in different manners; some suddenly set a running with all their swiftness possible, and continued to do so till they dropped down dead; in others, carbuncles arose, which were dressed by the practitioner before-mentioned with the poultice above-described, mixing a large quantity of the herb with their food: and by this method great numbers were cured. A plant so well recommended, and which will grow in our own country, deserves to be better known to us.

Throughout the whole work the author has shown a complete knowledge of the botanic science, among the first professors of which he is deservedly placed.

XXII. On a Mistake of Professor Gmelin, concerning the Sphondylium Vulgare Hirsutum of Caspar Bauhin. By Mr. Philip Miller, F. R. S. p. 153.

Mr. Miller here remarks that in the abstract of the Flora Sibirica, which Mr. Watson laid before the Royal Society, it was mentioned, that the inhabitants of

* Centaurea squamis ovatis, foliis pinnatis, foliolis decurrentibus, linearibus, serratis et integris. Flor. Sibir. tom. ii. p. 89, tab. 41.

Cyanus floridus odoratus Turcicus, seu orientalis major, flore luteo. Hort. Lugd. Bat. p. 211.—Orig.

† Fæce cerevisiæ; though I am inclined to think yeast is intended, which is usually written flos cerevisiæ, or fermentum cerevisiæ.—Orig.

Siberia ate the stalks of the sphondylium hirsutum C. B. P. But Mr. Miller had great reason to believe that Gmelin mistook the species; for he describes that plant as growing upwards of 6 feet high; whereas the common sort seldom rises much above half that height. Therefore probably the plant mentioned by Gmelin was that species, which Breyn mentions in his 2d Prodromus, under the title of sphondylium maximum Transilvanicum Ricini folio, the seeds of which Mr. Miller brought from Dr. Boerhaave's garden in the year 1727, where it was growing near the common sort of Caspar Bauhin, and in the same soil and situation was more than twice the height; and the same difference has continued in the growth of both these plants since, in the Chelsea garden; where the large sort constantly rises to a stem, at least a month sooner in the spring than the common sort, and the leaves are much larger, less divided, and not so hairy; so that there can be no doubt of their being distinct species.

The seeds of that species of Breyn Mr. M. had received from Siberia, by the title of sphondylium vulgare, and Dr. Boerhaave told him, he had received the seeds from Austria, Hungary, and Petersburg, by the same name; so that it is certainly the common sort in those countries. And it is very usual to find many mistakes in the writers on botany; which has happened from their supposing that the plants, which have been mentioned as common in one country, were the same with those of the country where they inhabited. An instance of this was the parietaria minor ocymi folio. C. B. which is the only species found wild in England; and so was by all the English botanists taken for the parietaria officinarum et Dioscoridis C. B. which are distinct species. And many other instances might be mentioned.

XXIII. Of an Eclipse mentioned by Zenophon. By the Rev. G. Costard. p. 155.

The doctrine of eclipses is of great use in history and chronology. The earliest account of any in the Greek history is that said to have been foretold by Thales to the Ionians, which Mr. C. has already treated of. The next, generally taken notice of by writers, is that in the first year of the Peloponnesian war mentioned by Thucydides. But there is another before, which Mr. C. thinks equally remarkable, deserving some further consideration.

It is well known, that Herodotus and other writers make Cyrus to have deposed Astyages. On the contrary, Zenophon says, that Astyages was succeeded by his son Cyaxares, who left the kingdom to Cyrus by will. The truth, Mr. C. thinks, is, that Cyrus did not depose Astyages, and therefore so far Zenophon is right; but deposed Cyaxares, in which he was designedly wrong. That he knew the Persians forced the empire from the Medes, appears from some no very obscure hints even in the Cyropædia itself. After some critical remarks on the situation of certain places mentioned by ancient geographers, and astronomical calculations of eclipses adapted to them, Mr. C. finds that the centre of the shadow passed over Kerkisia, not improbably, the Carchemish of the prophet Jeremiah, and a little to the north of Bagdad. It is not improbable therefore that it crossed the Tigris not far from the place where, it seems by Xenophon's account, Larissa was situated, and where consequently it would cause such a darkness as might well be attended with the effects he mentions.

This eclipse is, Mr. C. thinks, no inconsiderable acquisition to history and chronology, and is at the same time a confirmation of the suspicion, that in these very ancient ones, there is some allowance or other to be made for the influence of some cause, whatever it may be, hitherto not fully determined. This must be left for future observations. In the mean time however it may be of service to the science of astronomy to examine all the past eclipses that can be come at, and compare them with circumstances in the best manner we are able.

XXIV. A new Method of opening the Cornea, in order to Extract the Crystalline Humour. By Mr. Samuel Sharp, F. R. S. Surgeon to Guy's Hospital.* p. 161.

The operation of discharging the crystalline humour from the eye, for the cure of that species of blindness called a cataract, was a few years since invented by Mons. Daviel, who performed it on many patients with remarkable success. Supposing it therefore admitted, that the extraction of the crystalline humour has been found by experience to be a useful method of cure, Mr. S. here submits to the Society a new manner of making the incision of the cornea, by which Mons. Daviel's operation may be very much shortened, the patient would suffer less pain, and every skilful operator be equal to the undertaking.

Place the patient in the same situation as for couching, either opening the eyelids with your fore finger and thumb, or letting an assistant raise the upper eyelid, while you yourself keep down the under eyelid. Then, with a small knife, holding its edge downwards, make a puncture through the cornea near its circumference, into the anterior chamber of the eye, in such a direction, as to carry it horizontally, and opposite to the transverse diameter of the pupil; after which you are to pass it towards the nose, through the cornea from within outwards, as near to its circumference as in the first puncture. When you have made the second puncture, push the extremity of the blade one-seventh of an inch beyond the surface of the cornea, and immediately cut the cornea downwards, drawing the knife towards you as you make the incision. After this, you press gently with your thumb against the inferior part of the globe of the eye, in order to expel the cataract, and the operation finishes, according to the different circumstances, as in the manner proposed by Mons. Daviel.

One extraordinary benefit seems to arise from the use of this single instrument,

* Author of 2 publications much esteemed, viz. one On the Operations of Surgery 1743, and another entitled Critical Inquiry into the present State of Surgery, 1750.

and perhaps from the shape of its blade, which increases in breadth all the way towards the handle; for, by this means, the punctures are so exactly filled up by the blade, that very little of the aqueous humour is discharged before you begin to make the incision, and consequently during this time, the cornea preserves its convexity; whereas by using one instrument to puncture, and others to dilate, the cornea immediately becomes flaccid on the issue of the aqueous humour, and renders the operation tedious and embarrassing, as he himself had found by experience in one patient, on whom he had performed the incision of the cornea with a pair of scissars, as recommended by Mons. Daviel.

XXV. Experiments on Fish and Flesh preserved in Lime-water. By Francis Hume, M. D. p. 163.

With a design to find out how long he could keep fish and flesh fit to eat in lime-water, Dr. H. put two haddocks, and a pound of beef, in different pots full of lime-water, and corked them well, setting them in a cellar 18 days. He then took out one of the fish; it was sweet, sound, and firm. He boiled one part of it, and he broiled the other; it eat well, and had not the least taste of lime-water; but was not quite so firm as a fresh fish. But when he opened the beef-pot, to his great surprise, it stunk abominably. He poured the lime-water from both pots, and put in fresh lime-water. This stood 4 weeks longer; the remaining fish was quite fresh, and a little swelled, but when boiled, it dissolved to a jelly. The flesh was very putrid.

Thus lime-water appears to preserve fish, but not flesh.

Dr. Alston's experiment was made with fish, and Dr. Pringle's with flesh; which made the former say, that lime-water withstood corruption strongly; and that the latter did it but weakly, if at all.

Dr. H. afterwards repeated the experiment more fully, and with the same success. On the 26th of March, he put a haddock into a pot of common water. He did the same to a piece of beef: the water was changed every day. At the same time he put a haddock into a pot of lime-water, and did the same with a piece of beef; at the same time he hung a fish and a bit of flesh in the air. On the 2d of April the fish and flesh in the air were a little corrupted and dried; the flesh and fish in common water smelt strong; the fish in the lime-water was sweet, and the lime-water good, and are so at present, April 6; but the flesh smelt rather worse than that in common water changed every day, and the corruption had quite overpowered the smell of the lime-water.

XXVI. A Letter from Mr. James Short, F. R. S. to the Earl of Macclesfield, P. R. S. concerning a Paper of the late Servington Savery, Esq. relating to his Invention of a New Micrometer. Dated May 10, 1753. p. 165.

It is now above a year, Mr. Short says, since he received a letter from the Rev. F. Pezenas, professor of hydrography to the French king at Marseilles, in which he informed him, that M. Bouguer had read, before the Royal Academy of Sciences at Paris, in the year 1748, a memoir, in which he describes an heliometer; which is an instrument consisting of 2 objective glasses, for measuring the diameters of the planets. He said also, that this memoir was actually in the hands of M. de Fouchy, perpetual secretary of the Academy, or at the Royal Printing-house; and that it was registered in the minutes of the Academy for the year 1748.

Immediately after reading this letter, Mr. S. recollected to have heard a paper on the same subject, from the late Servington Savery, of Exeter, Esq. read before the Royal Society, about the year 1743. He therefore had recourse to the minute book of the Society for that year, where he found the following minute, which he copied in the presence of Lord Charles Cavendish, then vice-president.

"A paper communicated from Mr. Savery at Exon, containing a new method for measuring the difference between the apogeal and perigeal diameters of the sun was shown; and thanks being ordered, Dr. Bradley was desired to oblige the Society with an account of its contents.—Oct. 27, 1743."

On application to Dr. Bradley, he sent the original paper to Mr. Short; on the back of which was a memorandum in the hand-writing of the late president, Martin Folkes, Esq. as a further proof of its authenticity, which runs in these words,

"Delivered to me by Mr. Graham, sealed up by the author, and then broke open in his presence ☿ 26th Oct. 1743. M: Folkes."

Mr. Savery's original paper was as follows:

A new Way of Measuring the Difference between the Apparent Diameter of the Sun at the Times of the Earth's Perihelion and Aphelion, or when the Sun is nearer to or farther from the Earth, with a Micrometer placed in a Telescope Invented for that Purpose; though the Charge or Magnifying Power of the Telescope is so great, that the whole Sun's Diameter does not appear in it at one View. By Servington Savery, of Exeter, Esq. Read Oct. 27, 1743. p. 167.

Though this may appear impossible, yet Mr. S. has contrived some dioptric telescopes, and a reflecting one; either of which, by representing the object double, will, if well made, answer the design.

Fig. 1, pl. 10, represents the whole body of the sun, as it appears double, and magnified in the telescope. Let an be the diameter of the one, and rx of the other image of the sun in perigæo; so shall nr be the distance between the two images at that time; which measured with the micrometer is equal to, suppose, 10 seconds. Let bm be the diameter of the one solar image, and sw of the other, when in apogæo; so shall ms be the then distance of the solar images, measuring with the micrometer, suppose, 1′ 10″. The difference of these two observations, 1 minute, is the apparent diminution of the sun's diameter.

The little circle, whose diameter is dt, is the whole area visible at once in the telescope, which is not a 3d part of the magnified diameter of the sun; but since both nr at one time, and ms at another time, are visible within the telescope's area, if good instruments are procured, Mr. S. sees no difficulty in performing what he has proposed above, more accurately than it has ever yet been done, except this one (which some time since Mr. Graham mentioned in a letter to him) viz. that of defining the sun's disk truly; and Mr. S. thinks, to do that to good perfection, is beyond human art. A telescope for this use may be made to magnify the sun's diameter to any degree whatever, not exceeding such degree as will make any part of the line ms fall without the area of the telescope: and he thinks it will be very difficult to make one with a charge so great, as not to have more than a geometrical minute of the sun's apparent diameter visible at once.

Since the sun is an object so very remote, the pencil of rays flowing from the centre of its disk, and incident all over an object-lens, though it should be a foot broad, would not differ sensibly from a perfect cylinder within the distance of above 100 miles from its basis at the lens; though in reality the whole pencil is an acute cone, whose angle at the vertex is almost evanescent. Hence it follows, that if the two poles of two equal object-glasses are placed at the distance, suppose, of a foot from each other, the two centres c, v, of the two solar images must, as to sense, remain always at that very same distance, viz. 1 foot from each other, though the sun should be placed 10 times as far off as it now is; but since the sun's greater distance would diminish the diameters of both of the solar images; mn, added to rs, must be the true difference of the apparent diameters of the images, and also of the sun, at different times.

According to Mr. Azout, Harris's Lexic. Techn. vol. i, see sun, the apparent diameter of the sun never exceeds 32′ 45″; hence its radius never exceeds 16′ 22″ 30‴; the tangent of which is about 476328, to the radius 100,000,000. Then, as the said tangent : to the said radius :: so half an inch : to 104.96 inches, and decimal parts. According to this, if the focal length of a lens be 104.96 inches and parts, it cannot collect the sun's rays to a less focus at the time of his perigee, than 1 inch in diameter, or half an inch radius.

In fig. 2, the whole circle represents a well centred object-lens, whose focal length is, as above calculated, 104.96 inches and parts, or rather a little less; that the two images may be sure not to touch each other. Let the two diameters dm, qf, divide it into 4 quadrants, but the diameter qf must be occult, or delible. Let cw be half an inch, and cv equal to it. Through v, and also through w, let a chord line be drawn parallel to the diameter dm, viz. bg, hp. Through the said chord lines bg and hp, and also through the diameter dm, divide the lens into 4 parts.

Fig. 3, Let the straight edge of the frustum bvgq, in the preceding figure, be cemented fast to that of the similar frustum hwpf of the same lens, as they appear in this fig. 3. Having then with barm fastened a white paper all over both sides of the lens, he made for trial (which he did, not only to secure the cemented joint from breaking, but to prevent the injury which the polish might receive in cutting and grinding the edges) he described a circle qmnf on the centre c, fit for the tube he had to put in it; and having made it round, and washed it clean, after the edges were ground true, that nothing sandy might hurt the polish, he soaked it in clean water, till he could easily take off the paper. This model, made of a spectacle glass about 12 or 13 inches focus, gave him encouragement to try the following one, which he thought better.

Fig. 4, Mr. S. made his second model of the 2 middle frustums mcdhwp, mcdbvg, of the lens in fig. 2, by cementing their edges, hwp, bvg, together, as they are placed in the present fig. 4, so the pole c of each part must consequently be half an inch, supposing its focal length is about 104 inches, from the middle where c stood in fig. 2, viz. the pole of one frustum where v, and of the other where w now stands. He left open at each pole a semicircular aperture rwq, svt, about $\frac{4}{5}$ of an inch diameter, and covered all the rest of the circle axlkzo, to which he had cut it fit for the tube. The focus of the lens he made it of, was about 3 feet.

Note, The rays of red light in the two solar images will be next to each other in both these models, which, he thinks, will render the sun's disk more easy to be observed than the violet ones. This he mentions, because the glasses in these two sorts are somewhat prismatical, but mostly those of the first model, which could therefore bear no great charge. Also the frustum on the right hand of the first model renders the solar image at the focus on the left, and that on the left hand renders it on the right; but it is not so with the second model, or with the next contrivance, which is the best, if well made.

Fig. 5, In this, the greatest difficulty consists in getting two well-centred object-glasses, whose focal lengths are equal; for it is necessary they should be so, because they are to be combined with the same convex eye-lens, common to them, at the same distance. ab is the diameter of a plain brass plate, which may be $2\frac{1}{2}$ inches broad, or somewhat less; two short equal cylindric brass tubes mn, rs, must be fastened on it, with their centre pc, equidistant from the centre l of the plate, and distant 1 inch from each other in the diameter ab, as the figure shows. In the tubes must be put two equal object-glasses of the focal length of $104\frac{9}{10}$ inches, or rather somewhat less, as aforesaid. Through the plate there must be made, in the middle of each tube, a round aperture, viz. hg, wx, whose diameters must be proportioned to the focal length of the eye-lens, and not exceed the third part of it, lest the object appear confused.

And since it is scarcely possible to centre an object-lens to very good perfection, those in the two cylinders, may happen to render the two solar images at too great a distance from, or too near to each other. But this fault, if not too great, may be remedied, by turning one or both of the lenses a little way round; and then their eccentric poles will by that means be brought nearer to, or farther from each other; and when they are once well placed, there should be a mark made in each lens, and its cylinder; that if it is taken out to be wiped, it may be put in again the same way. There should also be a different mark in one of the glasses, that each may know its own cylinder. They must both of them be very close all round to their respective cylinders; otherwise one lens may slide nearer to or farther from the other; which if it should in the least degree, between the first and second observation, all the labour would be lost. Either of these three parts of double lenses may be combined with a convex eye-lens as usual, and have a micrometer placed at the common focus.

Such a double lens, of either sort, may be proved whether it is well composed or not, without the trouble of combining it with its eye lens, by holding it in the sun's rays, as one would a burning glass, and applying a piece of white paper at its focus, where, he apprehends, the two solar images will appear as distinct as when an eye-lens is applied, though not so large; and each of them 1 inch broad, if the focal length be as above, i. e. almost $104\frac{9}{10}$ inches. After the same manner may the double object-mirror of a reflecting telescope for this use be proved.

In fig. 6, the circle bdhpmg is the circumference of a concave mirror made of black glass: it must be very thick, that it may not spring or bend with any thing that presses on it to keep it fast, for that may injure its concavity. The circle within it, on the same centre c, shows that its concavity must not be continued quite home to the very edge of the mirror, but the little space between the two circles must be ground very true on a plain. The pricked lines must not be drawn; they are only to indicate where the poles vw of the two frustums must be brought, after the mirror is diametrically bisected. Let the concave side be defended, by pasting a paper all over it, and then let it be divided with a saw in the diameter dcm; taking care that the said diameter be in the middle of the kerf, which may be as broad as the space between the lines ao, eq. Let the asperities of the edges of both frustums be ground off, that they may be very straight after being sawed.

Fig. 7, represents a thick round plate of brass, very plain, and equally thick all over, having lines drawn on it, as on fig. 2, also one line on each side of the diameter dm, equidistant from, and parallel to it. The distance of these two lines ao, eq, from each other equal to the kerf of the saw, which divided the mirror. The diameter of this plate must be equal to that of the mirror before it was divided.

On the under side of the plate must be two pins fastened t, t, their diameters equal to the kerf of the saw, that they may keep the two frustums of the mirror at the same distance from each other that they were before their division; so shall their circular edges be extended as far as the circumference of the plate, and their straight edges touch the said pins in the lines ao, eq.

The end of the tube must be turned on the inside exactly to fit the plate and mirror, that they may not slide any way, for that would spoil the observations.

In the diameter of the plate rs, on the points v, w, distant half an inch from c, the centre of the plate, and a whole inch from each other, let a circle, for the aperture of each frustum, of a proper size, according to the intended charge of the telescope, be described, and cut out. Also in the said diameter, equidistant from the centre c, viz. at x and z, let there be a screw for each frustum, to elevate it a little from the plate, as shall be needful. Let there be a spring contrived to press on the back of the one frustum ora, against the point v, being the middle between the edge ao, and the screw x, to keep the frustum close to the plate at the points a, o, and also close to the screw x, when it is screwed in. Let the like be also done on the back of the other frustum esq.

Then, 1, before the two screws are put in at x, z, the two frustums of the mirror will lie plain on the plate of brass, and have one pole at c common to them, and consequently will collect all rays which, during their incidence, are parallel to the axis of the tube, to one common focus in the said axis of the tube, just as they would have done before the mirror was divided. 2. But when the two screws xz are put in their places, and screwed a little way through the brass plate, they will lift the two frustums free from the plate at their circular edges, viz. at r and s, while their straight edges ao, eq, are kept to touch the plate with both their ends (not in the middle, by reason of the mirror's concavity) by the pressure of the springs, as mentioned above. By this means the pole c of the frustum ora, will be removed from c towards r, and likewise the pole c of the other frustum esq be removed from c towards s, more or less according to the quantity of the elevation of each frustum, by the screw that raises it; so that now there will appear at the focus two solar images; whereas there was but one, before the screws were put in.

By moving the screws, the two solar images may be brought to any distance from each other; but care must be taken not to raise one frustum more than the other, and the two solar images must almost touch one another at the time of the perigee; otherwise it must be better adjusted.

This telescope may be finished with a small elliptical specillum of black glass, ground plain on its reflecting surface, and a convex eye-lens, like that described by J. Hadley, Esq. F. R. S. in Phil. Trans. N° 376. A micrometer may be contrived for it at the common focus, near the eye-lens.

Such a double-object speculum would be capable of a vast improvement, by combining it with a concave specillum, which would reflect the images through a hole in the centre c of the said speculum to fall on a convex eye-lens, after the manner of our new sort of reflecting telescopes, were it not for the difficulty of adapting such a micrometer to it as would exactly measure minutes and seconds; for the eye-glasses of such having usually a pretty large focal length, would bear much larger divisions on a micrometer, than Mr. Hadley's with a small eye-glass can do, though their charges should be equal, or that of the former did exceed.

Finding that large object-glasses for telescopes are not commonly well centered, with their poles in the very middle of them, gives the following a rule for centering optic-glasses; which may be very ready for a glass-grinder's use, and soon try whether a convex lens is well centered.

Fig. 8, represents a round plate of brass, conveniently thick, and well hardened by hammering, having many notches round it, one a little wider than that which is next to it, and numbered 1, 2, 3, &c. in their proper order, each of them wider at the bottom than at the entrance. He fitted such a notch to the thickest side of one of the glasses he had received from London, so as the edge entered it but a little way, not half its depth; but, on trying the opposite side, it went in, the whole depth, and would have gone deeper, if the notch had been so cut: he then ground the lens narrower on that side which was thinnest, till he found it was at that place as thick as where he first tried it in the notch. After this manner he reduced the glass to an equal thickness on its 4 quarters, and then ground off from other places what was needful to bring it circular. He also took care, when he tried it in the notch, that the lens should not be warmer on the one side than on the other by grinding, but stopped till it was thoroughly cold; and was also careful not to thrust it in harder on the one side than on the opposite side; for he could plainly observe a difference afterward, if he neglected to mind both these circumstances, or indeed either of them.*

XXVII. *Of a Contrivance for Measuring Small Angles. By Mr. John Dollond.* p. 178.

Let an object-glass, of any convenient focal length (being truly ground and

* Dr. Smith, in his Complete System of Optics, published in 1738, has described a very accurate and ready method of centering object-glasses, which was always used by the late Mr. George Graham, from whom the Doctor had it.—Orig.

well centered) be divided into 2 equal parts or segments, by cutting it straight through the centre; and let a piece of machinery be so contrived, as to hold these two segments in the same position to each other, as they stood in before they were cut asunder; and to be capable at the same time of drawing them to different distances from that position, in the manner as represented in fig. 7, pl. 8.

Each of those segments will form a distinct image of any object, to which they are directed; differing in nothing from that which might have been made by the whole glass before it was cut, except in brightness. And while these segments are held in their original position, the images will coincide, and become one single image as at first; but in proportion as they are drawn off from that situation, the images will separate more or less, according to the distance they are drawn to. By this means the images of two different objects, or of different parts of the same object, not very far from each other, may be brought to a contact or coincidence at the focus: and this coincidence may be viewed to a very great nicety with a proper eye-glass.

The measure of the angle subtended by the two objects, whose images are thus brought to a coincidence, depends on 3 things: 1st, a careful observation of the coincidence of the images: 2dly, an exact measure of the distance, which the glasses are drawn out to, from that situation which makes the image single: and lastly, a true knowledge of the focal distance of the glass. How the angle is to be found from these measures, and how it may likewise be come at, by viewing two land-objects at a convenient distance, will be shown hereafter in the explanation of the figure. It is easy to understand, in the mean time, that the angle will be measured with more accuracy, in proportion to the length of the glass which is used for that purpose; but the difficulty of managing long telescopes is no less apparent. Therefore the most practicable method of using this micrometer to advantage, is to apply the divided object-glass to the object-end of a reflecting telescope: for, as the apertures of this sort of telescopes are large in proportion to their lengths, they will admit of very long glasses; nor will the measures be any way affected by the metals or glasses, which the reflector is composed of: and the angles will be found in the same manner, as though the images were viewed with a single eye-glass, in the manner of a common refracting astronomical telescope; but with this advantage, that as the images will be exhibited larger and distincter by the reflecting telescope; and as every part of it will be much more manageable than a long refracting telescope; so the contact or coincidence of the images will be more accurately observed.

Explanation of the Figure.

The two semicircles represent the two segments of the object-glass, whose centres C and D are drawn off to the distance CD, and the points A and B are two objects, or different parts of the same object; therefore the lines ACG and BDG represent two rays that pass through the centres or poles of the segments, and are therefore not at all refracted, but go straight through to G, where they intersect; and G being the respective focus to the distance of the objects from the glass, the two images will coincide at that point. It appears from the figure, that AB : CD :: GH : GE; and from a common proportion in optics, GH : GE :: HE : EF. Therefore AB : CD :: HE : EF; F being the focus of parallel rays; and consequently the angles AEB and CFD are equal. That is, the angle subtended by the distance of the centres of the segments from the distance of the focus of parallel rays, is equal to the angle subtended by the distance between the objects A and B from the end of the telescope.

XXVIII. On the Copper Springs in Wicklow in Ireland. By John Bond, M. D. p. 181.

A spring of water flows from a rich copper mine, and is of a sharp acid taste, and light-blue colour. It is received and collected in pits, where iron bars are placed, which, after lying in the water about 3 months, are entirely consumed, and at the bottom of the pits, a quantity of copper, greater than that of the iron is found, in the form of coarse sand. This fact is confirmed by profitable experiments, often repeated since the discovery, the honour of which is due to Mr. Matthew Johnston, a worthy old gentleman, and one of the proprietors of the mine, who first proposed this method of collecting the copper.

Experiment 1. Into some of this water, taken out of the stream above the pits where the iron bars are placed, he poured a solution of an alkaline salt, which raised a strong effervescence, and precipitated a large quantity of a dark-brown substance. Which showed that the water contained a strong acid, with a solution of the substance precipitated.

Exp. 2. He put some aqua-fortis, or spirit of nitre, into water taken out of the same place; and observed, that the strong acid immediately destroyed the blue colour. Whence he concluded that the substance, which was precipitated by the alkali in the first experiment, was so perfectly dissolved by the acid spirit in the 2d, as to transmit all the rays of light.

Exp. 3. Some small iron nails put into this water, were in 4 minutes so closely covered with some substance of a copper colour, that, with a magnifier of $\frac{1}{2}$ inch focus, he could not discern the iron through it. In that time the nails gained 4 grs. The water had the same effect on silver and tin, but not on gold. The colour and increase of weight were owing to the adhesion of the particles of the matter dissolved in the water by an acid, that could not penetrate gold.

Exp. 4. In order to determine the quantity and quality of the matter in the water, he put 2 drs. of small iron nails into 3 oz. of it, and let them stand 24 hours; then examined, and found the surface of the water covered with a thick scum, like that of a chalybeate spa. It lost the blue colour, and sharp vitriolic taste. It was quite transparent, and at the bottom there was a quantity of a brown ponderous powder, which, when dried, weighed 14 grs. This powder melted without any flux, produced 12 grs. of pure copper. The nails lost 8 grs. in the water, and were, in several places, covered with a solid lamina of pure copper. The water, in which the nails lay, after being filtrated and evaporated, afforded a green vitriol, which in every respect resembled sal martis, and produced the same effects, when dissolved, and mixed with any astringent tincture.

Exp. 5. From the spring water treated in the same manner, he obtained a blue vitriol, the basis of which is copper.

From all these experiments he infers, that a mineral acid is the active quality in this water; which being diffused through the copper ore, unites itself with that metal, and forms a vitriol, which is dissolved by the water, and remains suspended in it, till it meets with iron in the pits, by which this acid is more strongly attracted than by the copper, therefore it quits the copper, corrodes the iron, and changes it into a vitriol, which is again dissolved, and carried off in the stream continually flowing from the pits; while the copper, deserted by the acid, falls, by its specific gravity, to the bottom of the pits.

By this account it is evident, that this admirable process is a simple precipitation of the copper, by means of the iron. Hence it has been improperly called a transmutation of iron into copper. But, lest any difficulty should still remain, concerning the consumption of the large quantities of iron put into the pits, he adds the following observations, to show that it is dissolved and carried off in the water.

Observation 1. The water in the pits are covered with a thick scum, occasioned by the air bubbles constantly rising, and bursting on the surface; which is an evident sign of the solution of the iron.—*Obs.* 2. The iron is gradually consumed in the pits, and abounds with irregular depressions, like old iron: a strong symptom of its being corroded by an acid.—*Obs.* 3. The channel of the stream running from the pits is furred with red ochre, which, after being roasted in a strong fire, was attracted by the magnet. As this ochre is only found in the stream below the pits, it appears to be part of the iron dissolved in the water.—*Obs.* 4. The quantity of copper found in the pits, after the iron disappears, is generally greater than that of the iron when first put in: for the proprietors assured him, that sometimes a ton of iron will produce, or rather precipitate, a ton and half of copper. This fact alone would be sufficient to prove, that the iron is not converted into copper; since, according to Sir Isaac Newton's table, the specific gravity of copper is to that of iron as 9000 to 7645.

When he was at this spring in August last, it ran at the rate of 12 oz. every second; and by putting iron into the water of the stream running from the pits, he found, that every ounce contained 3 grs. of copper. Hence by calculation it appears, that 129600 grains of copper are carried off every minute, and consequently 124100 lb. Troy weight in a year; supposing the quantity and quality of the water to continue the same. Hence we may easily account for the death of the fish, and other phenomena in the river, which receives this vitriolic stream.

In a hot sunny day, when the water is exhaled, the heaps of mould, raised out of the ore-pits, are covered with a vitriolic efflorescence: hence, in rainy weather, the water appears like a strong solution of verdigrise. Whoever is desirous to imitate the process carried on in these pits, may readily gratify his curiosity, by putting pieces of iron into strong solution of vitriol. It is a common experiment, to tinge polished iron by rubbing it with Roman vitriol; which depends on the cause before-mentioned; viz. the acid in the vitriol penetrates the iron, and leaves the copper on the surface. This experiment is also taken notice of by that excellent chemist, and celebrated philosopher, Mr. Boyle, who calls it a sympathetic precipitation, in his Essay on specific Medicines.

As soon as the attraction between the copper and the acid ceases, the mutual attraction between the minute particles of the metal prevails, so as to form large solid masses at the bottom of the pits, $\frac{19}{20}$ of which are pure copper. These solid masses are partly occasioned by the pressure of the incumbent heap of granulated copper, constantly increasing. Hence we see, that the art of essaying, or separating metals from their ores, chiefly consists in evaporating an acid, which prevents the mutual attraction of the metallic particles: for when the acid is driven off by the violence of fire, the particles fall into their proper sphere of attraction, and assume a solid form. From what has been offered on the theory of this admirable process, several practical hints may be taken to render such springs more profitable; and perhaps an easier method may be discovered of separating copper from its ore, by precipitation, than by calcination.

This spring perhaps is as remarkable for its medicinal as its metallic qualities. Though physicians generally reckon copper, taken internally, poisonous, yet the miners and other people drink this water frequently, without any ill consequences. It purges and vomits severely, and is become their specific in several diseases, particularly in cutaneous eruptions, arising either from an alkaline acrimony in the blood, which stimulates the sensible extremities of the cutaneous arteries, and occasions a pustule, or from the irritation of insects lodged in the skin; both which causes may be removed by the strong acid in this water. It is an excellent detergent for scorbutic ulcers, as Hoffman justly observes. It has already performed several remarkable cures of this kind. Dr. B. had often recommended it in such cases with success, joined with proper internal medicines. How far the success of practice in the miners, who drink it frequently, might be depended on, longer experience must determine. Certainly, a great allowance must be made for the strength of their constitutions, and the insensibility of their nerves, constantly exposed to the noxious steams of damp pits. He never ventured to prescribe it internally: and as the materia medica affords vomits and purges of a more innocent kind, he thought it in that respect unnecessary. He had reason to imagine, from the effects which this water had on some earth-worms, that it was a very powerful anthelminthic, if cautiously given.

Some fresh filings of iron, put in this water, soon precipitated all the copper, and made it a strong and agreeable chalybeate. Hence it might be used as a substitute for spa-water, the virtue of which depends on the iron. Some prepared filings of iron remained 8 days in this water; without producing the least alteration. Hence it appeared, that this medicine could have but a weak effect, if any at all, in absorbing acids in the first passages.

XXIX. On Mr. Gascoigne's Invention of the Micrometer. By Doctor Bevis, p. 190.

Though Mr. Townley, in his paper printed in the Philos. Trans. N° 25, p. 457, has sufficiently made it appear that the invention of the micrometer was Mr. Gascoigne's, and that he applied it to measuring small angles in the heavens, and for settling the moon's parallax, long before Messieurs Auzout and Picard thought of any such matters; yet are the French astronomers apt to ascribe it to their countrymen, without so much as once mentioning the name of Mr. Gascoigne. No sooner had the late Dr. Derham restored the application of telescopic sights to quadrants to its true author Mr. Gascoigne, than M. de la Hire, who never made the doctor any reply on that head, took occasion, in the memoirs of the Royal Academy of Sciences for 1717, to ascribe this contrivance of the micrometer to M. Auzout, in conjunction with M. Picard; alleging, for proof, an extract of a letter, dated Dec. 28, 1666, from M. Auzout to M. Oldenburg, and printed in the Phil. Trans. N° 21. Several others have since copied M. de la Hire's assertion, and last of all, M. Bouguer, in the memoirs of the Royal Academy of Sciences for 1748, lately published, where he describes an instrument which he calls a heliometer; the contrivance of which seems in every respect the same as that sent about 10 years ago to the Royal Society, by Servington Savery, Esq.

I have now, says Dr. B. before me the copy of a letter of Mr. Gascoigne to Mr. Oughtred, which I made myself from the original, written in 1640-1; which original was in the possession of the late William Jones, Esq. F. R. S. and is now in the library of the Earl of Macclesfield. It consists of several sheets of paper, all about his invention for measuring small angles to seconds; where he not only gives the geometrical and optical principles of his contrivance, and the construction of the instrument, but also a series of observations actually taken with it; some of which I shall transcribe.

1640, Aug. 5.	Jupiter's diameter		0′ 51″
	Mars's		0 38
Dec. 24.	Mars's		0 25
	Venus's		0 25
1640, Aug. 25.	Moon's semidiam.	h. 8 p. m.	15 17
Sept. 19.			15 11
Oct. 9.		h. 8 p. m.	16 36
10.			16 36
27.		h. 7 p. m.	15 38
29.			15 41
30.			15 43
31.			15 49

These may suffice to prove that Mr. Gascoigne's micrometer was not a thing merely in embryo, but brought to a good degree of perfection above 40 years before that of the French gentlemen was ever so much as mentioned.

XXX. Observations of the Transit of Mercury over the Sun, May 6, 1753. *By Mr. John Short, F. R. S.* p. 192.

The instrument prepared for these observations was a reflecting telescope, of 2 feet focal length, of the Gregorian form, magnifying about 65 times, and so constructed in its machinery as to move in a plane parallel to the horizon, and also to move in a plane parallel to the equator. This telescope had 2 eye-pieces, each a combination of 2 glasses, viz. one eye-piece for the horizontal motion, with wires at right angles to each other, the wires being between the glasses, and one of the wires placed parallel to the horizon, and consequently the other was vertical; the other eye-piece was also a combination of 2 glasses, and adapted to a micrometer, the glasses being placed between the wires of the micrometer, and the eye of the observer, and was to be used when the telescope moved in a plane parallel to the equator.

Mr. Short's house in Surry-street, being so situated as not to see the sun at rising, the Rev. Dr. Birch, was pleased to allow the use of the leads on his house in Norfolk-street, from whence we should be enabled to see the sun soon after he rose. The doctor's leads were chosen, as being not far from Mr. Short's clock, which was easily within call, in order to compare a second-watch with the clock, at every observation.

About half an hour after 4 in the morning of the 6th of May, Dr. Bevis and Mr. Short went to Dr. Birch's house, where a series of observations was taken with the above reflecting telescope moving parallel to the horizon, and the eye-piece with the wires at right angles; Dr. Bevis observing, and Mr. Short writing down the times. These were observations of the passage of the limbs of the sun and mercury, by the vertical and horizontal wires of the telescope.

About half an hour after 6, Dr. Bevis and Mr. Short went to Mr. Short's house, the sun then shining into his windows, in order to be near the clock, where observations were made in the same manner as before, Dr. Bevis observing, and Mr. Short writing down the times.

The telescope, which hitherto had moved parallel to the horizon, was now altered, to move parallel to the equator, and the eye-piece with the micrometer was applied, about half an hour after 7 o'clock, Mr. Short observing, and Dr. Bevis writing down the times of observation.

Mr. Short observed the last internal contact of mercury with the sun, with a 4-foot focal length reflector, magnifying about 135 times, at $10^h\ 5'\ 7''$ by the clock, uncertain to 2 or $3''$; and the total egress at $10^h\ 7'\ 42''$ by the clock; uncertain to 5 or $6''$, the air then undulating through thin clouds.

Dr. Bevis observed the last internal contact with a 2-foot focal length reflector, magnifying about 65 times, at $10^h\ 5'$, and the total egress at $10^h\ 7'\ 38''$, by the clock.

Mr. Sisson, at Beaufort Buildings in the Strand, observed the total egress at $10^h\ 7'\ 43''$, by Mr. Short's clock, through a 5-foot refracting telescope.

Mr. Bird, at his house in York-buildings, observed the last internal contact at $10^h\ 4'\ 57''$; and the total egress at $10^h\ 7'\ 43''$, by Mr. Short's clock, through a 9-inch focal length reflector.

Mr. Smeaton in Furnival's-Inn-Court, Holborn, observed the total egress at $10^h\ 8'\ 30''$, by Mr. Short's clock, through a 6-foot refracting telescope.—He suspects his time some seconds too late, a cloud having just passed off the sun, when he perceived Mercury was gone.

Mr. Canton, in Spital-square, observed the total egress at $10^h\ 8'\ 12''$, mean time, through a reflecting telescope, 3-foot focal length.

M. Short's house in Surry street, is 26^s of time west of the Royal Observatory at Greenwich.

N. B. Mr. Short's clock, by which these observations were made, was found to be 28^s slower than mean time.

May 7, Sun's preceding limb passed the meridian at. . . . . . $11^h\ 54'\ 38''$
Subsequent limb passed the meridian. . . . . 11 56 $51\frac{1}{2}$
May 8, Sun's preceding limb passed the meridian at. . . . . . 11 54 $34\frac{1}{2}$
Subsequent limb passed the meridian at. . . . . 11 56 $47\frac{1}{2}$

and this clock, by repeated observations, was not found to have varied above 1^s since the 22d of February last to the day of observation.

In the observatory of the Earl of Macclesfield, at Shirburn Castle in Oxfordshire, the total egress was observed at $10^h\ 8'\ 11''$, apparent time. Latitude of Shirburn Castle is $51°\ 39'\ 25''$, and its longitude is $4^m\ 0^s$ of time, west of Greenwich observatory.

XXXII. Account of a Treatise, intitled, "Letters concerning Electricity; in which the latest Discoveries on this Subject, and the Consequences which may be deduced from them, are examined; by the Abbé Nollet, Mem. of the Royal Acad. of Sciences of Paris, F. R. S. &c." Extracted and Translated from the French, by Mr. William Watson, F. R. S. p. 201.

This treatise is the production of a great master on the subject of electricity: he has already published two volumes expressly on it, besides several memoirs among the works of the Royal Acad. of Sciences at Paris, as well as several valuable papers to the R. S.

The discoveries made in the summer of the year 1752 will make it memorable in the history of electricity. These have opened a new field to philosophers, and have given them room to hope, that what they have learned before in their museums, they may apply, with more propriety than they hitherto could have done, in illustrating the nature and effects of thunder; a phenomenon hitherto almost inaccessible to their inquiries.

These considerations have induced our author to examine with care, what may truly be concluded from the experiments proposed by Mr. Franklin of Philadelphia, and since carried into execution in France, and elsewhere, in regard to the electricity of the clouds during a storm; by weighing every circumstance, and comparing the magnitude of the effects, with the more than apparent insufficiency of the means, which have been employed to produce them. He thinks, he sees clearly, that considering the electrization of pointed bodies as a proof of lessening the matter of thunder, is abusing a real discovery to flatter ourselves with a vain hope; and it is chiefly to dissipate this error, if it yet subsists, that determined our author to print, in the work before us, some reflexions, which he had made at first only for himself, and a few persons, to whom he was desirous of communicating his opinion.

Mr. Franklin's treatise on electricity contains many very curious experiments; but the deductions from them being different from those which the Abbé Nollet has given on the same subjects, it might be imagined, if he were silent on this head, that he had given up his former opinions. This has been one motive for the present publication, which the author is desirous should be considered, less as a criticism on Mr. Franklin's doctrine, than as a defence of his own.

In some parts of these letters, the author mentions an electricity, which is very often, and perhaps always, in our atmosphere, when there is no appearance of thunder. He speaks of this, as if he only suspected it, and in a manner as if it wanted confirmation. He was then unacquainted with some decisive experiments made on this subject by le Monnier,* at St. Germain-en-laye, and which have been just published. He now considers, as a thing certain, that electricity is a very common meteor, which may manifest itself, when the weather is most serene; and that thunder is, strictly speaking, only one of its modifications, which renders it more sensible to us.

The Abbé Nollet's treatise contains 9 letters; 6 of which are addressed to Mr. Franklin, one to Mad^{lle.} Ardinghelli, who, when only 16 years old, translated Dr. Hales's treatise of Hæmastatics into Italian, and added to it some very ingenious remarks; one to Mr. Jallabert of Geneva, and one to Mr. Boze of Wittemberg: to these are added some experiments in electricity, made in support of opinions, laid down in this work, in the presence of Messrs Bouguer, de Montigny, de Courtivron, d'Alembert, and le Roi, who were appointed by the Royal Academy of Sciences for that purpose.

In the first letter the author gives his correspondent Mad^{lle.} Ardinghelli an account of the discoveries in electricity in the year 1752; among which he takes particular notice of the experiment made on May 10, at Marly-la-ville, in consequence of Mr. Franklin's hypothesis; when pointed non-electrics, supported by electrics per se, gave manifest signs of electricity during a thunder storm. This experiment, in the letters to Mr. Collinson, Mr. Franklin had proposed, but, as far as may be judged, had not then carried into execution. The experiment of Marly-la-ville was soon after verified by le Monnier at St. Germain-en-laye, who found further, first, that the like effects were produced, whether the iron rods were pointed, or not; and that it was indifferent, whether their position was horizontal or not. Secondly, that thunder electrized not only iron, but also wood, living bodies, and other electrizable substances. Thirdly, that it was not absolutely necessary to place these bodies at the tops of buildings; but that it was sufficient for them to be placed about 4 feet from the ground in an open situation, and at some distance from large buildings. Fourthly, that bodies electrized in this manner, produced the like phenomena with those electrized by glass after the usual manner. It was afterwards discovered, that

* In a memoir read to the Royal Academy of Sciences at Paris, Nov. 15, 1752.—Orig.

electrizable bodies, thus disposed in open air, were sometimes electrized under thick clouds, but without thunder, lightning, or even without rain or hail.

The Abbé Nollet recommends, that these experiments should be made with circumspection, as he has been informed by letters from Florence and Bologna, that those who have made them there, have had their curiosity more than satisfied by the violent shocks, which they have sustained, in drawing off the sparks from an iron bar electrized by thunder. One of these in particular says, that once, as he was endeavouring to fasten a small chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building, in order to draw off the electrical sparks by means of the oscillations of this ball, there came a flash of lightning, which he did not see, but which affected the chain with a noise like wild-fire. At that instant, the electricity communicated itself to the chain of the copper-ball, and gave the observer so violent a commotion, that the ball fell out of his hands, and he was struck backwards 4 or 5 paces. He never had been so much shocked by the Leyden experiment.

From the experiment at Marly-la-ville, and those which have been made since, have been drawn 2 consequences: one, that the matter of thunder, and that of electricity, are one and the same: the other, that by the means of pointed iron rods, one might, without its doing any harm, draw off all the fulminating matter from a stormy cloud. But our author has shown, that pointed bodies are not absolutely necessary; and is desirous we should not too hastily believe, that mischiefs arising from thunder may be averted by the apparatus proposed. He thinks the means vastly too small for the magnitude of the cause. This first letter to Mr. Franklin is an introduction to the 5 subsequent ones.

The 2d letter treats of the nature of the electric matter. In this its analogy with fire is considered and proved; and the author takes notice that Mr. Franklin, he imagines, who has certainly made some important discoveries in the properties of electricity, cannot but be dissatisfied with the editors of his work, for publishing, "that he exhibited to our consideration an invisible subtil matter, disseminated throughout all nature, &c. which had hitherto escaped our observations." The latter part of which assertion is not strictly true; as the considering the matter of fire, and that of electricity, to be one and the same, is a fundamental principle of what both the Abbé Nollet and Mr. W. formerly published on this subject.

The 3d letter to Mr. Franklin contains several proofs, that glass is not impermeable to the electric matter. Some of which experiments on this subject Mr. W. heretofore laid before the R. S. and they are in his opinion fully conclusive.

The 4th letter to Mr. Franklin relates to several phenomena of the Leyden experiment. In this letter it is examined, whether the effects of this experiment proceed from the glass phial, or from the non-electrics contained in it; and experiments are produced to prove, that the power of giving a shock in an electrized phial of water, proceeds from the water in the phial, and not from the phial itself, as Mr. Franklin imagines. In this letter likewise is an examination of Mr. Franklin's opinion, that in the charged phial, as much fire as is received by one of its surfaces is lost by the other.

The 5th letter to Mr. Franklin is in respect to the power of pointed non-electric bodies drawing off and throwing off electrical fire, at a much greater distance than obtuse bodies do of the same kind. Our author thinks, that Mr. Franklin has attributed more power to pointed bodies, than on experiment he finds to be true.

The 6th letter to Mr. Franklin is on the analogy of thunder with electricity. This is a fact at present so well established, as to admit of no doubt. But our author cannot agree with Mr. Franklin in his opinion, "that thunder is at present in the power of men, and that we are able to dissipate it at our pleasure: that an iron rod (such a one as Mr. Franklin has directed, and such a one as has been made use of) is sufficient to discharge of all its fire a stormy cloud against which it is directed." For his part he confesses, that he cannot believe it; first, because he sees too great a disproportion between the effect and the cause: secondly, because the principle, which is given us to support this opinion, is not sufficiently established. He can hardly think, that the fulminating matter contained in a cloud, capable of covering a great city, can be drawn off in a few minutes by a pointed bar, as thick as your finger. If even a number of these placed on the tops of eminencies were only necessary to prevent the effects of thunder, would not the vanes and crosses at the tops of our steeples have been sufficient to procure us this advantage? These buildings however, in all times, have not been exempted from the mischiefs of thunder. He despairs of our weak efforts ever being able to disarm the heavens.

In this letter are also considered the validity of Mr. Franklin's hypothesis of electric and non-electric clouds; the former arising from the sea, the latter from the land; their operation approaching near each other; the difference, according to Mr. Franklin, between electrical and common fire; and several other parts of Mr. Franklin's doctrine.

The 8th letter is addressed to M. Jallabert of Geneva; and, among other curious particulars, inserts part of a letter, which our author had received from Mr. Jallabert, giving an account of an experiment, which Mr. Jallabert had some time since made at the water-works at Geneva. An account of this experiment was communicated by Mr. W. to the Society; and it has a near relation to the experiment which was made here in electrizing the river Thames 6 years ago. Mr. Jallabert consults the Abbé Nollet in regard to the solution of the phenomena of this experiment; and the Abbé now gives the same solution to it, which Mr. W. first gave to a similar experiment of Le Monnier's, and laid before the Society in January 1746, and since applied on other occasions in illustrating the electrical circuit.

The 9th letter to Mr. Boze at Wittemberg, is in answer to one of Mr. Boze, in which this gentleman expresses himself surprized, that so many ages have passed, without it having been discovered that thunder electrizes bodies; since it depends on an experiment so simple. and which it is hardly possible to fail in, when you desire to repeat it under proper circumstances. On this our author observes, that people in general only see the facts, or are ignorant of, or do not consider the means by which philosophers arrive at them; nor perceive the circumstances, without which these phenomena could never have been made known to us; and that Mr. Boze will cease to be surprized, as he is so well versed in these phenomena, when he reflects on what our author offers.

To make the experiment in question, it is necessary that bodies should be supported by glass, silk, or resin, without touching any thing else communicating with what we now call non-electrics; without which, the signs of electricity, which are sought for, cannot manifest themselves. To this experiment therefore, a previous knowledge is required of insulating bodies to be electrized; but where is the man who was acquainted with this fact 30 years ago? Before that period, it was not even guessed at by any one.

Since Mr. Gray discovered, that bodies must be insulated, to communicate to them a perceptible electric virtue, to what purpose could we set up iron bars under a stormy cloud? This thought could not have happened, but to those who had taken notice of the analogy between lightning and electricity, and on whom this idea had made a strong impression. And no one could think seriously on this analogy, but since the discovery of the Leyden experiment, that is, since the year 1746. Before that time the electrization of bodies by thunder could not have been perceived, but by an accident very difficult to meet, on account of the conditions requisite.

Yet it may be urged, that bodies, being really electrized, have shown themselves in all ages,* as historians both ancient and modern have expressly men-

* I formerly took notice, that the electrical attraction had been observed so early, as to be mentioned by Theophrastus (see Phil. Trans. vol. xliv. p. 732); so its luminous appearance, though only considered as a meteor, is mentioned by Plutarch, in the life of Lysander. Pliny, in the second book of his Natural History, chap. 37, calls these appearances stars; and tells us, not only that they settled on the masts, and other parts of ships, but also on men's heads. Seneca too in his Natural Questions, chap. i. takes notice of the same phenomenon. And in Cæsar, de Bello Africano, cap. 6, edit. Amstel. 1686, we find them attending a very violent storm. Livy, chap. 22, mentions two similar facts.

tioned. But to this it may be replied, that it was not enough to know the fact, unless people were sufficiently acquainted with it to take it for what it really was; that is, the electric virtue: for without that, observations of this kind could have very little weight with any person engaged in the inquiry. At present, indeed, when we know, from the experiment of Marly-la-ville, that a stormy cloud is a great electric mass, the action of which extends itself sensibly even to bodies which are on the surface of the earth, we must agree, by reflecting on them, that the lights which have been seen on the crosses placed on the tops of several steeples, those which the Roman soldiers said they had observed at the end of their pikes, and those lambent flames which appear on the masts of ships, which mariners call St. Helmo's fire, are so many electrical phenomena. But until the moment that this experiment was made, which opened our eyes with regard to the possibility and nature of these marvellous effects, these appearances were regarded either as popular illusions, or false prodigies, or even as luminous vapours, which might be ranged in the class of phosphori. Besides, as these were seen but seldom, if ever we had been tempted to attribute them to the influence of stormy clouds, we might have been dissuaded from it, by considering the little agreement there is between the rarity of these effects, and the frequency of the causes, which might produce them.

We see therefore how important it is to describe exactly the phenomena we observe: otherwise, how long may it be, before we can deduce any real instruction from those, which we have been informed of in a negligent and superficial manner? We have heard all our lives of St. Helmo's fire, of those which the ancients call Castor and Pollux, and of the comazants of our mariners. But, from what we have had related to us, and from what we have read, who could have been prevailed on to range them with electrical phenomena? We have heard them represented as thin lambent shining lights, a kind of phosphoreal vapour: but there is a passage in the memoirs of the Count de Forbin, quoted by our author, mentioning St. Helmo's fire; which if any one, well versed in the phenomena of electricity, had carefully attended to and considered a few years ago, he might have prognosticated success to Mr. Franklin, when he proposed his

These appearances are called, by both French and Spaniards inhabiting the coasts of the Mediterranean, St. Helme or St. Telme's fires; by the Italians, the fires of St. Peter and St. Nicholas, and are frequently taken notice of by the writers of voyages. If some late accounts from France are to be depended on, we are informed, that at Plauzet it has been observed for time immemorial; and M. Binon, the curé of the place, bears his testimony of the truth, that, for 27 years, which he has resided there in that capacity, in great storms, accompanied with black clouds, and frequent lightning, the three pointed extremities of the cross of the steeple of that place appear surrounded with a body of flame; and that, when this phenomenon has been seen, the storm was no longer to be dreaded, and calm weather returned soon after.—Orig.

experiment on thunder. "In the night, says the author of those memoirs, on a sudden it became exceedingly dark, and thundered and lightned most dreadfully. As we were threatened with the ship's being torn to pieces, I ordered the sails to be taken in: we saw, on different parts of the ship, above 30 St. Helmo's fires; among the rest, there was one on the top of the vane of the main-mast, which was more than a foot and half in height. I ordered one of the sailors to take it down; when this man was on the top, he heard this fire; its noise resembled that of fired wet gunpowder. I ordered him to lower the vane, and come down; but scarcely had he taken it from its place, but the fire left it, and fixed itself on the top of the mainmast, from which it was impossible to remove it; and it continued there a considerable time, till it gradually went out, &c."

If all the authors, who have taken notice of St. Helmo's fire, had spoken of it as this just quoted, philosophers might have reproached themselves for its having been so long before they had a just idea of it, and for their not having shown the principle on which it depended. But how few historians are there, who could have related this fact with circumstances so proper to put us in a right train, as those just mentioned?

"And here I cannot but observe, as I am convinced, that the matter of thunder and that of electricity are one and the same, how vast an idea must the attending to the before-mentioned passage excite in the mind of persons, accustomed to the phenomena of electricity? How immense a quantity of it must they conceive to have been at that time in the atmosphere surrounding the ship, and within the verge of its action, to furnish more than 30 St. Helmo's fires; the same in fact which we see at the ends of our conductors in electrizing, one of which was more than a foot and half in height? At this time, and under these circumstances, the mast, yards, and every part of the ship, I consider as conductors of electricity, between the then electrized atmosphere, and the sea; and though, being of a vegetable nature, and, if dry, even of the worst kind for this purpose, they conducted electricity much less perfectly than metal under the like circumstances would have done, I doubt not but that they were greatly instrumental in averting the danger, with which the ship was threatened.

"On these considerations, I do not scruple to recommend, as Mr. Franklin has done, communications of metal between the spindles and iron-work at the tops of the masts of ships, and the sea; or, which will answer the same purpose, the bilge water in the well. This can be liable to little objection, as the doing it is neither difficult nor expensive; an iron wire, of the thickness of a goose-quill, conducting electricity more readily than any piece of timber, however large; and these masts do it so much the worse, as they are of a resinous nature.

"From attending to these phenomena, we every day see more and more the perfect analogy, to compare great things with small, between the highly electrized glass jar, in the experiment of Leyden, and a cloud replete with the matter of thunder. But more of this perhaps on some future occasion.

"Though the number and continuance of the St. Helmo's fires, in the passage before mentioned, probably tended greatly to preserve the ship from the destruction with which it was then threatened, yet the cause may be too great, and come on too fast, to be lessened enough by these means to avert the mischief. Thus in the account, published in the Philosophical Transactions,* from captain John Waddel, his ship was almost beaten to pieces by the thunder and lightning: though, as he expresses himself, there were sundry large comazants over head, some of which settled on the spindles on the topmost heads, and burnt like very large torches. When this account was written, these phenomena were only considered as the presages or attendants of a storm, and no sort of inference proposed from them."

But to return to our author. His work closes with a series of experiments, intended to demonstrate the validity of the conclusions exhibited in it. These merit the particular attention of those conversant in these matters. It may be further observed, that some of the experiments are made in vacuo, and are of the same kind with those which Mr. W. communicated to the Royal Society in February 1752; and which have been since published in the Philosophical Transactions.†

On the whole, he thinks this treatise a very valuable one, as it gives us the still riper thoughts of an able writer on a difficult, and till very lately, an almost unknown subject; of one who, besides his inquiries into this part of philosophy, has a great compass in the knowledge of nature, and is therefore well qualified to investigate her phenomena.

XXXII. The Number of Persons in the City of Bristol, calculated from the Burials for Ten successive Years, and also from the Number of Houses. By John Browning, Esq. of Barton-hill near Bristol. p. 217.

The certificates were obtained under the hands of the præcentor of the college, the several ministers of the 17 parish-churches, the register keeper of the several quakers' cemeteries, the several Anabaptists' cemeteries, the Jews' new-erected cemetery, for 10 years, including the year 1741 and 1750. As some of the parishes within the liberties of the city extend beyond the liberties into the counties of Gloucester and Somerset, they are distinguished by the names of the out-parishes. The inhabitants of the several out-parishes being buried within

* Vol. xlvi. p. 111.
† Vol. xlvii. p. 363, et seq.

the liberties of the city, must of consequence be brought into the city bill of mortality.

The total number of burials for 10 years was 17317, which is on a medium 1731 in each year.

Now the latest and most accurate observations demonstrate, that in great cities a 25th part of the people die yearly. Therefore, at this rate, 1731 × 25 = 43275, is the number of the inhabitants, computed on this principle.

The number of houses rated to the land-tax, as appears by the rates in the council-house, Michaelmas, 1751, is 4866.

But as the rates are not always accurately made, and as it is the usual custom not to rate houses, which are untenanted, nor hospitals, nor alms-houses, it will be necessary to make a very large allowance for these deficiencies, especially as many houses are rated in gross under the denomination of several tenements, when they belong to the same landlord; in all the several parishes, an allowance of 25 per cent. will be more than sufficient, which produces 1216.

Also the number of houses in the out-parishes is 1228.

These 3 numbers collected, make the total number of houses 7282. Now the usual number of souls allowed to each house is 6. Hence,

7282 × 6 = 43692 number of inhabitants by the houses.

And 43275 number of inhabitants by the burials.

XXXIII. Of the Eclipse predicted by Thales. By the Rev. William Stukely, M.D. p. 221.

The eclipse predicted by Thales the Milesian, happened in the 603d year before the Christian æra. At this time there was a sharp war between the Medes and Lydians, of which Herodotus gives an account. Halyattes, father of the famous Crœsus, was then king of the Lydians.

After the Medes had conquered all the upper or northern part of Asia, from the old possessors the Scythians, they again extended their borders to the river Halys in Lesser Asia, the boundary between Cappadocia and Armenia, or between the Lydians and Medes. It was not long before a war took place between these nations, which continued for 5 years together, with various success. In the 6th year they engaged each other, with the utmost of their strength; intending to make that battle decisive, but while the fortune of the day seemed to hang in an equal balance, there happened a total eclipse of the sun, which overspread both armies with a horrible darkness; so that being affrighted at such a critical judgment of Heaven, as they thought it, both sides put up their swords, and agreed to refer the controversy between them to two arbitrators. Halyattes, king of Lydia, chose Siennesis, king of Cilicia; Cyaxares, the Median monarch, chose Nebuchadnezzar, now busy in leading the Jews into captivity. Nebuchaduezzar is by Herodotus called Libynetus. It seems that the letter N, in the beginning of the word, has, in the ancient copies of Herodotus, been turned into A; and then the words, in two different dialects, are not very different.

These great arbitrators compromised the matter between the contending parties, by making a match between the two royal families; and so restored peace and friendship. Astyages, the son of Cyaxares, king of Media, married Ariena, daughter of Halyattes, king of Lydia, of whom, a year after, was born Cyaxares, whom the prophet Daniel calls Darius the Mede. And in that last-mentioned year, king Cyaxares gave his daughter Mandane in marriage to Cambyses king of Persia; of whom, the next year, was born the great Cyrus, the founder of the Persian monarchy, whom the prophet Isaiah foretold by name, that he should restore the polity of the Jews, the city of Jerusalem, and the temple, and return the sacred vessels of gold and silver, which Nebuchadnezzar had carried away, and put into his heathen temple at Babylon.

Thus ended this famous quarrel between the Medes and Lydians, through the timely event of a total solar eclipse, made still the more eminent, that it was foretold to the Ionians by Thales of Miletus, then in the 37th year of his age. He was born in Phœnicia; and there doubtless he acquired his knowledge in astronomy. He was the first who brought this science into Greece, 300 years after the pretended Chiron of the Argonauts. It is an invincible argument, that he *learned* his art; for a whole life is not sufficient, so to observe the motions of sun and moon, as to be able to calculate an eclipse.

This is the first eclipse, which we have recorded in so circumstantial a manner. Notwithstanding all this, it is strange how the learned have erred about the true year of this memorable affair. Pliny begins the mistake, telling us, that it was the 4th year of the 48th Olympiad; whereas it was the 4th year of the 43d. It is not unlikely that the numeral letter V is crept into the original. Clemens Alexandrinus makes it about the 50th Olympiad. Dr. Prideaux makes it 5 years too late; Archbishop Usher 2 years. Sir Isaac Newton gives us the true month and day, but assigns the 585 year, as Ricciolus.

Of this eclipse, Dr. S. has traced the moon's shade, as it passed over the earth's surface from 20 to 60 degrees of longitude east from London; and from 25 to 50 degrees of north latitude, with the hours, half-hours, and quarters of time, where vertical. This was on the 18th of May in the proleptic Julian style, in the year of the Julian period 4111, the 603d year before the vulgar æra of Christ. The eclipse was total 4 minutes and a half, where the battle was fought. The shade entered the desert of Barca in Africa, soon after 9 in the morning. It traversed the Mediterranean sea, and isle of Cyprus; entered Asia Minor at Cilicia, a little before 11; about half an hour after, it passed the city now called Erzerum; near which Dr. S. supposes the battle was fought, as being at the boundary between the two kingdoms. It is between the river Halys, and the river Melas, on which was the ancient city Melitene. The river Melas runs eastward into the Euphrates. At half an hour after 12, the shade entered on the Caspian sea, and at 1 on the Kalmuc Tartary.

We see here an authentic parapegma in ancient history, deduced from astronomy: and we see a remarkable instance of a most furious war terminated by the intervention of an eclipse.

XXXIV. A further Account of the Giants' Causeway in the County of Antrim in Ireland. By the Rev. Richard Pocock, LL.D., F.R.S. p. 226.

Dr. P. having taken another, and more particular view of the country about the causeway, states that he went about 2 miles to a peninsula called Donseverik, where he saw some tendency in the rock towards this work of nature; and going about half a mile farther, came to the beginning of the pillars in the sea cliff, about 5 miles from the causeway: and the shore and cliffs being shaped mostly in little semicircular bays, he had many beautiful views of the upper and middle strata of pillars: in one particularly they had much the appearance of ruined porticos one over the other; and turning the little end of a spy-glass, it appeared something like the ruins of Palmyra, as a view of them is represented in a copper-plate, published in the Philosophical Transactions. This wonderful work of nature is continued on in the cliffs for about a quarter of a mile beyond the Giants' Causeway.

He saw it again in the road to Coleraine, 5 miles to the west of the Causeway, in a low hill a furlong to the south of the road, and 2 miles to the south of the sea. The pillars here are small; and being about a mile and a half from Ballimagarry, where the earl of Antrim has a ruined house, lately burnt down, it served as a quarry for building part of that house, in which he saw a great number of the stones, and particularly one of 9 sides. He saw others near 2 miles farther, to the south of the road in a low hill, within 2 miles of Coleraine; so that the whole extends about 11 Irish miles, or 14 English.

Beyond Coleraine, to the east of Magilligan, he saw in the rocks towards the sea-cliffs, the stones in the hills very regular, appearing at a distance much like these pillars. This is 6 computed miles beyond Coleraine, and consequently about 10 English miles from the last pillars. At Fairhead also, a high point of land, 3 miles to the east of Ballycastle, towards the top of it, the rock appears as in grand pillars. They say it is not in joints, but it has something of the appearance of a grand Gothic piece of workmanship. It is a black stone, weighty and brittle: and he had been informed, that it was tried in a glass-house, and that it melted with kelp, so as to make the black glass bottles: which experiment he was told had been made by Mr. Dobbs.

Mr. Drury found in a stone of the Causeway a rough pebble, in the shape of an egg, about $\frac{3}{4}$ of an inch long, and above an inch thick, and when it was polished, it proved to be a white cornelian. They are from 3 to 9 sides, frequently encompassed with as many stones as there are sides; but many of them have a narrow side, which has no stone to it, but is filled up with a piece or pieces of stone; which pieces, when the stones are moved, commonly separate, and break off. Some stones have 2 or 3 or more of these sides; so that it is possible, a stone that has any number of stones round it, may have double the number of sides.

XXXV. A Letter on the same Subject from the Rev. Richard Pocock, LL.D. Archdeacon of Dublin, to the Rev. Tho. Birch, D.D. Secr. R.S. p. 238.

Does not contain any important additions to the preceding and other accounts of the Giant's Causeway.

XXXVI. A View of the Relation between Dr. Halley's Mortuary Tables, and the Notions of M. De Buffon, for establishing a Rule for the Probable Duration of the Life of Man. By Mr. William Kersseboom, of the Hague. Translated from the French, by James Parsons, M. D., F. R. S. p. 239.

" Man, says M. de Buffon, at the end of the 2d tome, dies at all ages; and though it may be said in general, that his life is longer than that of almost any other animal, it cannot be denied that it is also more variable and uncertain. Attempts have been of late years made to know the degrees of these variations, and to establish, by observations, some certainty concerning the mortality of mankind of different ages. If these observations were sufficiently exact, and a good number of them made, they would be of great use towards knowing the number of the people, of their increase, of the consumption of provisions, of the division of taxes, &c. Many ingenious men have studied this subject; and lately M. Deparcieux, of the Academy of Sciences, has given an excellent work, which serves as a rule with respect to annuities for life: but as his principal view was to calculate the mortality of annuitants, and that generally annuitants for life are men in one state, no conclusion can be drawn from it for the mortality of mankind at large.

" Dr. Halley, Mess. Graunt, Kersseboom, Simpson, &c. have also published tables of the mortality of mankind; and they have founded them on extracts from the bills of mortality of some parishes of London, Breslaw, &c. But it appears that their researches, however ample, and the result of long study, can afford only very distant approaches to the knowledge of the mortality of mankind in general. In order to make a good table of that kind, not only the registers of the parishes of such cities should be used, where foreigners are daily coming in, and natives going out, but also those of the country; that, by adding together the results of each, the one may compensate for the other. M. Dupré, de St. Maur, of the French Academy, has begun this on 12 country parishes, and 3 of those of Paris: these I publish, because they are the only tables on which the probabilities of the life of mankind in general can be established with any certainty."

On this passage M. Kersseboom says he is greatly surprized, that a philosopher should condemn works which he never either saw or read: for it is evident that M. de Buffon never saw his Essays on Political Arithmetic; and that all which he appears to know of it, is indeed very slightly drawn from M. Deparcieux's work, who knew no more of it, as he himself makes it appear, than what he found in the Bibliotheque raisonnée for the first 3 months of the year 1743, Tom. 30. This extract happens unluckily not to be made by an able hand; but, on the contrary, very fit, by its confusion, and the irregularities which run through it, to lead into errors. The corrections, that were made in the 2nd part of the same 30th tome, are not even sufficient to secure the reader from mistakes.

Yet M. de Buffon, without even reading the work, might have known more of it, though written in a language which he is probably a stranger to; since Mr. Eames has given an excellent extract of the first essay in English, printed in N° 450 of the Philos. Trans.

M. K. would say much the same of that excellent piece of the learned Dr. Halley, if his surprize did not increase, the more he reflected, that this work ought to be thoroughly known to a member of the Royal Society of London; and yet that this very member makes so careless a judgment on it. This reflection leads to another kind of defence of that famous deceased author; which is to make M. de Buffon sensible, that "nearly the same degrees of probability of the duration of the life of man in general" are in the table of Dr. Halley, which he would have us think are in the extracts of M. Dupré's observations or tables, which he has published. For this purpose M. K. constructed a table parallel to that of Dr. Halley, which begins with 1000 lives of one year old, and which he found, in the reduction of the great general numbers of Dupré's tables, to have also the smaller numbers analogous; that is, by beginning also with 1000 lives of a year old. Both tables are laid down as follows:

Halley's Table.			*Halley's Table.*			*Dupre's, reduced.*			*Dupre's, reduced.*		
Years of Age.	Numb. of Lives.	Num. of Deaths fr Yr. to Yr.	Years of Age.	Numb. of Lives.	Numb. of Deaths fr Yr. to Yr.	Years of Age.	Numb. of Lives.	Numb. of Deaths fr. Yr. to Yr.	Years of Age.	Numb. of Lives.	Numb. of Deaths fr. Yr. to Yr.
1	1000	145	43	417	10	1	1000	136	43	406	6
2	855	57	44	407	10	2	864	56	44	400	18
3	798	38	45	397	10	3	808	40	45	382	8
4	760	28	46	387	10	4	768	29	46	374	6
5	732	22	47	377	10	5	739	23	47	368	9
6	710	18	48	367	10	6	716	17	48	359	6
7	692	12	49	357	11	7	699	14	49	353	21
8	680	10	50	346	11	8	685	9	50	832	5
9	670	9	51	335	11	9	676	6	51	327	9
10	661	8	52	324	11	10	670	5	52	318	5
11	653	7	53	313	11	11	665	6	53	313	7
12	646	6	54	302	10	12	659	4	54	306	4
13	640	6	55	292	10	13	655	4	55	302	19
14	634	6	56	282	10	14	651	5	56	283	7
15	628	6	57	272	10	15	646	6	57	276	11
16	622	6	58	262	10	16	640	6	58	265	5
17	616	6	59	252	10	17	634	6	59	260	30
18	610	6	60	242	10	18	628	6	60	230	5
19	604	6	61	232	10	19	622	8	61	225	10
20	598	6	62	222	10	20	614	5	62	215	9
21	592	6	63	212	10	21	609	10	63	206	13
22	586	7	64	202	10	22	599	7	64	197	13
23	579	6	65	192	10	23	592	7	65	184	13
24	573	6	66	182	10	24	585	11	66	171	7
25	567	7	67	172	10	25	574	8	67	164	13
26	560	7	68	162	10	26	566	8	68	151	6
27	553	7	69	152	10	27	558	8	69	145	22
28	546	7	70	142	11	28	550	6	70	123	6
29	539	8	71	131	11	29	544	13	71	117	15
30	531	8	72	120	11	30	531	5	72	102	7
31	523	8	73	109	11	31	526	10	73	95	9
32	515	8	74	98	10	32	516	8	74	86	15
33	507	8	75	88	10	33	508	7	75	71	6
34	499	9	76	78	10	34	501	16	76	65	7
35	490	9	77	68	10	35	485	10	77	58	9
36	481	9	78	58	9	36	475	9	78	49	3
37	472	9	79	49	8	37	466	9	79	46	14
38	463	9	80	41	7	38	457	5	80	32	3
39	454	9	81	34	6	39	452	23	81	29	5
40	445	9	82	28	5	40	429	5	82	24	4
41	436	9	83	23	3	41	424	11	83	20	3
42	427	9	84	20		42	413	7	84	17	

Sum total of Dr. Halley's table.............. 34000

Sum total of M. Dupré's table reduced............33911

In the whole matter, all the difference between these two tables consists in this, that Dr. Halley's is more perfect, more compact, and more conformable to those observations which conduct us to the idea of a progression nearly arithmetical, which the great number of researches enables us to unfold by little and little, in the representation of the strength of human life, when that strength is become more uniform.

If M. de Buffon will compare the table given by himself of the probability of the duration of life, which is founded on that of M. Dupré de St. Maur, with that given by M. Deparcieux, in his ingenious work constructed on that of Dr. Halley, he will find a like conformity between them. It is M. Deparcieux's 13th table which is meant. What follows, in Halley's column, is set down, in order to compare it with M. de Buffon's table.

Deparcieux on Halley.			*De Buffon on Dupré of St. Maur.*			*Deparcieux on Halley.*			*De Buffon on Dupré of St. Maur.*		
Years of Age.	Lives at a Medium. Years	Mths.	Years of Age.	Duration of Life. Years	Mths.	Years of Age.	Lives at a Medium. Y ars	Mths.	Years of Age.	Duration of Life. Years	Mths.
1	33	6	1	33		43			43	20	4
2	38	0	2	38		44			44	19	9
3	39	9	3	40		45	19	8	45	19	3
4	40	9	4	41		46			46	18	9
5	41	3	5	41	6	47			47	18	2
6			6	42		48			48	17	8
7			7	42	3	49			49	17	2
8			8	41	6	50	17	3	50	16	7
9			9	40	10	51			51	16	0
10	40	5	10	40	2	52			52	15	6
11			11	39	6	53			53	15	0
12			12	38	9	54			54	14	6
13			13	88	1	55	14	10	55	14	0
14			14	37	5	56			56	13	5
15	37	6	15	36	9	57			57	12	10
16			16	36	0	58			58	12	3
17			17	35	4	59			59	11	8
18			18	34	8	60	12	5	60	11	1
19			19	34	0	61			61	10	6
20	34	2	20	33	5	62			62	10	0
21			21	32	11	63			63	9	6
22			22	32	4	64			64	9	0
23			23	31	10	65	9	11	65	8	6
24			24	31	3	66			66	8	0
25	30	11	25	30	9	67			67	7	6
26			26	30	2	68			68	7	0
27			27	29	7	69			69	6	7
28			28	29	0	70	7	7	70	6	2
29			29	28	6	71			71	5	8
30	27	11	30	28	0	72			72	5	4
31			31	27	6	73			73	5	0
32			32	26	11	74			74	4	9
33			33	26	3	75	5	7	75	4	6
34			34	25	7	76			76	4	3
35	25	0	35	25	0	77			77	4	1
36			36	24	5	78			78	3	11
37			37	23	10	79			79	3	9
38			38	23	3	80	4	6	80	3	7
39			39	22	8	81			81	3	5
40	22	4	40	22	1	82			82	3	3
41			41	21	6	83			83	3	2
42			42	20	11	84	3	6	84	3	1

It is therefore sufficiently demonstrated, that Dr. Halley's table ought not, in M. de Buffon's opinion, to be excluded from the class of those which "are the only tables on which the probabilities of the life of mankind in general can be established with any certainty;" far from being comprised, in his severe judgment, among those of authors, "whose researches, however ample, and the result of long study, can afford only distant approaches to the knowledge of the mortality of mankind in general."

M. de Buffon begins his table of the probabilities of life with a term, which precedes that of a year old, called zero d'âge; and from M. Dupre's observations, assigns to it a duration of 8 years. M. K. first thought it an error of the press; but there is no room for this doubt, after what M. de Buffon says, "We see by this table, that we may reasonably hope, that is, lay an even wager, that an infant just born, or who has no age, will live 8 years; that an infant of a year old, will live 33 years," &c. This little space of 8 years struck Mr. K. because all the observations, which he knew, are very far from it. He had therefore recourse to the source, to the observations of M. Dupré himself, and found it was a mistake of M. de Buffon; the mean life of infants of no age being, according to M. Dupré's tables, 25 years and upwards; and, from the observations of Justel, which Dr. Halley made use of, the mean life of a child of no age is above 27 years.

M. K. thinks the subject not absolutely requires his offering a word concerning the nature of both Justel's and M. Dupré's observations. The remark has not escaped the sagacity of Dr. Halley himself: it is, "that they want the essential; which is, the number of living persons, among whom the observations on the dead are made." If M. de Buffon had made the same reflections on M. Dupré's tables, he would have found the irreparable defect of them, as well as Dr. Halley did in Justel's observations; and he would doubtless have attended more to the method proposed by Mons. Deparcieux.

XXXVII. Abstract of a Letter from Father d'Incarville, of the Society of Jesus, at Pekin in China, to the late Cromwell Mortimer, M. D., R. S. Secr. Dated Pekin, Nov. 15, 1751. p. 253.

The leaves and flowers of the varnish-tree, which he sent, came from the province of Nan King. This tree is different from that he saw in the king's garden at Paris. The latter is the same with what he saw at Macao; which was brought from Mississipi into France. There is not in Europe the tree, from whose fruit the toeng yeou is drawn. It were to be wished they could raise it there. The toeng yeou is an oil, or natural varnish, drawn by expression from the fruits, which he sent, of which they make a very great trade in China. It costs but very little, the pound weight being worth about 7 or 8 sols of our money. It is said, that they sell it at Paris under the name of China varnish. It is excellent for preserving furniture, giving them a polish not inferior to our varnishes of Europe, which cost so much money. Perhaps they may make some attempts to use it in Europe; but they will not succeed, because they know not how to prepare it. This oil is so common in China, that the greatest part of the people, in tolerable circumstances, rub over their timber with it, giving it what colour they please. It not only adorns their houses, but also preserves the wood. The columns that support their houses, and those of the great room where the emperor's throne is, are varnished with no other than this oil.

The kou chou is a tree, of the bark of which they make the best paper in China. The common paper of their books, which looks yellowish, is made of a particular species of bambou, of which they prepare the young shoots, as we prepare hemp. They whiten it by boiling it in lime-water: in this manner they prepare the kou chou. There is no silken paper in China; all the different kinds of paper here are made either of bark, hemp, or of the straw of corn or rice. Sometimes they blend with this last the stalks of the typha.* The paper made of hemp or straw serves only for wrapping up goods, or to make pasteboard; and that made of the bark of the cotton-plant serves for fans, being less apt to crack than any other white paper.

The white wax, produced by certain insects, is a very curious and profitable thing. What had been told him by one of their missionaries, who had bred them himself, is not sufficient to give a proper idea of them. As to the manner of their depositing this wax, it appears that there is some analogy between it, and the manner of the gum lac's being deposited by certain ants.†

In the emperor's palace they very rarely use any other candles, than such as are made of this wax, because it never emits any smoke. The learned therefore use them only, when they compose an exercise on their examination for degrees; for then they are confined in very small rooms, where the smoke of tallow candles would incommode them greatly. The chief consumption of this wax is owing to their coating tallow candles with it. This wax is procured by boiling the matter rasped off the branches of the tree, the leaves of which are the proper nourishment of these insects, in a large vessel of water; the wax swims at the top, and when cold it is taken off in a cake.

* Typha palustris major of Caspar Bauhin. Cat's-tail.—Orig.

† In order to explain this passage, I take the liberty of making the following remark. The lacca-tree is the jujuba indica of the great Ray; which produces this gum. The letter writer is misled by what Garcias ab Horto says about it, that certain large winged ants make this gum out of the juice sucked from this tree, and deposit it upon the surculi, &c. of the same: but the celebrated Ray and J. Bauhin say, it is exudated, and by the heat of the sun concreted into the form, in which it is found on the parts of this tree. There are other trees which produce this gum, as well as this, mentioned by Hermannus.—Orig.

The berries of the tallow-tree are of great use in the southern provinces, where there are very few sheep. Almost all the candles, sold there, are made of the oil drawn from these berries. They procure this oil in the same manner mentioned concerning the wax, and as this oil is not of so good a consistence as tallow; for its cohesion, when candles are made of it, they dip them in the white wax before-mentioned; the external coat, thus made, prevents them from guttering.* At Pekin the same thing is done with tallow candles; nor does he ever remember to have seen them run down. He imagines that our bee's-wax would answer the same purposes with this white wax of China.

The seeds of the yen tchi come from a plant which is very particular. From these seeds or berries, when very ripe, a tincture of a fine red is drawn, as may be seen in the flakes of cotton charged with this colour, sold here. They moisten them with a little warm water, and then express the colour, which is afterwards evaporated to dryness, and serves for water-colours.

The persicaria, of which they make indigo in and about Pekin, merits attention. Indigo is also made of the persicaria maculata, with which the banks of rivers and streams often abound; but it is of an inferior quality to that made with the other persicaria, and this even is not of equal value with that made of the anil, such as is made in the southern provinces here, and in those of America.

The stones of apricots come from a species of tree, whose fruit is not eatable. These trees are only cultivated for these stones, from which an excellent oil is produced for burning; and which, instead of olive oil, they use for sallads.

The hoai tze are the clusters of the flower of a bastard acacia, from which a most beautiful yellow tincture is drawn, by boiling them with a little alum. The hoang tchi tze produces yet a finer tincture: but the finest yellow colour of China comes from the hoang pe pi; and these three are prepared in the same manner.

A kind of stuff is made from the cods of the wild silk-worm, called kien tcheou, excellent for wear, when made for gain. It is scarce, and dear. There is another kind of kien tcheou, of which they sell a large quantity at Canton: it is made of the silk drawn from other cods. These cods are capable of being wound on wheels or spindles. First they must be boiled in a strong lee, made of the ashes of the stalks of the Sarazin corn, till they are capable of being pulled asunder with the fingers, in order to turn them inside out, and take out the fragments of the chrysalis; and as this kind of stuff is worked like other cloth, the weavers do the rest.

The fruits of the tong yeou, and of the tallow tree, which you should have

* This is applicable to the green wax of Mississippi.—Orig.

received last year, were fresher than those before sent. This year you will receive the cods of silk, which makes the silk called kien tcheou, with the butterflies, which come from them. The other things sent want no explanation.

The empire of China abounds in mines of all sorts, as gold, silver, copper, tin, lead, iron, &c. The provinces which produce the greatest quantity are, Yun nan, and See tchouen. The two greatest rivers of China, Kiang and Hoang ho, send down quantities of gold sand. The former takes its source in the province of See tchouen, and the latter from Coconor; but they find mines of gold and silver in the provinces of Yun nan, See tchouen, Chen si, Chan tong, Hou kouang, Fou kien, Kouei tcheou, Pe tche si: but, for political reasons, they work but few of them. He believes the principal is, lest the greediness of gain should excite popular insurrections. They open them sometimes in one place, sometimes in another; but on the least appearance of a rising, they immediately shut them up again. We cannot give any account of what is desired, concerning the manner of working the several mines. We are not in a way of informing ourselves. As to what regards petrifactions, he had only seen a few crabs, pieces of wood, and some bones, which he takes to be those of buffaloes.

The Chinese have but a very confused idea of a universal deluge. They only conclude from things seen on the surface of the earth, that there must formerly have been some terrible hurricane, and that the sea had covered the face of the earth. A great mandarin, who had a better understanding than the Chinese commonly have, being sent into Ho nan, to visit several places, observed, on the top of a very high mountain, a kind of basin, the circumference of which, formed by the mountain, was filled with different figures of fishes, shells, and marine plants, impressed on stones; he said to another mandarin, who accompanied him, "Certainly the sea must have been here; these fishes, shells, and plants are found only in the sea." F. Gaubil says, the Chinese books pretend that such impressions are found on the highest mountains of Thibet, and See tchouen.

The greatest part of the cinnabar of China comes from the province of Yun nan: and it is said there is some also in Kiang si, Hou kouang, and Koui tcheou. Kang hi, the great grandfather of the present emperor, ordered a general search to be made through the whole empire for antimony, but found none in any of the mines.

XXXVIII. On the Cause of the Different Refrangibility of the Rays of Light. By Mr. T. Melville. p. 261.

In order to account for the different refrangibility of the differently-coloured rays, Sir Isaac Newton (Optics, Query 29), and several of his followers, have supposed that their particles are of different magnitudes or densities: but if there be any analogy between the refractive power and gravity, it will produce equal velocities in all particles, whatever their magnitude or density be; and so all sorts of rays would be equally bent from their right-lined direction.

It seems therefore a more probable opinion, which others have advanced, that the differently-coloured rays are projected with different velocities from the luminous body; the red with the greatest, violet with the least, and the intermediate colours with intermediate degrees of velocity; for, on this hypothesis it is manifest, that they will be differently refracted in the prismatic order, according to observation.

On supposition that the different refrangibility of light arises solely from the different velocities of the rays before incidence, these velocities must be to each other nearly as their sines of refraction.

Their velocities in any given medium, suppose air, being once determined, their velocities in any other may be easily discovered; for they are to those in air as the sine of incidence to the sine of refraction, when the ray passes from air into the other medium.

While the differently-coloured rays are supposed to move with one common velocity, any pulses, excited in the æthereal medium, must overtake them at equal distances: and therefore the intervals of the fits of reflexion and transmission, if they arise in this manner, as Sir Isaac conjectures, would be all equal: but if the red move swiftest, the violet slowest, and the intermediate colours with intermediate velocities, it is plain that the same pulses must overtake the violet soonest, the other colours in their order, and last of all the red; that is, the intervals of the fits must be least in the violet, and gradually greater in the prismatic order, agreeably to observation.

Let c denote the velocity of the æthereal pulses, v the velocity of red light, and u that of violet; i and j the intervals of their fits, and d the distance between 2 succeeding pulses: it is plain, from the nature of Newton's hypothesis, that i is to d as v to $c - v$: and again, d to j as $c - u$ to u: therefore, ex æquo, i is to j, as $cv - vu$ to $cu - vu$, from which we have the equation $c = \frac{i - j}{iu - jv} \times vu$. Therefore, as the proportion between the intervals of the fits in red and violet, can be assigned by experiment, and the proportion of their velocities in any medium also, the velocity of the æthereal pulses may be easily computed. The velocities of the red and violet in air are nearly as 78 and 77. In the celestial spaces they are less, but almost in the same proportion; the intervals of their fits are by experiment as 100 and 63. Whence, by the canon now laid down, the velocity of the æthereal pulses in the celestial space, is found to be to that of red light, as 79763 to 78000. As light moves from the sun to us, by

Dr. Bradley's accurate estimation, in $8^m\ 12^s$, the pulses of the æthereal fluid must be propagated through the same space in about $8^m\ 1^s$.

Hence also may be determined, in known measures, the distance between two succeeding æthereal pulses; for $d = \frac{ci - vi}{v}$.

On the hypothesis of the different velocities of different colours, we may understand, at least in general, the reason of the strange analogy, discovered by Sir Isaac, between the intervals of the fits, and the spaees occupied by the several colours in the spectrum (a thing hitherto unexplained *); since, from the velocities of the several rays, on which depend the intervals of the fits, as has been now explained, arise likewise their several degrees of refrangibility.

But, as it is of great consequence in philosophy, to distinguish between facts and hypotheses, however plausible, the various refrangibility, reflexibility, and inflexibility, of the different colours, and their alternate dispositions, at equal intervals, to be reflected and transmitted, which are the whole ground-work of the Newtonian system, are to be considered as undoubted facts, deduced from experiment; but that the velocities of different rays are different in the manner now described, is no more than probable conjecture: and though this point should be decided, by a method that we are now to propose, it would still remain uncertain, whether the fits of reflexion and transmission consists in an alternate acceleration and retardation of the particles of light, or in something else. For instance, it might be supposed, that every particle of light has 2 contrary poles, like a loadstone; the one of which is attracted by the parts of bodies, and the other repelled; and that, besides their uniform rectilineal motion, the particles of differently-coloured rays revolve in different periods round their centres; for thus their friendly and unfriendly poles being alternately turned towards the surfaces of bodies, they might be alternately disposed to reflexion and transmission, and that at different intervals, in proportion to the periods of their rotation. Lastly, though it were proved, that the fits proceed from an alternate acceleration and retardation of the particles of light, it would still be no more than probable conjecture, that this is brought about by pulses excited in the æthereal medium. Nay there are some circumstances in these phenomena, that seem hardly intelligible by that hypothesis alone: as, why the intervals of the fits are less in denser mediums; † and why they increase so fast, and in so intricate a proportion, according to the obliquity of incidence. ‡

By Dr. Bradley's beautiful theory of the aberration of light, the stars appear to be removed from their true place to a certain distance, according to the proportion which the transverse motion of the spectator's eye bears to the velocity of

* Compare Newt. Opt. Book i. part 2, prop. 3, with Book ii. part 3, prop. 16.—Orig.

† Newt. Opt. Book ii. part 3, prop. 17.—Orig. ‡ Prop. 15, ibidem.—Orig.

light. It is plain therefore, that on our hypothesis, any star must have a different apparent place for every different colour; that is, its apparent disk must be drawn out by the aberration into a longitudinal form, resembling the prismatic spectrum, having its red extremity nearest its mean place. In the stars situated about the pole of the ecliptic, its length should continue always the same, though directed along all the different secondaries of the ecliptic in the course of a year: but in those which lie in or near the plane of the ecliptic, it should be greatest at the limits of the eastern and western aberrations, the star recovering its colour and figure, when the true and mean places coincide. But there is no hope of discovering, whether our system be true or false, by this consequence of it: for the greatest length of the dilated disk being to the whole aberration, as the difference of the velocity of the red and violet to the mean velocity of light, i.e. but about a 77th part of it, cannot much exceed the 4th part of a second.

The time which the extreme violet light takes in arriving from any distance to the eye, will be to that which the extreme red takes in coming from the same, as 78 to 77. If Jupiter be supposed in a quadrate aspect with the sun, in which position the eclipses of his satellites are most commodiously observed, his distance from the earth being nearly equal to his distance from the sun, light takes about 41^m in passing from him to the earth; therefore the last sensible violet-light, which the satellite reflects before its total immersion into Jupiter's shadow, ought to continue to affect the eye for a 77th part of 41^m; that is, about 32^s of time after the last sensible red light is gone. It is therefore a certain consequence of our hypothesis, that a satellite, seen from the earth, ought to change its colour about half a minute before its total immersion, from white to a livid greenish colour; then into blue, and at last evanish in violet. It need hardly be observed, that the same phenomenon must take place in the time of emersion by a contrary succession of colours, beginning with red, and ending in white.

If this phenomenon be perceived by astronomers, we shall have a direct proof of the different velocities of the differently-coloured rays, and consequently a mechanical account of their different degrees of refrangibility; for he sees not, to what other cause such an appearance could be reasonably ascribed. If it be not, we may conclude, that rays of all colours are emitted from the luminous body with one common velocity.

Remarks on the Preceding Paper. By Mr. Short. p. 268.

Ever since the above paper was delivered, Mr. S. had carefully attended the emersions of Jupiter's first satellite through a reflecting telescope of 4 feet focal length, and with a proper magnifying power; but he had not perceived the least alteration in the colour of the light reflected by the satellite, except in quantity. It may indeed be observed, that these emersions are seen sooner or later through telescopes of different lengths, and by eyes of different goodness: and it may therefore be alleged, that there is a certain quantity of time elapsed between the very first emersion of the satellite, and the instant when it is perceived by the very best eye, assisted by the best telescope; and that, during this interval, the succession of colours above-mentioned is performed. But our author, in consequence of his hypothesis, says, that this succession of colours may be perceived for the space of 32^s after the first emersion of the satellite; and Mr. S. was fully satisfied, from repeated observations, that the quantity of time elapsed from the very first emersion of the satellite, till it is perceived by a good eye, assisted by a good telescope, can amount only to a very few seconds. So that, on the whole, we may conclude, that it does not appear, by the observations of the emersions of the first satellite of Jupiter, that the rays of different colours move with different degrees of velocity.

But our author's conclusion, that, if the rays of light emitted from Jupiter's satellites, at the time of their immersion and emersion, should not be found of different colours, the rays of all colours emitted from luminous bodies will have one common velocity, seems only to hold good, on a supposition that light is propagated by a continued motion, in the manner of a projectile.

Dr. Knight, in his treatise on attraction and repulsion, prop. 69, has considered the propagation of light, as performed by vibrations in an elastic fluid, in the same manner as sound is produced by vibrations in the air: and he thinks that it is as easy to conceive how the velocities of the particles of light may be different, and yet take up equal times in propagating their motions from one to another through a given space, as to explain how sounds of different tones move with equal velocities. In accounting for both, he shows, that in a series of particles, which mutually repel each other, the greater their velocity, the nearer they will approach each other, in communicating their motions from one to another; and consequently each of them must move through a greater space in so doing: therefore the same time may be spent in propagating a successive motion through a series of particles, whose velocity is greater, if each particle has to move through a greater space, as is spent where the velocity of each particle is less, but is continued through a less space. The dilemma, to which our author's reasoning seems to have reduced the doctrine of refrangibility, may therefore be considered as a probable argument for adopting this hypothesis of the propagation of light through an elastic medium.

XXXIX. The Case of the Operation for the Empyema, successfully performed by Joseph Warner, F.R.S., and Surgeon to Guy's Hospital. p. 270.

John Collier, aged 17, was admitted into Guy's Hospital on the 10th of May, 1753, on account of a complaint in his chest, which he had laboured under for

3 or 4 weeks. His symptoms were a continual pain in his left side, a difficulty in breathing, and an inability of lying on his right side, or of sitting upright, without greatly increasing his complaints. His pulse was quick, and low; he had a short cough, was a good deal emaciated, and appeared sallow in his complexion.

On examination, Mr. W. perceived a small tumor, situated on the anterior part of the thorax obliquely, on the left side of the extremity of the sternum or breast-bone. There was not the least discoloration of the integuments. On pressing on the tumor, his pain and difficulty of breathing were increased, and there appeared something like a fluctuation under the fingers. He had never any rigor, which is a symptom generally attending the formation of matter; but from experience he had found, that the want of this symptom is no proof of the contrary.

From the foregoing circumstances, and symptoms, he had no doubt of the propriety of the operation, which he performed in the following manner: The patient being properly situated and secured, he began with making an incision of about 2 inches long through the integuments, and tendinous expansion of the oblique muscles of the abdomen on the most prominent part of the tumor; then he proceeded to make a 2d incision, of an equal length with the former, transversely through the upper part of the rectus muscle, which had a perfect healthy appearance, directing his knife forwards, between the cartilaginous portions of the 7th and 8th ribs, into the cavity of the thorax; on which a thick clotted matter, to the quantity of 23 oz. and upwards, was discharged. After the whole of the matter was discharged, he introduced the fore-finger of his right hand into the cavity, with which he evidently felt the lungs quite loose, and free from adhesion, the mediastinum, and superior part of the diaphragm; which last had been pressed somewhat lower than its natural situation by the weight of the incumbent matter. Hence it undoubtedly appeared, that this great quantity of matter was contained in the cavity of the thorax.

After the whole of the matter was discharged, he introduced a linen tent, properly secured, into the cavity; which was continued to be introduced every day for about 3 weeks; now and then, as occasion required, making use of the prepared sponge-tent. The discharge of matter was considerable for the first week; then it began to decrease gradually till, at the end of 3 weeks, there was no discharge at all. From this time, superficial applications only were made use of. At the end of 5 weeks he was perfectly well, and soon recovered his former plumpness, and healthy appearance.

He observes, that, about 2 years before, he received a violent blow on his left side by a fall; for which he had little or no care taken of him. He had ever after this accident had some complaints in his side at times, but not constantly;

nor have they ever been so bad, as to prevent his acting in his business as a sailor, till within a few weeks before he applied to Mr. W.

XL. On Infinite Series and Logarithms. By Mr. James Dodson. p. 273.

The terms of one of the most simple series, for expressing the logarithm of a given number, is composed of the powers of the excess of that number above unity, divided by their respective indices; of which the 1st, 3d, 5th, &c. terms are affirmative, and the 2d, 4th, 6th, &c. terms are negative; and the difference between the sums of the affirmative and the negative terms, is the Neperian, hyperbolic, or as some call it, the natural logarithm of the given number.

Now a mathematician, who understands the nature and management of series, (though wholly ignorant of fluxions, or what Dr. Halley, in his investigation of this very series, published in N° 216 of the Philos. Trans. calls ratiunculæ, &c.) might arrive at the same conclusion, in the following manner:

Since the logarithm of 1 is universally determined to be nothing; that of 2, 3, 4, 10, or any other number, considered as a root, is 1; that of 4, 9, 16, 100, &c. considered as the square of that root, is 2, and so on; it follows, that in all cases the logarithm of a greater number will exceed that of its less; and each logarithm will have some relation to the excess of its number above unity, the number whose logarithm is nothing: the terms of the series therefore which will represent the logarithm of any number, will consist of the powers of the excess of that number, above 1, with some, yet unknown, but constant co-efficients.

That the logarithm of the square of any number is twice the logarithm of its root, is a well-known property of those artificial numbers; and therefore the doubles of the particular terms of the assumed series will constitute a series expressing the logarithm of the square of the given number. But by prop. 4, book 2 of Euclid, the square of any quantity is equal the sum of the squares of its 2 parts, plus a double rectangle of those parts; which, in this case (where the given number has been assumed to consist of 1 and an excess) will be 1 plus twice that excess, plus the square of it.

If therefore the several powers of the compound quantity (twice the excess of the given number above 1 plus its square) be multiplied by the above assumed co efficients, and afterwards ranged under each other, according to the powers of the said excess, their sums will again express the logarithm of the square of the given number.

Now since the logarithm of the square of the given number may be thus expressed by 2 infinite series, each constituted of its excess above 1, and its powers; it follows, that the co-efficients of the like powers of that excess, in each series, will be equal between themselves; and consequently the values of the unknown co-efficients may be obtained, by simple equations; and these co-efficients will, by the process annexed, appear to be the reciprocals of the several indexes of the powers of that excess, affected alternately with the signs + and −, as was before found, by the quadrature of the hyperbola, and by Dr. Halley in the above-cited Phil. Trans., and by many who have used a fluxional process.

But there is another logarithmic series, equally simple with the former, consisting of the same terms, but all affirmative. This has been demonstrated to be the logarithm of that fraction, whose numerator is unity, and denominator a number as much less than unity, as the former number exceeded it. Now if an infinite series be formed from that fraction, by actual division, it will consist of unity and all the powers of that defect; and if the several powers of the excess of this infinite series above unity, be multiplied by the co-efficients above found, and ranged according to the powers of that defect, their sums will exhibit the above-described series for the logarithm of that fraction, as appears by the operation subjoined.

Secondly, the terms of one of the best series, for the rectification of the circle, are composed of the odd powers of the tangent of any arc, not exceeding 45°, severally divided by their respective indexes; of which the 1st, 3d, 5th, &c. terms are affirmative; and the 2d, 4th, 6th, &c. terms are negative; and the difference between the sums of the affirmative and negative terms, is the length of that arc, of which the tangent and its powers constitute the series.

Now a mathematician, who understands the nature and management of series, though wholly ignorant of fluxions, might investigate this series in the following manner: It has been geometrically demonstrated that, the radius of a circle being unity, if double the tangent of any arc, be divided by the difference between unity and the square of that tangent, the quotient will be the tangent of twice the arc. Now if an infinite series be formed by actual division, its terms will consist of the doubles of the odd powers of the tangent, and will be all affirmative; which series will express the length of the tangent of the double of that arc whose tangent and its powers constitute the same.

If a series, consisting of the tangent and its powers, with unknown co-efficients, be assumed, as in the former case, to express the length of the arc; then the length of double that arc may be expressed 2 ways; viz. either by multiplying each term of the series assumed by the number 2; or by finding the powers of the series above described, which exhibits the length of the tangent of the double arc, multiplying each power by its proper co-efficient, ranging the products under each other, according to the powers of the tangent of the single arc, and finding their sum. Now, since the length of the double arc may be thus expressed by 2 infinite series, each constituted of the tangent of the single arc and its powers; therefore the co-efficients of the like powers of that tangent, in each series, will be equal between themselves; and consequently the values of the unknown co-efficients may be obtained by simple equations.

Lastly, since the series, which gives the length of the tangent of the double arc, consists only of the odd powers of the tangent of the single arc, therefore none of its even powers can range with it: now these will not occur in the odd powers of that series; and therefore the series assumed to express the length of the single arc, whose double is to be compared with the sum of the former, must consist only of the odd powers of that tangent; and then the series first mentioned results from the operation, as will appear by examining the same, as hereto annexed.

The operation necessary to find the co-efficients of a series, which will express the logarithm of a given number, is as below.

If the given number be represented by $1 + n$, then the following series may be assumed to represent its logarithm:

viz. $n + xn^2 + yn^3 + zn^4 + un^5 +$ &c.

and $2n + 2xn^2 + 2yn^3 + 2zn^4 + 2un^5 +$ &c. will represent the logarithm of the square of that number; viz. of $1 + 2n + nn$.

But, because $2n + nn$ is the excess of $1 + 2n + nn$ above unity, therefore its logarithm will be also expressed by

$(2n + nn) + x\,(2n + nn)^2 + y\,(2n + nn)^3 + z\,(2n + nn)^4$ &c.

$$
\begin{aligned}
\text{Now } (2n + nn)^2 &= 4nn + 4n^3 + n^4 \\
(2n + nn)^3 &= 8n^3 + 12n^4 + 6n^5 + \text{\&c.} \\
(2n + nn)^4 &= 16n^4 + 32n^5 + \text{\&c.} \\
(2n + nn)^5 &= 32n^5 + \text{\&c.}
\end{aligned}
$$

Therefore,

$$
\begin{aligned}
2n + nn &= 2n + nn \\
x\,(2n + nn)^2 &= 4xnn + 4xn^3 + xn^4 \\
y\,(2n + nn)^3 &= 8yn^3 + 12yn^4 + 6yn^5 + \text{\&c.} \\
z\,(2n + nn)^4 &= 16zn^4 + 32zn^5 + \text{\&c.} \\
u\,(2n + nn)^5 &= 32un^5 + \text{\&c.}
\end{aligned}
$$

And the sum of these is equal to the logarithm of the square of $1 + n$.

If an equation be formed, of the co-efficients of n^2, in each of these expressions of the logarithm of that square, then $2x = 1 + 4x$; hence $-\frac{1}{2} = x$.

And, by proceeding in the same manner with the co-efficients of n^3, n^4, n^5, &c. and supplying the places of x, y, z, &c. as they arise, by the numbers so found, we shall have

$$
\begin{aligned}
2y &= -\tfrac{4}{2} + 8y; & \text{hence } +\tfrac{1}{3} &= y; \\
2z &= -\tfrac{1}{2} + \tfrac{12}{3} + 16z; & \text{hence } -\tfrac{1}{4} &= z; \\
2u &= \tfrac{6}{3} - \tfrac{32}{4} + 32u; & \text{hence } +\tfrac{1}{5} &= u;
\end{aligned}
$$

Consequently, the logarithm of $1 + n$ will be expressed by $n - \frac{1}{2}n^2 + \frac{1}{3}n^3 - \frac{1}{4}n^4 + \frac{1}{5}n^5$, &c. as above asserted.

Again, since $\frac{1}{1-n} = 1 + n + n^2 + n^3 + n^4 + n^5 +$ &c. as appears by actual division. And, since the excess of that series above unity, is the series $n + n^2 + n^3 + n^4$ &c.

Therefore the logarithm of $\frac{1}{1-n}$ will consist of the sums of the powers of that series, multiplied by the above-found co-efficients $\frac{1}{1}, -\frac{1}{2}, +\frac{1}{3}, -\frac{1}{4}, +\frac{1}{5}$, &c.

$$\text{Now the } \left\{\begin{array}{c} 2 \\ 3 \\ 4 \\ 5 \end{array}\right\} \text{Power of that series will be} \left\{\begin{array}{r} n^2 + 2n^3 + 3n^4 + 4n^5, \text{ \&c.} \\ n^3 + 3n^4 + 6n^5, \text{ \&c.} \\ n^4 + 4n^5, \text{ \&c.} \\ n^5, \text{ \&c.} \end{array}\right.$$

And,

$$\left.\begin{array}{r} \frac{1}{1} \\ -\frac{1}{2} \\ +\frac{1}{3} \\ -\frac{1}{4} \\ +\frac{1}{5} \end{array}\right\{ \text{of that} \left\{\begin{array}{c} 1 \\ 2 \\ 3 \\ 4 \\ 5 \end{array}\right\} \text{Power will be} \left\{\begin{array}{r} n + n^2 + n^3 + n^4 + n^5, \text{ \&c.} \\ -\frac{1}{2}n^2 - \frac{2}{2}n^3 - \frac{3}{2}n^4 - \frac{4}{2}n^5, \text{ \&c.} \\ +\frac{1}{3}n^3 + \frac{3}{3}n^4 + \frac{6}{3}n^5, \text{ \&c.} \\ -\frac{1}{4}n^4 - \frac{4}{4}n^5, \text{ \&c.} \\ +\frac{1}{5}n^5, \text{ \&c.} \end{array}\right.$$

The sums of which, viz. $n + \frac{1}{2}n^2 + \frac{1}{3}n^3 + \frac{1}{4}n^4 + \frac{1}{5}n^5$, &c. will be the logarithm of $\frac{1}{1-n}$, as above affirmed.

The operation necessary to find the co-efficients of a series, which will express the length of the arc of a circle, by the tangent of that arc, and its powers, is as follows:

Let a represent the length of the arc, and t its tangent; then the tangent of that arc whose length is $2a$, will be $\frac{2t}{1-tt}$; which fraction is equal to the infinite series, $2t + 2t^3 + 2t^5 + 2t^7 + 2t^9$, &c. by division. And by performing the necessary multiplications, or divisions, it will also appear, that

$$\left(\frac{2t}{1-tt}\right)^3 = 8t^3 + 24t^5 + 48t^7 + 80t^9, \text{ \&c.}$$

$$\left(\frac{2t}{1-tt}\right)^5 = 32t^5 + 160t^7 + 480t^9, \text{ \&c.}$$

$$\left(\frac{2t}{1-tt}\right)^7 = 128t^7 + 896t^9, \text{ \&c.}$$

$$\left(\frac{2t}{1-tt}\right)^9 = 512t^9, \text{ \&c.}$$

Now if we assume, for the value of a, the following series, $t + xt^3 + yt^5 + zt^7 + ut^9$, &c. Then $2t + 2xt^3 + 2yt^5 + 2zt^7 + 2ut^9$, &c. $= 2a$.

And because $\frac{2t}{1-tt}$ is the tangent of the arc whose length is $2a$, therefore

$$\frac{2t}{1-tt} + x\left(\frac{2t}{1-tt}\right)^3 + y\left(\frac{2t}{1-tt}\right)^5 + z\left(\frac{2t}{1-tt}\right)^7 \text{ \&c.} = 2a.$$

Which expression is equivalent to the sum of the following series; for

$$\frac{2t}{1-tt} = 2t + 2t^3 + 2t^5 + 2t^7 + 2t^9, \text{ \&c.}$$

$$x\left(\frac{2t}{1-tt}\right)^3 = 8xt^3 + 24xt^5 + 48xt^7 + 80xt^9, \text{ \&c.}$$

$$y\left(\frac{2t}{1-tt}\right)^5 = 32yt^5 + 160yt^7 + 480yt^9, \text{ \&c.}$$

$$z\left(\frac{2t}{1-tt}\right)^7 = 128zt^7 + 896zt^9, \text{ \&c.}$$

$$u\left(\frac{2t}{1-tt}\right)^9 = 512ut^9, \text{ \&c.}$$

And, by making an equation between $2x$, the co-efficient of t^3 in the first found value of $2a$, and $2 + 8x$, the sum of the co-efficients of t^3 in the latter, gives $2x = 2 + 8x$; hence $-\frac{1}{3} = x$.

And by proceeding in the same manner with the co-efficients of t^5, t^7, t^9, &c. and supplying the places of x, y, z, &c. as they arise, by the numbers so found, we shall have

$2y = 2 - \frac{24}{3} + 32y$; hence $y = +\frac{1}{5}$.
$2z = 2 - \frac{48}{3} + \frac{160}{5} + 128z$; hence $z = -\frac{1}{7}$.
$2u = 2 - \frac{80}{3} + \frac{480}{6} - \frac{896}{7} + 512u$; hence $u = +\frac{1}{9}$.

Therefore we may conclude that $t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9$, &c. $= a$.

When the arc is just 45 degrees, then $t = 1$, and the series becomes $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$, &c. which converges exceedingly slow; but, by the assistance of a method, given in the appendix to M. de Moivre's Miscellanea Analytica, it may be transformed to another, converging quicker; which method is applied to this very series, in folio 362 of the Mathematical Repository, vol. i.

XLI. A Letter from John Lining, M. D. of Charlestown, South-Carolina, concerning the Quantity of Rain fallen there from Jan. 1738, to Dec. 1752. p. 284.

Months, &c.	The Means.	Months, &c.	The Means.
Jan.	2.326	Oct.	3.049
Feb.	3.389	Nov.	2.229
March	3.024	Dec.	3.684
April	1.721	Spring	8.068
May	3.655	Summer	14.804
June	5.000	Autumn	16.913
July	6.149	Winter	8.340
Aug.	7.530		
Sept.	6.343	Total Depth	48.023

As there are thunder-gusts here in the hot months, in which a vast quantity of rain falls, the depth of the rain in these months is greatly increased; for there is very little rain, excepting in thunder-showers. Thus, on June 30, 1750, in a thunder-storm, there fell, in 24 hours, 5.335 inches of rain. On September 16, 1751, there fell, in 24 hours (but the greatest part in 6 hours) 9.955 inches of rain. On Sept. 15, 1752, during the time of the most violent hurricane that was ever felt in this town, the depth of rain which fell, was only 3.740

inches, and the greatest part of that was the spray of the sea. And the mean quantity for each month of the aforesaid 15 years, is as in the margin annexed.

XLII. On the Fossil found at Dudley in Staffordshire, and described in the Phil. Trans. N° 496. *By Mr. Emanuel Mendez da Costa, F.R.S.* p. 286.

The famous fossil, which Dr. Lyttelton showed to the R. S. some time ago from Dudley, and which is described in N° 496 of the Trans. caused many arguments as to what class of animals it belonged. Dr. Pococke afterwards produced 2 or 3 specimens of it extended, which proved it to be of the crustaceous tribe of animals. But none of his specimens being very perfect, M. da Costa here sends a fair specimen of the said fossil extended, from the iron mines at Colnbrook-dale in Shropshire, and which determines him to pronounce it to be the remains of a crustaceous animal, of that kind called pediculi marini, which are scaled all round, and can at will roll themselves up: and this particular kind may be justly denominated pediculus marinus major trilobos. See fig. 8, pl. 9.

Though he before thought it not described by any English author, yet he finds it described and figured, though badly, by Mr. Edw. Lhuyd, in his Lithophylacium Britannicum Ichnographicum, Epist. 1, p. 96, table 22; who found them in plenty in quarries, juxta ædes nob. v. D. Gryfidii Rice de Newton, arm. prope oppidum Sancti Teilavii, in comitatu Maridunizæ. He calls it buglossa curta strigosa. He also gives the figure of it without any description, in the Phil. Trans. N° 243.

XLIII. Letters relating to a Theorem of Mr. Euler, of the Royal Acad. of Sciences at Berlin, and F.R.S. for Correcting the Aberrations in the Object-Glasses of Refracting Telescopes. p. 287.

Letter I. From Mr. James Short, F.R.S. to Peter Daval, Esq. F.R.S. Dated April 9, 1752. p. 287.

There is published, in the Memoirs of the Royal Acad. at Berlin, for the year 1747, a theorem by Mr. Euler, in which he shows a method of making object-glasses of telescopes, in such a manner, as not to be affected by the aberrations arising from the different refrangibility of the rays of light; these object-glasses consisting of two meniscus lenses, with water between them.

Mr. John Dollond, who is an excellent analyst and optician, has examined the said theorem, and has discovered a mistake in it, which arises by assuming an hypothesis contrary to the established principles of optics; and in consequence of this Mr. Dollond has sent me the inclosed letter, which contains the discovery of the said mistake, and a demonstration of it.

In order to act in the most candid manner with Mr. Euler, I have proposed to Mr. Dollond to write to him, showing him the mistake, and desiring to know his reasons for that hypothesis; and therefore I desire, that this letter of Mr. Dollond's to me may be kept among the Society's papers, till Mr. Euler has had a sufficient time to answer Mr. Dollond's letter to him.

Letter II. From Mr. John Dollond to James Short, A. M., F. R. S. concerning a Mistake in M. Euler's Theorem for Correcting the Aberrations in the Object-glasses of Refracting Telescopes. Dated March 11, 1752. p. 289.

The famous experiments of the prism, first tried by Sir Isaac Newton, sufficiently convinced that great man, that the perfection of telescopes was impeded by the different refrangibility of the rays of light, and not by the spherical figure of the glasses, as the common notion had been till that time; which put the philosopher on grinding concave metals, in order to come at that by reflexion, which he despaired of obtaining by refraction. For, that he was satisfied of the impossibility of correcting the aberration by a multiplicity of refractions, appears by his own words, in his treatise of Light and Colours, Book i. part 2, prop. 3. "I found moreover, that when light goes out of air through several contiguous mediums, as through water and glass, as often as by contrary refractions it is so corrected, that it emerges in lines parallel to those in which it was incident, continues ever after to be white. But if the emergent rays be inclined to the incident, the whiteness of the emerging light will by degrees, in passing on from the place of emergence, becomes tinged in its edges with colours."

It is therefore somewhat strange, that any person should now attempt to do that, which so long ago has been demonstrated impossible. But, as so great a mathematician as Mr. Euler has lately published a theorem * for making object-glasses, that should be free from the aberration arising from the different refrangibility of light, the subject deserves a particular consideration. I have therefore carefully examined every step of his algebraic reasoning, which I have found strictly true in every part. But a certain hypothesis in p. 285 appears to be destitute of support either from reason or experiment, though it be there laid down as the foundation of the whole fabric. This gentleman puts $m : 1$ for the ratio of refraction out of air into glass of the mean refrangible rays, and $M : 1$ for that of the least refrangible. Also for the ratio of refraction out of air into water of the mean refrangible rays he puts $n : 1$, and for the least refrangible $N : 1$. As to the numbers, he makes $m = \frac{31}{20}$, $M = \frac{77}{50}$, and $n = \frac{4}{3}$; which so far answer well enough to experiments. But the difficulty consists in finding the value of N in a true proportion to the rest.

Here the author introduces the supposition above-mentioned; which is, that

* Vide Memoires of the Royal Acad. of Berlin for the year 1747.—Orig.

m is the same power of M, as n is of N; and therefore puts $n = m^a$, and $\text{N} - \text{M}^a$. Whereas, by all the experiments that have hitherto been made, the proportion will come out thus, $m - 1 : n - 1 :: m - \text{M} : n - \text{N}$.

The letters fixed on by Mr. Euler, to represent the radii of the 4 refracting surfaces of his compound object-glass, are f, g, h, k, and the distance of the object he expresses by a; then will the focal distance be $= \frac{1}{n(\frac{1}{g} - \frac{1}{h}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) - \frac{1}{a} - \frac{1}{f} + \frac{1}{k}}$. Now, says he, it is evident, that the different refrangibility of the rays would make no alteration, either in the place of the image, or in its magnitude, if it were possible to determine the radii of the four surfaces, so as to have $n(\frac{1}{g} - \frac{1}{h}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) = \text{N}(\frac{1}{g} - \frac{1}{h}) + \text{M}(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k})$. And this I shall readily grant. But when the surfaces are thus proportioned, the sum of the refractions will be $= 0$; that is to say, the emergent rays will be parallel to the incident. For, if $n(\frac{1}{g} - \frac{1}{h}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) = \text{N}(\frac{1}{g} - \frac{1}{h}) + \text{M}(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k})$, then $n - \text{N}(\frac{1}{g} - \frac{1}{h}) + m - \text{M}(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) = 0$. Also if $n - \text{N} : m - \text{M} :: n - 1 : m - 1$, then $n - 1(\frac{1}{g} - \frac{1}{h}) + m - 1(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) = 0$; or otherwise $n(\frac{1}{g} - \frac{1}{h}) + m(\frac{1}{f} - \frac{1}{g} + \frac{1}{h} - \frac{1}{k}) - \frac{1}{f} + \frac{1}{k} = 0$; which reduces the denominator of the fraction expressing the focal distance to $\frac{1}{a}$. Hence the focal distance will be $= a$; or, in other words, the image will be the object itself. And as, in this case, there will be no refraction, it will be easy to conceive how there should be no aberration.

And now Sir I think I have demonstrated, that Mr. Euler's theorem is entirely founded on a new law of refraction of his own; but that, according to the laws discovered by experiment, the aberration arising from the different refrangibility of light at the object-glass, cannot be corrected by any number of refractions whatever.

Letter III. From Mr. Euler to Mr. James Short, F.R.S. Dated Berlin, June* 19, 1752. p. 292.

Mons.

Vous m'avez fait un tres sensible plaisir, en ayant disposé M. Dollond de remettre la proposition de ses objections contre mes verres objectifs, jusqu' à ce que j'y aurois repondu, et je vous en suis infiniment obligé. Je prends donc la liberté de vous addresser ma reponse à lui, en vous priant, après l'avoir daignée de votre examen, de la vouloir bien lui remettre: et en cas que vous jugiez cette matiere digne de l'attention de la Societé Royale, je vous prierois de lui communiquer les preuves detaillées de ma theorie, que j'ai exposée dans cette lettre. Cependant j'espere, que M. Dollond en sera satisfait, puisque je tombe d'accord

* As these letters are on nice controversial matters, it is considered safer and more satisfactory to give them in their original language.

avec lui du peu de succes, qu'on sauroit se promettre de mes objectifs, en les travaillant selon la maniere ordinaire.

Letter IV. From M. Euler to Mr. Dollond. Dated Berlin, June 15, 1752. p. 293.

Etant tres sensible à l'honneur que vous me faites, au sujet des verres objectifs, que j'avois proposé, j'ai celui de vous marquer d'abord ingenumen, que j'ai rencontré aussi ici les plus grands obstacles dans l'execution de ce dessein, vu qu'il s'agit de quatre faces, qui doivent etre travaillées exactement selon les proportions que j'avois trouvées: cependant ayant fait les experiences sur quelquesuns, qui parurent le mieux reussi, nous avons trouvé, que l'intervalle entre les deux foyers des rayons rouges et violets etoit beaucoup plus petit, qu'il ne seroit d'un verre simple de la meme distance focale. Neant-moins je dois avoüer, qu'un tel verre, quand même il bien seroit parfaitement executé sur mes principes, auroit d'autres defauts, qui le mettroient au dessous même des verres ordinaires; c'est qu'un tel verre n'admet qu'un tres petite ouverture en consequence des grandes courbures, qu'on doit donner aux faces interieures: desorte que lorsqu'on donne une ouverture ordinaire, l'image devient tres confus.

Ainsi puisque vous vous etes donné la peine, monsieur, d'executer de tels verres, en en faisant des experiences,* je vous prie de bien distinguer les defauts, qui peuvent naitre de la diverse refrangibilité des rayons, de ceux, qui viennent d'une trop grande ouverture: pour cet effet vous n'aurez qu'à laisser une tres petite ouverture.

Or si ma theorie etoit juste, dont j'aurai bientot l'honneur de parler, il seroit moyen de remedier à ce defaut; il faudroit renoncer à la figure spherique qu'on donne ordinairement aux faces des verres, et tacher de leur donner une autre figure, et j'ai remarqué que la figure d'une parabole leur procureroit l'avantage, qu'ils admettroient une ouverture tres considerable. Notre savant M. Lieberkuhn s'est appliqué à travailler des verres dont la courbure des faces décroit depuis le milieu vers le bords, et il s'en est aperçu de tres grands avantages. Par ces raisons je crois, que ma theorie ne souffre encore rien de ce cotè.

Pour la theorie, je conviens avec vous, monsieur, que posant la raport de refraction d'un milieu dans un autre quelconque pour les rayons moyens comme m à 1, et pour les rayons rouges comme M à 1, la raison de $m - \mathrm{M}$ à $m - 1$ sera toujours si à peu prés constant, qu'elle satisfera à toutes les experiences, comme la grand Newton a remarqué. Cette raison ne differe non plus de ma theorie que presque imperceptiblement; car puisque je soutiens que $\mathrm{M} = m'$, et que m

* Mr. Dollond, in his letter to Mr. Euler, here referred to, does not say that he had made any trials himself, but only he had understood that such had been made by others, without success.—Orig.

diffère ordinairement fort peu de l'unité, soit $m = 1 + \omega$; et puisque $M = m^{\alpha} = 1 + \alpha\, lm$ à peu pres, et $l\,(1 + \omega) = lm = \omega$, aussi fort à peu pres, j'aurai $m - M = 1 + \omega - 1 - \alpha\omega = (1 - \alpha)\,\omega$, et $m - 1 = \omega$, donc la raison $\frac{m - M}{m - 1}$ sera $= 1 - \alpha$, ou fort à peu pres constante. Delà je concluds, que les experiences d'où le grand Newton a tiré son raport, ne sauroient etre contraires à ma theorie.

En second lieu, je conviens aussi que si la raison $\frac{m - M}{m - 1}$ = constant etoit juste à la rigueur, il n'y auroit plus moyen de remedier au defaut qui resulte de la diverse refrangibilité des rayons, de quelque maniere qu'on disposeroit divers milieux transparens, et que l'intervalle entre les divers foyers tiendroit toujours un raport constant à la distance focale entiere du verre. Mais c'est precisement cette consideration, qui me fournit le plus fort argument: l'oeil me paroit une telle machine dioptrique parfaite, qui ne se ressent en aucune maniere de la diverse refrangibilité des rayons: quelque petite que soit sa distance focale, sa sensibilité est si grande, que les divers foyers, s'il y en avoit, ne manqueroient pas de troubler tres considerablement la vision. Or il est bien certain, qu'un oeil bien constitué ne sent point l'effet de la diverse refrangibilité.

La structure merveilleux de l'oeil, et les diverses humeurs, dont il est composé, me confirme infiniment dans ce sentiment. Car s'il s'agissoit seulement de produire une representation sur le fond de l'oeil, une seule humeur auroit été suffisante; et le createur n'y auroit pas surement employé plusieurs. Delà je concluds, qu'il est possible d' anéantir l'effet de la diverse refrangibilité des rayons par une juste arrangement de plusieurs milieux transparens, donc puisque cela ne seroit pas possible, si la formule $\frac{m - M}{m - 1}$ = constant etoit vraye à la rigueur, j'en tire la consequence qu'elle n'est pas parfaitement conforme à la nature.

Mais voila une preuve directe de ma these: je conçois divers milieux transparens, A, B, C, D, E, etc. qui different entr'eux également par raport à leur densité optique: desorte que la raison de refraction de chacun dans le suivant soit le meme. Soit donc dans le passage du premier dans le second la raison de refraction pour les rayons rouges $= r : 1$, et pour les violets $= v : 1$; qui sera la meme dans le passage du second dans le troisieme, de celuicy dans le quatrieme, du quatrieme dans le cinquieme, et ainsi de suite. Delà il est clair, que dans le passage du premier dans le troisieme sera $= r^2 : 1$ pour les rayons rouges, et $= v^2 : 1$ pour les violets: de meme dans le passage du premier dans le quatrieme les raisons seront $r^3 : 1$ et $v^3 : 1$.

Donc si dans le passage dans un milieu quelconque la raison de refraction des rayons rouges est $= r^n : 1$, celle des rayons violets sera $= v^n : 1$; tout cela est parfaitement conforme aux principes du grand Newton. Posons $r^n = R$, et $v^n = V$, desorte que $R : 1$, et $V : 1$ expriment les raisons de refraction des rayons rouges et violets dans un passage quelconque: et ayant $nlr = lR$, et $nlv = lV$, nous aurons $l\text{R} : lr = l\text{V} : lv$, ou $\frac{l\text{R}}{l\text{V}} = \frac{lr}{lv}$. Ou bien mettes $v = r'$, et à cause de $lv = \alpha lr$, on aura $\frac{l\text{R}}{l\text{V}} = \frac{1}{\alpha}$, ou $l\text{V} = \alpha l\text{R}$, et partant $\text{V} = \text{R}'$.

Voilà donc le fondement du principe, que j'ai employé dans ma piece, qui me paroit encore inebranlable; cependant j'en soumets la decision à l' illustre Societé Royale, et à votre jugement en particulier, ayant l'honneur d'être avec la plus parfaite consideration, Monsieur, &c. &c.

XLIV. A remarkable Case of Fragility, Flexibility, and Dissolution, of the Bones. By John Pringle, M.D., F.R.S. p. 297.

Mary Hayes, of Stoke-Holy-Cross, near Norwich, gave the following account, June 21, 1752. That she was born Jan. 11, 1718, and never married, nor was addicted to any kind of intemperance; that her father was unhealthy a great part of his life, but she knew not what disease he was subject to; that her mother died when she was a child; but she did not remember having ever heard of her being unhealthy; that she herself was always considered as a healthy strong girl, till about 15 years of age; then fell into the green-sickness, and took various medicines, to no purpose; that this disease, as far as she could recollect, was all she had to complain of; doing the ordinary work in a farmer's house, till October 1748; she then was seized with pain universally, attended with feverish symptoms. Thus she continued some weeks; after which the pain was chiefly confined to her thighs and legs, but not increased by external pressure. In September 1749, she broke her leg, as she was walking from the bed to her chair, without falling down, and heard the bones snap. The fracture was properly treated, and regard had to her disposition; but no callus was generated, the bones growing flexible from the knee to the ancle in a few months, as did those of her other leg. Soon after, those of her thighs were visibly affected in like manner. Both legs and thighs then became very œdematous, and subject to excoriate, discharging a thin yellow ichor. The winter after breaking her leg, she had symptoms of the scurvy, and bled much at the gums.

Many eminent physicians, who were of opinion that this disease of the bones might arise from acidity abounding in the blood, prescribed for her, but without effect; unless the regularity of her menstruation for the last 18 months may be attributed to a chalybeate medicine; though medicines of that nature had no such effect formerly, when she was in a condition to take exercise, and regularly persisted in the use of them.

For some considerable time past she had found little alteration in her complaints in general; thought her appetite and digestion rather better, but that the difficulty of breathing, which she had long laboured under, gradually increased; and the thorax appeared so much straitened, as necessarily impeded the expansion of the lungs. Her spine became much distorted; any motion of the vertebræ of her loins gave extreme pain; and her thighs and legs were become entirely useless; which wholly confined her to her bed, in a sitting posture: and the bones she rested on, having lost their solidity, were much spread. Also the ends of her fingers and thumbs, by frequent endeavours to lift herself up for ease, became very broad and flat. Then she measured but 4 feet; though, before this disease came on her, she was about 5¼ feet high, and well shaped.

This is the best information that could be obtained from her own mouth, and what was observed in the case before, and at the first-mentioned time, when she readily consented to the examination of her body, &c. after death.

From that time to her death, which happened Feb. 6, 1753, the chief thing she complained of, and what the people about her observed, was a gradual increase of difficulty of breathing; a wasting of her flesh; a cessation of her menstruation for the last 4 months; a tendency in her legs to mortify, which had long been anasarcous, and excoriated almost all over; she retaining her senses perfectly to the last moment of her life, and dying without showing the least signs of the agonies of death.

Two days after death, her limbs being first well stretched out, she was exactly measured, and found wanting of her natural stature more than 2 feet 2 inches. Then the thorax and abdomen were opened, the sternum being entirely removed, with part of the ribs, in order to gain at once a full view of those cavities, and discover how the viscera there contained had obstructed each other in their respective functions. The heart and lungs were sound, but flaccid, and much confined in their motion; to which the enormous size of the liver contributed in some measure, extending quite across the abdomen, and bearing hard against the diaphragm. The lungs did not adhere to the pleura: nor was the liver scirrhous, but faulty only in its bulk. The mesentery was sound, except only one large scirrhous gland on it. The spleen extremely small. Nothing else was found observable in those cavities.

The skull was not opened, to examine the brain, as intended, through want of time, the minister waiting at church for interment, and the relations becoming impatient; but the operators had no reason to suspect any defect there, from any previous complaint.

All her bones were more or less affected, and scarcely any would resist the knife; those of the head, thorax, spine, and pelvis, nearly to the same degree of softness; those of the lower extremities much more dissolved than those of the upper, or of any other part. They were cut quite through their whole length, without turning the edge of the knife, and much less resistance was found, than firm muscular flesh would have made; being changed into a kind of parenchymous substance, like soft dark coloured liver, only meeting here and there with bony laminæ, thin as an egg-shell.

Those bones were most dissolved which, in their natural state, were most compact, and contained most marrow in their cavities; and the heads of them were least dissolved. This perhaps is the more worthy observation, as it held good throughout, and looks as if the wonderful change they had undergone might be caused by the marrow having acquired a dissolving quality; for it was evident that the dissolution began withinside, from the bony laminæ remaining here and there on the outside, and no where else, and the pain not being increased at first by external pressure.

The periosteum was thicker than ordinary: the cartilages rather thinner; but no where in a state of dissolution like the bones. The day after this examination, some of the whole substance of the leg and thigh bones, that was entirely dissolved into a kind of pulp, was sent to an ingenious chemist; and, by the experiments which he made, he said he could discover neither acid nor alkali prevailing in it.

XLV. Astronomical Observations made in Surry-street, London. By J. Bevis, M. D., and James Short, A. M., F. R. S. p. 301.

Eclipse of Venus by the moon, apparent time, July 26, 1753.

16^h	2^m	17^s	Venus totally hid by the moon.
17	5	6. .	Her northern cusp emerged; and, a few seconds after, her southern one.
	5	31. .	Venus was totally emerged. All these with a reflector of 2 feet focus.
			Then her diameter was found to be 32¾″, with a new kind of micrometer; and also with one of Mr. Graham's sort, in a 2 feet Gregorian reflector.

Eclipse of Mars by the moon, Aug. 20.

17	6	49½	The moon's consequent limb passed the meridian.
	8	4. .	Mars's centre passed the meridian.
			His diameter then, with both micrometers, 13¼″.
			The moon's diameter 31′ 21″.
18	6	59½	Mars totally hid by the moon with a reflector of 4 feet focus.

Occultation of β Capricorni by the moon, Oct. 5.

7	16	50. .	The moon's preceding limb passed the meridian.
	20	4. .	A small star, which preceded β, passed the meridian.
	20	19.	β passed the meridian.
			Presently after, the moon's diameter was found to be 29′ 48″, with the new micrometer, applied to a reflector of 2 feet focus.

8^h 21^m 3^s The small star eclipsed by the moon.
28 48. . β eclipsed by the moon.
9 48 24. . β emerged from the moon.

Eclipse of the sun, Oct. 25.

20 30 10. . The eclipse had been some time begun; but, for clouds, could not be seen till now; when the distance between the cusps, measured with the new micrometer, applied to a 2 feet reflector, was 12′ 26½″.
21 15 23. . The distance between the cusps 29′ 49″.
18 6. . The distance between the visible limbs of the sun and moon 11′ 32″.
22 18 56. . The distance between the cusps 24′ 12½″.

The day before, about 10 in the morning, the sun's horizontal diameter was 32′ 17″.

These measures were all taken when the sun continued visible but for a few seconds, through the interstices of flying clouds; and yet from the nature of this micrometer, they may be very safely relied on: though it would have been impossible to have catched any one of them with the common micrometer.

The principle on which this most excellent instrument is constructed, was laid before this Society last May: and it is to be hoped that Mr. Dollond will evince the certainty of its measurements, from the least to the greatest angle it is capable of comprehending; and that, under every consideration of reflexion as well as refraction by spherical surfaces; so as to leave no room for such objections or cavils, as otherwise may probably be brought against it. For our own parts, we are fully satisfied of the justness of it, from a great variety of trials and comparisons. That which we have hitherto used, is the first that has been made of the kind: and might perhaps have been better constructed in some respects, though in nothing material.

Applied to a reflector of only 2 feet, the scale is as large as the common micrometer can have in a 40-foot refractor; and all is done without the help of screws or wires; so that there is no need of illuminating. In virtue of such a scale it is, that even fractions of seconds may be depended on: as we have found, by often repeated trials on the diameters of the planets. These, as well as small distances of stars, may be measured in all directions, with equal and almost incredible facility, without a polar axis; as well out of doors, in a rough wind, as within.

XLVI. Concerning a Cluster-Polype, found in the Sea near the Coast of Greenland. By Mr. John Ellis.* p. 305.

This marine production, sent him by Mr. Collinson, appears to be an animal,

* *Vorticella encrinus.* Linn. *Pennatula encrinus.* Linn. Gmel.

not a vegetable, being a species of cluster-polype, consisting of many bodies united at one common base. This specimen appears to have 23 distinct ones; he saw another, that was taken at the same time, that had between 30 and 40. Each body is furnished at the top with 8 arms or tentacula, which expand themselves in the form of a star. Each arm is again furnished on each side with a row of small fibres, which seem to do the office of fingers. In the centre of the 8 arms appears the mouth, surrounded by 6 little semicircular lips standing upright.

On dissecting one of the bodies lengthwise, it appeared to consist of a strong muscle, contracted into little waves or wrinkles. In the little cavities of these are sundry small seed-like particles, perhaps the spawn of the animal: when magnified, they appeared of a spherical form, a little compressed. To the centre of the base, where the cluster of polypes unite, and make one body, there grows a four-square bony stem of 6 feet long, having 4 grooves, one on each side. At the joining to the fleshy part, the bony stem is very small. and a little twisted, like the turn of a screw, extending a membrane like a bladder, for about 2 or 3 inches in length, and nearly an inch in breadth, from the fleshy part downwards. The membrane then begins to close insensibly, and becomes a cuticular covering to the bony stem, which now increases gradually, till it becomes a quarter of an inch square. Within 5 or 6 inches of the bottom of the stem the bony part begins to grow smaller, till it comes to a point; and the cuticular part becomes cartilaginous, and supplies this tapering part with a quantity of this elastic substance, equal to the deficiency of the bone. The use of this membrane, or bladder-like skin at the top of the stem, may possibly be intended to give the animal a power to raise and fall itself in the water at pleasure. It appears from the twist in one part of the stem, that the stem, when very small, and not so bony, had met with some violence, that had turned it out of its direction; the mark of which has still grown on with it: for the stem of the other specimen, taken at the same time, was quite even.

On cutting it across, they discovered the distinct laminæ to each angle, rising from a small point in the centre, and separated by a cross, that joins the opposite grooves. On putting a thin shaving of it into vinegar, a strong effervescence was immediately raised, which dissolved the gritty or coralline part, and discovered the fine membranes that enclosed it. These two substances seem to compose this bony, ivory, or coral-like stem.

The disposition of the polypes, with regard to each other, is represented by a cross section in pl. 9, fig. F, where 10 occupy the outward circle, 9 are in the next, and 4 are in the centre.

Mr. Ellis learned that it was taken in the latitude of 79° north; which is within 11° of the pole, and 80 English miles from the coast of Greenland, by

Captain Adriaanz, commander of the Britannia, while he was on the whale fishery last summer. The captain sounding one day in very deep water 236 fathom, 2 of them clung to his line. He says the arms or tentacula of the polypes were of a bright yellow colour, and fully extended, when he brought them to the surface of the water; and made a most agreeable figure, like a fine full-blown flower, which the captain took them for. Mr. Ellis further observes, that the encrinos, or the lilium lapideum of the curious in fossils, so little known before, is thought to be of this class.

References to the figures in pl. 9.—A, the clustered polype in its natural size, extending itself; B, the same polype, as it was received, after it had been soaked in water, and the tentacula laid straight; a, the polype in miniature, with its stem of bone or ivory; c, part of the ivory stem twisted; D, the lower part of the stem, covered with a cartilage; E, the cartilage opened, to show the tapering of the bony part; F, the cross section, to show the position of the several bodies of the polype; H, the cross section of the bony stem magnified; G, one of the bodies cut open, to show its internal muscular form; l, the eggs or spawn in the natural size; L, the same magnified; I, the cuticular covering, which is continued from the bladder at M to the cartilage at E, or from one end of the stem to the other; N, the indented muscular base, where the bodies of the polype all unite; K, a figure of the encrinos, or lilium lapideum, from Rosinus.

XLVII. Extracts of two Letters from Father Gaubil, of the Society of Jesus, at Pekin in China, translated from the French. Dated Pekin, Nov. 2, 1752. p. 309. 1. *To the R. S.*

The Chinese, without being consummate, or even passable astronomers, might be capable of observing an eclipse, and of making observations on it, and of looking on the shadow of the gnomon of a sun-dial. The knowledge, which they had from time immemorial of the rectangle triangle, and of its principal properties, might easily teach them a thousand curious things in geometry, without knowing the theory of trigonometry.

The Chinese, from time immemorial, knew the passage of the sun in the ecliptic; they knew the stars; they had globes and hemispheres; and, by means of divers practices and precepts, received from their ancients, without any great knowledge of spherical trigonometry, might be able on the globe itself to resolve many problems. We ought to conclude, that our ancients were possessed of several kinds of knowledge, received from the patriarchs, and transmitted to the Chinese. Without these kinds of knowledge, and these traditions, by mere observations alone, the Chinese could not perform what they did at first. They never well understood the stations and retrogressions of the planets. Reflections on the eclipses of the sun and stars taught them anciently, by practice, something of the parallaxes of the moon.

Every thing was almost forgotten, about the time of Tsin chi hoam, 240 or 246 years before Christ. But it is evident that, before that time, the Chinese must have known something of the calculations of the eclipses of the sun and moon, and of some equations for reducing the mean motion to the true, and for calculating the solstices. Mengtse, a classical author, who wrote before the burning of their books, mentions clearly enough, part at least of what is here said. They certainly knew indifferently well the proper motion of the fixed stars; which was afterwards forgotten, for want of examining what was extant written in many books.

On the 15th of August, an ambassador from the king of Portugal arrived at Macao, with presents for the emperor of China. The queen-mother of the king of Portugal ordered the ambassador to desire, that Father Hallerstein, whom she personally knew, might come to him to Macao, with a mandarin sent by the emperor. The emperor consented to this without any difficulty, and dispatched the mandarin and Father Hallerstein to the ambassador. He will be here again in May. I am of opinion that the reigning emperor will never permit any missionaries in the provinces; and that they will find it very difficult to conceal themselves. But there is no appearance that we shall be sent away from Pekin; on the contrary those who shall be sent thither, will be well received, if they have but the qualifications requisite.

Letter 2. To Mons. De l'Isle of the Royal Academy of Sciences at Paris. Dated Pekin, Nov. 18, 1751. p. 313.

I had furnished M. Freret with a quantity of memoirs, as I had likewise done to others, both seculars, and those of our own society. I digested into order all that I had collected; and, in 1749, sent a complete treatise on the Chinese chronology, by two different ways, into France. I directed it to M. Freret, and to the fathers of our society at Paris. It was in 3 parts. I desired them to communicate it to you, and to Mons. de Mairan. I have had no account of the arrival of that treatise, in which I had laboured for more than 22 years past. It seemed to me necessary, on account of the great number of pieces, either printed or manuscript, which were sent hither on that subject. If I find that my treatise is lost, I can easily digest it into order again, from the rough draught which I have by me.

Besides many astronomical observations, which I have punctually sent you, I have transmitted to you the treatise of Father Duchamp on the Indian astronomy, a collection of ancient approximations and occultations of the stars and planets, both by each other, and by the moon, and with the moon; which I had collected and made for determining the longitude and latitude of Pekin, &c. This year I have sent to Paris, by two different ways, a memoir, which had been desired of me, concerning the isles of Lequoyo, or Licoukicou, which Kempfer calls Roukou. It is a pretty long one. I had an opportunity of being well informed about these isles; but there are many things yet wanting to be known. To this memoir I have added some remarks concerning the longitude of Namgazaki, and other places on the south coast of Japan, and the south coast of Corée, with its distance from Japan, and the island of Touyma, which, in the map of Father du Halde, is called Touyla Tao, or Touyla. It should be called Touy Ma. It is the isle Tsutsima. It depends on Japan. I have spoken here with several Coreans, who have been in that island.

I have already sent to you observations made here to the close of the year 1750, and during this year. I now send you others of 1750; and others I inclosed to you at large in 1749 and 1750. I wait for some answer from you; and especially your opinion concerning the manner, in which I ought to dispose my memoirs concerning the Chinese astronomy. I am resolved to put my last hand to that work. But memoirs of that kind ought to be examined by persons intelligent and zealous like yourself.

At Petersburgh you must undoubtedly have seen what I wrote to Mr. Bayer about what the Chinese have said concerning the Huns and Turks. Dr. Mortimer has written to me, that he had received from a nephew of Mons. Fourmont, a small piece on the origin of the Turks and Huns, as drawn from the Chinese books. I shall speak again of that subject in the memoirs which I have of the history of the great dynasty of Tang. There are a great number of very interesting things on what the Chinese have delivered at that time concerning the empire of the Persians, and its destruction by the Mahometans; concerning the Mahometans, and the assistance which they gave to Chinese emperors against their rebels; concerning the Christian religion, or the Tatsin, but in very obscure terms; concerning the sects and countries of the Indians, Japan, Corée, Tartary, and the countries between China and the Caspian sea, Thibet, and its princes. All these particulars may be of considerable service to unravel the eastern history from the year 500 of Christ to the year 1000 before him, and even much higher.

There are here a great number of Lamas and Tartars, who have gone from Lassa, the capital of Thibet, to the lakes and mountains, where the sources of the Ganges are, and at Latac, &c. in the country to the north of Thibet and Latac; but what they say is extremely confused; and this part of geography is still very little known to us here.

XLVIII. A Letter of Mr. William Sherrington to Benjamin Franklin, Esq. concerning the Transit of Mercury over the Sun, on the 6th of May 1753, as observed in the Island of Antigua. Communicated by Mr. Peter Collinson, F.R.S. Dated Antigua, June 20, 1753. p. 318.

Sunday, May 6, at $6^h\ 7^m\ 51^s$, he observed the western limb of Mercury to touch the western limb of the sun; and, at $6^h\ 10^m\ 37^s$, he touched the same with his eastern limb, and totally disappeared. Lat. of the place 17° 0′ N. Lon. by estimation 61° 45′ W. from London. This was taken by a Graham's watch, and corrected by two altitudes taken by a most exquisite quadrant; which makes the true apparent time of the transit at $6^h\ 6^m\ 32^s\ 32^{th}$.

XLIX. Of the Barometer and the State of the Weather, at Dublin, from March 7, 1752, to Feb. 1753. By James Simon, Esq. F.R.S. p. 320.

This register contains the daily height of the barometer, with the state of the weather, as to wind, rain, &c. but is of no manner of consequence now.

L. A Second Account of the New Method of Opening the Cornea, for taking away the Cataract. By Samuel Sharp, Surgeon to Guy's Hospital, and F.R.S. p. 322.

Mr. S. here gives a short account of the success of his new method of removing a cataract, with some observations on the principal phenomena attending this operation; to which he adds a description of a further improvement of the operation itself. For a fuller view of the history of these cases, he has here set down the ages of the patients, the dates of the days on which they underwent the operation, and the particular circumstance of its being done on one or both eyes. This was a list of 11 patients, from 48 to 70 years of age, on whom the operation had been performed.

From this catalogue it appears, that the operation had been performed on 19 eyes; and, from the most exact information, which he had been able to procure, the state of the success stood thus: AC, AD, AF, AG, AL, all whom had the operation performed on both eyes, had every one of them recovered the sight of both eyes, to as great a perfection as can be supposed, without the help of the crystalline humour; that is, they could read and write, with proper spectacles. The first of them, AC, had found so much benefit, as to be able to carry on the exercise of his profession, that of a surgeon. AH saw with both eyes, but not so well as the other 5. He had received an account from the surgeon, who had attended her (in a distant country), that her eyes looked well, and her sight improved.* AI, another patient, at a distance from London, had the operation done on one eye only; which he recovered, as his correspondent informs him, so as to see tolerably well. AM, on one eye only, with which he already sees very well. AE had it performed on both ; one of which was lost, and the other recovered; but continued inflamed, and could not bear much light. AB had it done on one eye only, which was lost.

Both the eyes, in which the operation failed, were destroyed by the subsequent inflammation ; but in the case of AB, the ill success was partly owing to the imperfection of the instrument ; a disadvantage that must frequently attend on the execution of new attempts. It was the first operation he had performed, and he had provided a knife with so thin a blade, that after he had passed through the cornea into the anterior chamber of the eye, the point was so blunted, that, on endeavouring to carry it through the cornea out on the other side, the blade bent, and he was apprehensive it might break: however, withdrawing it a little, he made 2 or 3 efforts, and succeeded in the incision, and the removal of the cataract. During this operation, the aqueous humour being discharged, and the patient struggling, he wounded the iris; which bled profusely, and continued for several days to discharge a great quantity of blood, and bloody ichor: and to this accident was imputed the miscarriage of the operation; though Mr. Daviel affirms, that wounds of the iris had been very seldom followed with bad effects in his practice.

He had reserved the mention of AK's history to the last, because of its singularity. She was altogether as blind as those whose cataracts are ripe; but her's had the appearance of a beginning cataract, being of a light blue, and but little opaque. On making the compression, the crystalline did not advance through the pupil, as in the other instances: and he found, that if he exerted more force, he should soon evacuate all the vitreous humour. It was evident, by the great distance of the cataract behind the iris, that this disappointment did not arise from an adhesion to the iris: however, he had immediately recourse to the experiment of cutting through the capsula with the point of his knife; hoping by that means to have set free the crystalline, but it gave him no assistance. He then passed the curette (a little scoop) through the pupil, and turned it several times round, in expectation of breaking the capsula; but found not the least resistance to his instruments; so that both operations proved ineffectual; the circumstances being exactly the same in each eye. He had, in couching, met with cataracts of this nature; but had no apprehension that he could not have discharged, by the wound of the cornea, the matter of a cataract, in however fluid a state it might prove.

* Some weeks after this paper was read, Mr. Sharp received an account, that the pupils of both eyes had contracted so much, as hardly to leave room for the admission of light; and it was apprehended the patient would soon become blind.—Orig.

Of all the 19, there was not one that escaped an inflammation; whereas, after couching, there are great numbers who have neither inflammation nor pain. But it was to be remarked, that notwithstanding the violent inflammation, which sometimes ensued after the incision of the cornea, even to an enlargement of the eyelids, and vesication of the tunica conjunctiva, the patient complained rather of a tenderness of the eye, on touching it, than of pain; being generally exempt from those dreadful dartings in the head, which for the most part accompany an inflammation after couching. And he believed he might assert, that none suffered very much in that particular, except AE; who was extremely bad, and lost the eye on that side where the pain was.

It could not, he presumed, be difficult to conceive how these inflammations should excite such different symptoms, on reflecting, that in the incision of the cornea, the cornea only suffers; and in couching, the conjunctiva, the sclerotica, the choroides, and the tunica retina, are punctured; most of which organs are either tendinous or nervous; and every surgeon knows the painfulness and obstinacy of inflammations, when they follow upon wounds and punctures of tendinous or nervous parts. He had not mentioned, in this comparison, the violence done to the vitreous humour; because he believed it did not occasion the subsequent pain; and because it seemed to be often as much or more injured in the new operation, without inconvenience.

It had not occurred in any of these cases, that the inflammation had been so slight, as to disappear entirely in a fortnight, or 3 weeks; most of them requiring 6 weeks, and some longer, for the total removal of them. The first 10 days, or more, the light was generally very offensive; and he had observed, in 3 or 4 instances, that on forcibly opening the eyelids during that time, the patient was only sensible of a glare of light, though the eye then appeared clear, and he afterwards recovered his sight. Which he mentioned to obviate the melancholy prognostic one would be disposed to make on a first examination. However, this was not to be understood as a constant fact; some patients distinguishing objects immediately from the time of the operation.

It sometimes happens, after this operation, that the pupil loses its circular figure; which he imagines is owing to the great tenderness of the iris, which, on the least violence, is subject to be ruptured; and he supposes in this operation, a slight pressure from the back or the flat of the blade may have produced the accident in the instances alluded to. Possibly the sudden dilatation of the pupil, from the rapid passage of the cataract through it, may sometimes occasion it; but the following history would induce one rather to ascribe it to the cause which he first mentioned.

Before he had thought of the knife for opening the cornea, he used the scissars, as Mons. Daviel directs; and in a certain patient, after he had made the wound of the cornea, and was going to compress the eye, for the expulsion of the cataract, he discovered, that from the disturbance he had given to the humours by the foregoing process, it was sunk almost as much as if it had been depressed by a couching needle. He therefore left it in that situation, and the man afterwards saw very well; though the cataract remained visible something below the pupil. Now in this instance the cataract had not passed through the pupil; and yet it was lacerated, so as to lose its circular form; but whatever may be the cause, he did not find, that the accident itself proved prejudicial to the sight. He adds that when an incomplete gutta serena is complicated with the cataract, the operation is of no avail.

It remains now to speak of the operation itself. In his former paper, after having described the manner of making the incision, he directed the operator to compress the inferior part of the globe of the eye with his thumb gently, till the cataract should be expelled through the incision of the cornea, on the patient's cheek; and in this method he had performed it on several subjects. But remarking, that though on the evacuation of the aqueous humour, the crystalline readily advanced through the pupil into the anterior chamber, yet that it required some force to expel it from its membrane through the wound of the cornea, and in that action it sometimes suddenly drew after it a portion of the vitreous humour, he changed his method, and no longer pressed the eye when once the crystalline was in the anterior chamber, but immediately stuck the point of his knife into the body of it, and extracted it contained in its capsula, without spilling any of the vitreous humour.

This new process, he supposes, would be found of considerable advantage, as it would in a great measure remove the danger of evacuating the whole, or too much of the vitreous humour: though it might be observed, to the praise of this operation, that, contrary to expectation, a large quantity of this humour, perhaps a 3d part, or more, had been sometimes discharged, without any bad consequence.

He supposes, that the great and sole benefit arising from this improvement, is the easy separation of the crystalline from the bed of the vitreous humour, so that none of this humour shall be evacuated. But perhaps it would also be approved of, as it would render unnecessary the measure prescribed by Mons. Daviel, of wounding the membrane of the crystalline, before we proceed to the extraction of the crystalline itself: to which purpose he advises the flap of the cornea to be suspended with a small spatula; then, with a pointed cutting needle, to wound the surface of the crystalline; after which, to introduce the same spatula through the pupil, in order to detach the cataract from the iris, and then proceed to the expulsion.

He had here recited these processes of M. Daviel's operation, which are calculated merely to procure an easy separation of the crystalline from the vitreous humour: but they are difficult to the operator, fatiguing to the patient, and, he should hope, altogether needless, if the knife be used in the manner which he has recommended: for whether the capsula of the crystalline be nothing more than the duplicature of the membrane of the vitreous humour, or whether it be a proper coat, which is also covered by the membrane of the vitreous humour; in either case, since by compression the crystalline advances with so much facility through the pupil, it will be easily seized by the knife, and removed from the vitreous humour, with its inveloping membrane: whereas, in making an incision on the surface of the crystalline, and wounding its capsula, the crystalline will frequently slip out of the capsula, which will be left behind: and in fact this has happened to M. Daviel, who advises pincers, and other instruments, to extract the remaining membrane. But he observes, in regard to the capsula of the crystalline, that should the humour slip out of it, before it be seized by the knife, it possibly will waste; for in milky cataracts, when the fluid is discharged, the membrane in length of time wastes: whole cataracts, with the inveloping membrane likewise, sometimes waste: and in one of his patients, the crystalline, from the mere pressure in the operation, burst out of its capsula, which he left in the eye; but in some weeks it entirely wasted. However, if the removing of the capsula should, by future experience, be found necessary, it may be conveniently done by the curette; one of the instruments M. Daviel recommends for that purpose. This instrument may be also used for the extraction of a cataract, which has been broken to pieces by the couching needle in a former operation, and for the removal of the capsula of a bag-cataract, when the fluid only has been discharged, and the bag remains behind; but it will be most eminently useful in detaching the crystalline from the back part of the iris, when any portion of it happens to adhere: which circumstance would render the operation fruitless, without such a precaution.

It had not happened, in any of the cases treated, that either during the operation, or after the operation, the iris had been pushed forwards, or insinuated itself through the wound of the cornea, forming a staphyloma; but M. Daviel speaks of it as an occurrence he had met with, and says it may easily be replaced by the small spatula.

Mr. S. hopes that when this operation is more generally practised, ingenious men will render it still more perfect: and he should not be surprised, if the use of a speculum oculi should hereafter be esteemed an improvement: but then it must be contrived so, as that it shall not compress the globe of the eye; or, if it does, the operator must be careful to remove it in the instant the incision is making, lest, by continuing the pressure after the wound is made, all the humours should suddenly gush out.

LI. An Attempt to explain an Ancient Roman Inscription, cut on a Stone lately found at Bath. By John Ward, LL.D., V.P.R.S. p. 332.

The stone was discovered on the 22d of June last, about 5 feet under ground, in digging the cellar of a house, rebuilding at the lower end of Stall-street. Among the rubbish of the old house, when it was pulled down, was a large quantity of walling stone, which had on it the marks of fire: so that probably some building had formerly stood there, which was burnt. And in sinking the ground about 4 or 5 feet lower than the stone, they found 2 coins of the emperor Carausius, in base metal, and very much defaced. In July 1727 the beautiful gilt head, which is now preserved in the town house, was dug up at the other end of this street, not far from the King's bath, about 16 feet below the surface of the earth, as they were making a common sewer through the town.

The stone, on which this inscription is cut, has been generally taken for a pedestal, either of a statue, or some other solid body, which it once supported. Though from the appearance of the horizontal plane at the top Mr. Prince Hoare, the ingenious statuary at Bath, is of opinion, that nothing was formerly placed on it; and supposes that the sinking in the middle, with the 2 lines erased, one on each side, might be made merely for ornament. Besides, the face and 2 sides only are finished; the back being flat, as if it was designed to stand against a wall. The height of it, which is very near 3 feet; as also the form both of the stone itself, and the plane above mentioned; appear by the draughts of them taken by Mr. Hoare. From a careful examination of the whole inscription, as it appeared in the cast taken by Mr. Hoare, Dr. W. copied it in the draught of the stone; and endeavoured to express the several letters in their proper form and proportional size, with the ligatures, divisions of the words, and their situations in each line, in the most exact manner he was capable of doing it. And on considering the whole in this view, he offers the following reading in words at length, as what appears to him the most probable:

Locum religiosum, per insolentiam erutum,
virtuti et numini Augusti repurgatum
reddidit Caius Severius Emeritus, centurio,
sua *pecunia.*

Dr. Ward thinks this a monumental stone, brought from some Roman burial place.

But who the reigning emperor was, at the time this stone was set up, no intimation is given in the inscription. Though, if one may be allowed to conjecture, the form of the letters suits very well with some others in the reign of

Severus. And perhaps no time was more open for such licentious practices, as might justly merit the name insolentia, than the loose reign of Commodus; who though he was not the immediate predecessor of Severus, yet died but a few months before he came to the empire. Besides, we have two other inscriptions found in Britain, addressed Numinibus Augustorum; both which are thought to relate to Severus and his elder son Caracalla, after he was joined with his father in the government. Nor can there be any doubt of this, as to one of them at least; which is an altar, and has on one side of it the names of both his sons, Caracalla and Geta, as consuls that year. So that on the whole, Dr. W. can find no other period of time so probable for fixing the date of this inscription.

LII. On some Electrical Experiments, made at Paris. By Mr. Benjamin Wilson, F.R.S. p. 347.

Mr. W. being at Paris, M. Mazeas informed him that Dr. le Monnier, some months ago, had read a paper at a meeting of the Royal Acad. of Sciences, in which he told them, that he had great reason to believe the electric matter did not come from the earth at all, but from the air. On Mr. W. mentioning this to the Doctor, he found him still of the same opinion. As there was a convenient apparatus in his apartment, Mr. W. proposed making the experiments: for he always thought that the electric matter came from both, but principally from the earth; and that probably a difference of 10 to 1 would be perceived, on making the experiments.

The machine was suspended by silk lines in such a manner, that every part of it was not less than 2 feet distant from any non-electric. The lines were dried by a chafing-dish of fire made with charcoal, as was also the glass globe; and every other precaution was strictly observed, that seemed necessary for making the experiments.

The doctor appeared to be well versed in electrical inquiries, and showed great judgment in conducting the whole. He got upon the suspended apparatus himself, and rubbed the globe with both his hands; while another person, who was likewise suspended, turned the wheel of the machine. Close to the globe was a slender slip of lead; at one end of which was fastened some brass tinsel, to serve as a collector of the electric matter. The other end of the lead had a communication with a tin tube, which was supported by silk lines about a foot in length: and as this tube hung higher than could be reached, another was hooked to it by means of a wire which hung down to a convenient distance.

As Mr. W. stood on the floor, he took hold of this last tube, while the glass was rubbed, that the apparatus, and the persons on it, might lose as much of their natural electricity as possible under such circumstances. On removing his hand, and afterwards approaching the tube, sometimes with his finger, and at other times with a key, they observed very small explosions, which were little more than just sensible.

Mr. W. then desired one of the doctor's servants, who also stood upon the floor, to lay hold of the suspended apparatus on which the doctor was mounted, while the friction of the globe was continued. Immediately on Mr. W. approaching the tube as before, with his finger, and then with the key, a very great difference was observed; for now the explosion was very large compared with the former trials. Dr. le Monnier desired the experiments might be repeated: which was done several times, and to all appearance the differences were the same. He was perfectly satisfied that the experiments were fairly made, and that the explosion was much greater when the apparatus communicated with the earth, than when it communicated with the air only.

LIII. Electrical Experiments, with an Attempt to Account for their Several Phenomena. Also some Observations on Thunder-clouds. By John Canton, M.A., F.R.S. p. 350.

Exp. 1.—From the ceiling, or any convenient part of a room, let 2 cork-balls, each about the size of a small pea, be suspended by linen threads of 8 or 9 inches in length, so as to be in contact with each other. Bring the excited glass tube under the balls, and they will be separated by it, when held at the distance of 3 or 4 feet; let it be brought nearer, and they will stand farther apart; entirely withdraw it, and they will immediately come together. This experiment may be made with very small brass balls hung by silver wire; and it will succeed as well with sealing-wax made electrical, as with glass.

Exp. 2.—If 2 cork balls be suspended by dry silk threads, the excited tube must be brought within 18 inches before they will repel each other; which they will continue to do, for some time, after the tube is taken away.

As the balls in the first experiment are not insulated, they cannot properly be said to be electrified: but when they hang within the atmosphere of the excited tube, they may attract and condense the electrical fluid round about them, and be separated by the repulsion of its particles. It is conjectured also, that the balls at this time contain less than their common share of the electrical fluid, on account of the repelling power of that which surrounds them; though some perhaps is continually entering and passing through the threads. And if that be the case, the reason is plain, why the balls hung by silk, in the 2d experiment, must be in a much more dense part of the atmosphere of the tube, before they will repel each other. At the approach of an excited stick of wax to the balls, in the first experiment, the electrical fire is supposed to come through the threads into the balls, and be condensed there, in its passage towards the wax: for, according to Mr. Franklin, excited glass emits the electrical fluid, but excited wax receives it.

Exp. 3.—Let a tin tube, of 4 or 5 feet in length, and about 2 inches in diameter, be insulated by silk; and from one end of it let the cork balls be suspended by linen threads. Electrify it, by bringing the excited glass tube near the other end, so as that the balls may stand an inch and a half, or 2 inches apart: then, at the approach of the excited tube, they will by degrees lose their repelling power, and come into contact; and as the tube is brought still nearer, they will separate again to as great a distance as before: in the return of the tube they will approach each other till they touch, and then repel as at first. If the tin tube be electrified by wax, or the wire of a charged phial, the balls will be affected in the same manner at the approach of excited wax, or the wire of the phial.

Exp. 4.—Electrify the balls as in the last experiment by glass; and at the approach of an excited stick of wax their repulsion will be increased. The effect will be the same, if the excited glass be brought towards them, when they have been electrified by wax.

The bringing the excited glass to the end, or edge of the tin tube, in the 3d experiment, is supposed to electrify it positively, or to add to the electrical fire it before contained; and therefore some will be running off through the balls, and they will repel each other. But at the approach of excited glass, which likewise emits the electrical fluid, the discharge of it from the balls will be diminished; or part will be driven back, by a force acting in a contrary direction; and they will come nearer together. If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them, above the common quantity in air, be equal to the excess of the density of that within them, above the common quantity contained in cork: their repulsion will be quite destroyed. But if the tube be brought nearer; the fluid without, being more dense than that within the balls, it will be attracted by them, and they will recede from each other again.

When the apparatus has lost part of its natural share of this fluid, by the approach of excited wax to one end of it, or is electrified negatively; the electrical fire is attracted and imbibed by the balls to supply the deficiency; and that more plentifully at the approach of excited glass, or a body positively electrified, than before; whence the distance between the balls will be increased, as the fluid surrounding them is augmented. And in general, whether by the approach or recess of any body; if the difference between the density of the internal and external fluid be increased, or diminished; the repulsion of the balls will be increased, or diminished, accordingly.

Exp. 5.—When the insulated tin tube is not electrified, bring the excited glass tube towards the middle of it, so as to be nearly at right angles with it, and the balls at the end will repel each other; and the more so, as the excited tube is brought nearer. When it has been held a few seconds, at the distance of about 6 inches, withdraw it, and the balls will approach each other till they touch; and then separating again, as the tube is moved farther off, will continue to repel when it is taken quite away. And this repulsion between the balls will be increased by the approach of excited glass, but diminished by excited wax; just as if the apparatus had been electrified by wax, after the manner described in the 3d experiment.

Exp. 6.—Insulate 2 tin tubes, distinguished by A and B, so as to be in a line with each other, and about half an inch apart; and at the remote end of each let a pair of cork balls be suspended. Towards the middle of A, bring the excited glass tube; and holding it a short time, at the distance of a few inches, each pair of balls will be observed to separate; withdraw the tube, and the balls of A will come together, and then repel each other again; but those of B will hardly be affected. By the approach of the excited glass tube, held under the balls of A, their repulsion will be increased: but if the tube be brought, in the same manner, towards the balls of B, their repulsion will be diminished.

In the 5th experiment, the common stock of electrical matter in the tin tube is supposed to be attenuated about the middle, and to be condensed at the ends, by the repelling power of the atmosphere of the excited glass tube, when held near it. And perhaps the tin tube may lose some of its natural quantity of the electrical fluid, before it receives any from the glass; as that fluid will more readily run off from the ends or edges of it, than enter at the middle: and accordingly, when the glass tube is withdrawn, and the fluid is again equally diffused through the apparatus, it is found to be electrified negatively: for excited glass brought under the balls will increase their repulsion.

In the 6th experiment, part of the fluid driven out of one tin tube enters the other; which is found to be electrified positively, by the decreasing of the repulsion of its balls, at the approach of excited glass.

Exp. 7.—Let the tin tube, with a pair of balls at one end, be placed 3 feet at least from any part of the room, and the air rendered very dry by means of a fire: electrify the apparatus to a considerable degree; then touch the tin tube with a finger, or any other conductor, and the balls will still continue to repel each other; though not at so great a distance as before.

The air surrounding the apparatus to the distance of 2 or 3 feet, is supposed to contain more or less of the electrical fire, than its common share, as the tin tube is electrified positively, or negatively; and when very dry, may not part with its overplus, or have its deficiency supplied so suddenly, as the tin; but may continue to be electrified, after that has been touched, for a considerable time.

Exp. 8.—Having made the Torricellian vacuum about 5 feet long, after the manner described in the Phil. Trans. vol. xlvii. p. 370, or p. 236 of this vol. of these Abridgments, if the excited tube be brought within a small distance of it, a light will be seen through more than half its length; which soon vanishes, if the tube be not brought nearer; but will appear again, as that is moved farther off. This may be repeated several times, without exciting the tube afresh.

This experiment may be considered as a kind of ocular demonstration of the truth of Mr. Franklin's hypothesis; that when the electrical fluid is condensed on one side of thin glass, it will be repelled from the other, if it meets with no resistance. According to which, at the approach of the excited tube, the fire is supposed to be repelled from the inside of the glass surrounding the vacuum, and to be carried off through the columns of mercury, but as the tube is withdrawn, the fire is supposed to return.

Exp. 9.—Let an excited stick of wax, of 2½ feet in length, and about an inch in diameter, be held near its middle. Excite the glass tube, and draw it over one half of it; then, turning it a little about its axis, let the tube be excited again, and drawn over the same half; and let this operation be repeated several times; then will that half destroy the repelling power of balls electrified by glass, and the other half will increase it.

By this experiment it appears that wax also may be electrified positively and negatively. And it is probable, that all bodies whatever may have the quantity they contain of the electrical fluid, increased, or diminished. The clouds he has observed, by a great number of experiments, to be some in a positive, and others in a negative state of electricity. For the cork balls, electrified by them, will sometimes close at the approach of excited glass; and at other times be separated to a greater distance. And this change he has known to happen 5 or 6 times in less than half an hour; the balls coming together each time, and remaining in contact a few seconds, before they repel each other again. It may likewise easily be discovered, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by a positive one: and by whichever it be electrified, should that cloud either part with its overplus, or have its deficiency supplied suddenly, the apparatus will lose its electricity: which is frequently observed to be the case, immediately after a flash of lightning. Yet when the air is very dry, the apparatus will continue to be electrified for 10 or 15 minutes, after the clouds have passed the zenith; and sometimes till they appear more than half-way towards the horizon. Rain, especially when the drops are large, generally brings down the electrical fire; and hail, in summer, he believes never fails. When the apparatus was last electrified, it was by the fall of thawing snow; which happened so lately as on the 12th of November; that being the 26th day, and 61st time, it has been electrified, since it was first set up; which was about the middle of May. And as Fahrenheit's thermometer was but 7 degrees above freezing, it is supposed the winter will not entirely put a stop to observations of this sort. At London, no more than 2 thunder storms have happened during the whole summer: and the apparatus was sometimes so strongly electrified in one of them, that the bells, which have been frequently rung by the clouds, so loud as to be heard in every room of the house, the doors being open, were silenced by the almost constant stream of dense electrical fire, between each bell and the brass ball, which would not suffer it to strike.

Mr. C. concludes this paper with the following queries:

1. May not air, suddenly rarefied, give electrical fire to, and air suddenly condensed, receive electrical fire from clouds and vapours passing through it?

2. Is not the aurora borealis, the flashing of electrical fire from positive, towards negative clouds at a great distance, through the upper part of the atmosphere, where the resistance is least?

LIV. Extract of a Letter from Professor Bose, dated Wittemberg, Aug. 1, 1753. *With Observations on it by Mr. Wm. Watson, F. R. S.* p. 358.

In the beginning of August 1752, after great and continued rains, many of our rivers overflowed the neighbouring grounds, more or less according to their level, to a considerable distance; and the quantity of water was so great, that in some places it was not discharged for more than a week. More particularly the river Unstrut in the territory of the landgrave of Thuringue required a long time to empty itself, not only as that river runs over a large tract of country, but also as between Artern and great Jena, where this river joins the Sales, its bed in several places is very much confined.

When the inundation was abated, it was observed from the little city Laucha quite up above Artern, not only on the fields and meadows, but also on the bushes and trees, that there was a green and very tough viscous slime, which by the help of a stick could be drawn out to 2 or 3 ells in length. The subsequent heat of the sun dried this matter, and it appeared like wool on the bushes; but the fields, when seen at a distance, seemed as if covered with sand. This matter had a smooth appearance outwards, but within was like a sheep's skin. Downwards next the ground it had a sort of wool; and when the whole was washed with soap, it whitened, and appeared like a clean fleece of white wool. Of this substance the country people soon made wicks for their lamps, and several lined their clothes with it, as they would with fur.

It was further observed, that where this substance was mowed off from the meadows, the grass under it was quickly dried up; but, where it was not removed, the grass in the following December was as green and fresh as in the spring. Thus far Mr. Bose. On which Mr. Watson observes, that the vegetable substance, which, on the specimen sent over by the professor, he has intitled "a sort, perhaps, of alcyonium molle," is a species of that genus of plants, which the more modern botanists call byssus. And it is of that species, or a very slight variety from it, which is called by Dillenius, in his Historia Muscorum, byssus tenerrima viridis velutum referens. It is also mentioned and figured by Micheli in his Nova Plantarum Genera, under the title of byssus terrestris viridis herbacea et mollissima, filamentis ramosis et non ramosis. This genus of plants, in the order of nature, comes between the mosses and fungi. The specimen now sent, being white on one side, arises from its either being washed or bleached by the sun; for when wet, according to Mr. Bose, it was green; and this colour is mentioned both by Dillenius and Micheli in their several denominations. This vegetable is found in England, as well as in many parts of Europe, in moist meadows, covering the ground like a carpet, and sometimes to a great extent.

We must be careful, however, how we connect the substance in question, and others of the same genus with the βύσσος of the ancient Greek writers, or the byssus of the Latin. What that substance was, has been matter of great controversy. This is certain, that garments made of it were the apparel of the rich. And in the New Testament, St. Luke, in the parable of the rich man and Lazarus, says of the former, as a mark of his opulence, ἐνεδιδύσκετο πορφύραν καὶ βύσσον; this is translated in our English version, "he was clothed in purple and fine linen." It is more probable, that the byssus of the ancients was a very fine sort of cotton; but whoever wishes to examine what has been said on this subject, may consult Pliny * and Wormius;† but, above all, Bodæus à Stapel,‡ in his Commentary on Theophrastus; who has on this occasion, as well as on many others, given us an ample testimony of his vast erudition.

LV. Account of a Memoir read at the Royal Academy of Sciences at Paris, by M. de Barros, a Portuguese Gentleman, concerning certain Phenomena observed by him at Paris, in the last Transit of Mercury over the Sun. By J. Short, A. M., F. R. S. p. 361.

The author says, he used an excellent Gregorian reflector 4 feet in length, taking in the eye-piece, and as much of the great tube, as exceeds the focal lengths of the two eye-glasses; probably it should be the two speculums: that the focus of the great speculum is 33 Paris inches; that of the small one 4 inches; the focus of the eye-glass next the eye 18 lines; the focus of the glass farthest from the eye 5 inches; and, lastly, that the combined power of these 2 glasses is nearly equal to that of a single eye-glass of 3 inches. The telescope

* Plinii lib. xix, c 1. † Mus. p. 139. ‡ P. 425, et seq.

therefore, according to Mr. Short's computation, magnified about 130 times. He was placed in the most commodious situation for observing the egress; his smoked glass was fixed perpendicular to the axis of his telescope within a close tube; and he always used the same part of this glass.

He took notice, that the interior contact of Mercury's and the sun's limbs, at $10^h\ 18^m\ 41^s$, was very rapid, having observed it with a green-coloured glass held over the smoked glass: immediately after which, looking through the smoked glass only, he perceived that a small thread of light was still visible between the limbs, before what he calls the second contact took place, which was not till 4 seconds after; that the exterior contact appeared stationary, or seemed to last 6 or 7 seconds; that having observed the total egress with the coloured glass on the smoked one, he brought Mercury on the sun's limb again, by removing the coloured glass; and that the second total egress did not happen till 6 or 7 seconds after the first. When he observed him at the distance of about 3 of his diameters from the sun's limb with both the glasses, he remarked that the same distance seemed diminished, and Mercury's diameter increased. That the part of the sun's limb where Mercury went off, to the extent of 6 degrees of circumference, seemed under much the same configuration, as the illuminated limb of the moon about the quadrature, somewhat uneven and undulating. The same looked also redder than the rest of the disk. This was about 18 or 20 seconds before Mercury disappeared, and was seen through the smoked glass alone; for when the green glass was applied, the appearance in a manner vanished.

The evening before the transit he viewed the sun with different coloured glasses, variously combined with each other, and with a smoked glass; and found, that a green glass before the smoked one did best; the sun appearing of a silvery hue, like the moon, and the spots and the limb exceedingly well defined.

M. de Barros, having thus described the particular phenomena, ingeniously attempts to account for them all, from this single supposition; that the disk of the sun, and of Mercury seen on it, are environed with a certain corona of light (like that which Sir Isaac Newton calls the circle of aberration or dissipation in refracting telescopes) by which the apparent diameter of the sun is enlarged, and that of Mercury contracted. But as this gentlemen made use of a reflecting telescope, and as no such circle, from the known principle of reflection, can take place in such a telescope, if well made, as Sir Isaac has proved long since; Mr. S. thinks it not worth while to pursue him through all his particular suppositions; but only to show that his hypothesis has really no foundation.

Sir Isaac, as before hinted, remarks, that the images of all objects seen in refracting telescopes, are surrounded with a circle of aberration; which is always less, the longer the telescopes are. In his optics, to avoid the indistinctness arising from this circle, he would propose catadioptric telescopes, in which, if the speculums, under limited apertures, be justly figured, no such circle of aberration can confuse the image; but if the speculums are of a spherical figure, with too large apertures, then a circle of aberration will take place; as it also will when the figure deviates from the circular towards the hyperbolic, even under a small aperture, and the same thing will happen, if the spherical figure be inaccurate.

About 3 days from the change of the moon, her whole body is visible; that part of the limb, which is directly enlightened by the solar rays appearing to the naked eye, as an arc of a greater circle than the other, which receives the reflex light from the earth. Look through a refracting telescope, and you will perceive the apparent difference of these circles very much diminished; and if they be viewed with a good reflector, they will be perfectly reduced to an equality, even if measured with a micrometer in the focus.

If a reflecting telescope, well constructed, be directed any considerable time to the sun, such a circle of aberration will be generated, from the little speculums being heated, and thereby its figure altered, from the sun's rays falling condensed on it from the great one; and if it continues long under this circumstance, the image will be rendered utterly indistinct and confused.

This we were thoroughly convinced of at the above-mentioned transit of Mercury; for a good reflector, which we used in taking, with the micrometer, the differences of right ascension and declination between the planet and the sun's limb, having been a good while exposed to the direct rays, was found at last to give a very indistinct image; but was restored to its former degree of perfection, by turning it from the sun, and screwing off the eye-piece, so as to admit the cool air into the great tube, by which the over-heated small speculum soon recovered its due temper and figure. The last-mentioned effect is scarcely sensible in the less reflectors of small apertures; but in those of large ones it is very considerable.

Dr. Bevis, Mr. Canton, and Mr. Bird, who viewed Mercury going off the sun, with very good reflectors of different lengths, assured him, they saw him quite distinct, and free from any corona, or circle of aberration, and the sun's limb perfectly well defined. And he appeared to Mr. S. through a reflector of 4 feet focus, magnifying about 135 times, as truly defined as he could wish to see a black circle on a white ground. On this occasion however Mr. S. takes notice, that during the whole time of this transit of Mercury, the air was perfectly calm with us: but that, in the last two transits of Mercury over the sun, viz. in the years 1736 and 1743, both the sun's and Mercury's limbs appeared to him indistinct, and surrounded with something like what this gentleman calls a luminous crown, or circle of aberration; though Mr. S. at both these times made use of reflecting telescopes, which he had by former trials esteemed good. But it is to be observed that, during both these transits, there was a constant hard gale of wind; and as he had, by other observations, formerly found that the images of the planets, in the night-time, did not appear so distinct in windy weather as when it was calm, he therefore imputed the indistinctness of the sun's and Mercury's limbs to the air's being agitated by the wind.* Of this we may be made sensible by a familiar instance:

Suppose a vessel full of water, having any thing lying at the bottom, as a shilling, the water being at rest; you will then perceive the image of the shilling distinctly; but if you give any commotion to the water, the image of the shilling will then appear indistinct and confused.

Somewhat analogous to this is this other appearance: if you look through a telescope at any of the planets, when the stars appear hazy, dim, and languid, you will see them distinctly: but look at them again, when the stars appear most bright and sparkling, you will then find their images less distinct. This may be accounted for by the just-mentioned instance of the vessel of water, by supposing air instead of water. And if we consider the infinite number of heterogeneous particles which continually float in the air, and suppose these to be at rest, or put into motion, we shall find that it is not at all surprizing, that we see the images of objects placed beyond the medium of air, more or less distinct. We are not so sensible of this indistinctness, arising from the agitation of the air, in refracting telescopes, as in reflectors: because the errors of reflexion, caused by any irregularity in their figure, or confusion in the air, are about 5 or 6 times greater than the same errors in refraction; even though both telescopes magnify the same number of times; as has long been demonstrated.

We also took notice of M. de Barro's first phenomenon; viz. the seeming greater velocity of Mercury when he was near the egress: which we thus accounted for. When he was at a considerable distance from the limb, there being nothing near enough to refer his velocity to, he seemed in a manner stationary; but being advanced near the sun's edge, we could refer his motion to that with ease; which thus becoming sensible, it might be esteemed rapid, in comparison of the former. Mr. S. had often made the same remark on the gradual approach of two luminous bodies, as the appulse of the moon's lucid limb to a star or planet.

The expedition with which the author observed his 2d phenomenon, is extraordinary; viz. that he should first observe what he names the final contact; 2dly, that he should take away his green glass; and thirdly, that he should be able suddenly to alter the conformation of his eye, so as to see distinctly with a much greater influx of light, and then take another observation, and all in the short

* Since this paper was read, Mr. Short has been informed by M. le Monnier, the French king's astronomer, that, during the last transit at Paris, they had a hard gale of wind from the N. E.—Orig.

space of 4 seconds! On the whole, it may be concluded, that the several phenomena, observed by this gentleman, in the transit and egress of Mercury, were owing to indistinctness of vision, arising either from the eye, the telescope, or the air; and that this alone may account for them all, without having recourse to supposed circles of aberration; which can never possibly exist in a well-constructed reflecting telescope.

LVI. An Explanation of an Obscure Passage in Albert Girard's Commentary on Simon Stevin's Works, p. 169, 170. By Mr. Simson, Prof. Math. Glasgow. Communicated by Philip, Earl Stanhope. p. 368.

" Puis que je suis entré en la matiere des nombres rationaux, j'adjousteray encore deux ou trois particularitez, non encor par cy devant practiquées, comme d'expliquer les radicaux extremement pres, &c."

The first thing Albert Girard gives in this place is a method of expressing the ratio of the segments of a line cut in extreme and mean proportion, by rational numbers, that converge to the true ratio. For this purpose he takes the progression 0, 1, 1, 2, 3, 5, 8, 13, 21, &c. every term of which is equal to the sum of the two terms that precede it, and he says, any number in this progression has to the following, the same ratio (nearly) that any other has to that which follows it. Thus 5 has to 8 nearly the same ratio that 8 has to 13; consequently, any 3 numbers next one another as 8, 13, 21, nearly express the segments of a line cut in extreme and mean proportion, and the whole line; so that 13, 21, 21, (N. B. 13 is wrong printed for the second number, instead of 21) constitute near enough an isosceles triangle, having the angle of a pentagon; i. e. whose angle at the vertex is subtended by the side of a pentagon in the circle described about the triangle.

Now this will be plain, if it be shown, that the squares of the numbers in this series are alternately lesser and greater by an unit, than the product of the two numbers on each side. Thus, in the 4 numbers, 5, 8, 13, 21, the square of 8 is a unit less than the product of 5 and 13; but the square of 13 that next follows 8, viz. 169, is a unit greater than 8 times 21, or 168; and so on constantly.

Case 1. If a, b, c, be such numbers, that $a + b = c$, and $ac = bb + 1$.

Then, if d be taken so that $d = b + c$; then shall $bd + 1 = cc$. For, because $d = b + c$; $bd + 1$ shall be $= bb + bc + 1 = ac + bc$, which is $= (a + b) \times c = cc$: ergo $bd + 1 = cc$.

Case 2. If a, b, c, be such, that $a + b = c$, and $ac + 1 = bb$.

Then, if d be taken so that $d = b + c$; then shall $bd = cc + 1$. For, because $bd = bb + bc = ac + bc + 1 = (a + b) \times c + 1 = cc + 1$.

Problem. Having given the number a, in case 1; to find b and c, i. e. having given a, to find b such, that $bb + 1 = (ac =) . aa + ab$; then is $bb - ab = aa - 1$: and therefore $b = \frac{1}{2}a + \frac{1}{2}\sqrt{5aa - 4}$. Hence, to make b a rational integer number, $5aa - 4$ must be a square, which it will be, if $a = 1$; and then b will also be 1, and c will be 2: and having continued the series, every number will have the properties mentioned.

The 2d thing which Albert Girard mentions, is a way of exhibiting a series of rational fractions, that converge to the square root of any number proposed, and that very fast. He tells nothing about the way of forming it, and only gives the two following examples, viz. He says, $\sqrt{2}$ is equal nearly to $\frac{577}{408}$: or, if you would have it nearer, to $\frac{1393}{985}$.

His other example is of $\sqrt{10}$, which, he says, is nearly equal to $3\frac{53353}{328776}$, i.e. to $\frac{1039681}{328776}$. And these are the fractions your lordship has turned, at first sight into continued fractions of the same value.*

The way of making a series of rational fractions, which converge to the square root of any number proposed, in such a manner, that the square of the numerator of any of them being lessened by a unit, or in some cases increased by a unit, the remainder or sum, divided by the square of the denominator, shall be exactly equal to the number proposed, depends on the following propositions.

Prop. 1. Let a be any number proposed, and $\frac{b}{c}$ be such a fraction, that $\frac{bb-1}{cc} = a$, i.e. $bb = acc + 1$; then if two other fractions be taken, one of which is $\frac{b}{ac}$, the first divided by the proposed number a, and the other is $\frac{c}{b}$ the reciprocal of the first fraction; then the fraction $\frac{bb + acc}{2bc}$, whose numerator is the sum of the products of the numerators, and of the denominators of the fractions $\frac{b}{c}$ and $\frac{b}{ac}$; and its denominator the sum of the products of the numerators, and of the denominators of the fractions $\frac{b}{c}$ and $\frac{c}{b}$, shall have the same property with the fraction $\frac{b}{e}$ i.e. $\frac{(bb + acc)^2 - 1}{(2bc)^2} = a$. Because $bb = acc + 1$, therefore $bb - acc = 1$, and squaring

$$b^4 - 2ab^2c^2 + a^2c^4 = 1. \text{ And adding } 4ab^2c^2 \text{ gives}$$

$$b^4 + 2ab^2c^2 + a^2c^4 = 4ab^2c^2 + 1. \text{ Hence } \frac{(b^2 + acc)^2 - 1}{(2bc)^2} = a.$$

Prop. 2. If $\frac{b}{c}$ be such a fraction, that $\frac{bb+1}{cc} = a$, i.e. $bb + 1 = acc$, all

* N.B. That the continued fraction here alluded to, for expressing the square root of 10, was
$\frac{1}{6} \times 19 - \frac{1}{38}$
$- \frac{1}{38}$
$- \frac{1}{38}$
$- \frac{1}{38}$, &c. ad infinitum.—Orig.

other things remaining as in prop. 1; then shall the fraction $\frac{bb + acc}{2bc}$, formed as there described, be such, that $\frac{(bb + acc)^2 - 1}{(2bc)^2} = a$.

For because $bb + 1 = acc$, then $acc - bb = 1$; and squaring,

$$b^4 - 2ab^2c^2 + a^2c^4 = 1.$$

Hence, as in the foregoing, it will follow, that $\frac{(bb + acc)^2 - 1}{(2bc)^2} = a$.

Prop. 3. Let the fraction $\frac{b}{c}$ be such, that $\frac{bb - 1}{cc} = a$, i.e. $bb = acc + 1$; also let $\frac{d}{e}$ be another fraction, having the same property with $\frac{b}{c}$, i.e. such, that $dd = aee + 1$. Then, if from the fraction $\frac{d}{e}$, and the two others mentioned in prop. 1, viz. $\frac{b}{ac}$, and $\frac{c}{b}$, a new fraction be formed, in the same manner as the fraction $\frac{bb + acc}{2bc}$ was formed from $\frac{b}{c}$, and the same two $\frac{b}{ac}$ and $\frac{c}{b}$, which fraction will be $\frac{bd + ace}{cd + be}$; this new fraction shall have the same property with the other two $\frac{b}{c}$ and $\frac{d}{e}$, i.e. $\frac{(bd + ace)^2 - 1}{(cd + be)^2} = a$.

Prop. 4. The same things being supposed as in prop. 3, except that bb, instead of being equal to $acc + 1$, as there, is equal to $acc - 1$, or $bb + 1 = acc$; it will follow, by the like steps as in prop. 3, that $\frac{(bd + ace)^2 + 1}{(cd + be)^2} = a$.

Prop. 5. If likewise d^2 be equal to $aee - 1$, as well as $b^2 = acc - 1$, all other things remaining as in prop. 3, then shall $(bd + ace)^2 = a \times (cd + be)^2 + 1$, i.e. $\frac{(bd + ace)^2 - 1}{(cd + be)^2} = a$.

Prop. 6. But if $b^2 = acc + 1$, and $d^2 = aee - 1$, all other things remaining as in prop. 3. Then shall $(bd + ace)^2 + 1 = a \times (cd + be)^2$, i.e. $\frac{(bd + ace)^2 + 1}{(cd + be)^2} = a$.

Now, let a be any number proposed, and let the fraction $\frac{b}{c}$ be such, that either $\frac{bb - 1}{cc} = a$, or $\frac{bb + 1}{cc} = a$, and take the fractions $\frac{b}{ac}$ and $\frac{c}{b}$, before described; then the series of fractions converging to $\sqrt{a}$, will be as follows:

$\left.\frac{c}{b}, \frac{b}{ac}\right\}\frac{b}{c}$ = the first term of the series.

$\frac{bb + ace}{2bc} = \frac{d}{e}$ the 2d term.
$\frac{bd + acc}{cd + be} = \frac{f}{g}$ the 3d term.
$\frac{bf + acg}{cf + bg} = \frac{h}{k}$ the 4th term.
&c. in infinitum.

} Every term is formed from the preceding; and the two fractions $\frac{b}{ac}$ and $\frac{c}{b}$, in the same manner as the second from the first, and these fractions.

And from the foregoing propositions it follows,

1. That if $\frac{bb - 1}{cc} = a$, then every fraction of the series shall be such, that if

To find $\frac{b}{c}$ such as makes $bb - 1 = acc$, i. e. $acc + 1 = bb$, recourse must be had to Lord Brouncker's method in Dr. Wallis's Commercium Epistolicum. from the square of its numerator be taken a unit, the remainder, divided by the square of its denominator, shall be equal to a.

For, by prop. 1, the fraction $\frac{d}{e}$ shall be such; and by prop. 3, the next fraction $\frac{f}{g}$ shall likewise be such; and so all the following terms.

Example. Let $a = 2$; then the first fraction, i. e. that in the smallest numbers, $\frac{b}{c}$, that makes $\frac{bb-1}{cc} = 2$, is when $b = 3$, and $c = 2$; so that

$\left.\frac{c}{b}.\ \frac{b}{ac}\right\}\frac{b}{c}$ are $\left.\frac{2}{3}.\ \frac{3}{4}\right\}\frac{3}{2}$.

And the terms following the first $\frac{3}{2}$, are $\frac{17}{12}$. $\frac{99}{70}$. $\frac{577}{408}$. $\frac{3363}{2378}$. &c.

2. But if $\frac{bb+1}{cc} = a$, i. e. if the first fraction $\frac{b}{c}$ of the series have the square of its numerator a unit less than acc, the multiple of the square of its denominator by the number a; the 2d term shall have the square of its numerator a unit greater than the said multiple of the square of its denominator; and the 3d term shall have the said square a unit less, and so on alternately. For, by prop. 2, the second term $\frac{d}{e}$ shall be such, that $\frac{dd-1}{ee} = a$: and therefore, by prop. 4, the 3d term $\frac{f}{g}$ shall be such, that $\frac{ff+1}{cc} = a$. And by prop. 5, it follows, that the next term $\frac{h}{k}$ shall be such, that $\frac{hh-1}{kk} = a$; and so on alternately, by prop. 4 and 5.

Example. Let $a = 2$: then the first fraction $\frac{b}{c}$ that makes $\frac{bb+1}{cc} = 2$, is when $b = 1$, and $c = 1$. So that

$\left.\frac{c}{b}.\ \frac{ac}{b}\right\}\frac{b}{c}$ are $\left.\frac{1}{1}.\ \frac{1}{2}\right\}\frac{1}{1}$. And the following terms are $\frac{3}{2}$. $\frac{7}{5}$. $\frac{17}{12}$. $\frac{41}{29}$. $\frac{99}{70}$ &c.

But if a be 13, then the fractions will be $\frac{18}{5}$. $\frac{5}{18}$. $\frac{18}{65}$] $\frac{18}{5}$. $\frac{649}{180}$. $\frac{23382}{6485}$. &c.

3. But if the fraction $\frac{b}{c}$ be such, that $\frac{bb-1}{cc} = a$, and if the fractions $\frac{b}{ac}$, $\frac{c}{b}$ be taken, from which the series is to be formed, as has been described; then if the first fraction of the series be made not $\frac{b}{c}$, but some fraction $\frac{d}{e}$, such that $\frac{dd+1}{ee} = a$; then shall every term of the series be such as the fraction $\frac{d}{e}$, i. e. the square of the numerator being increased by a unit, and the sum divided by the square of the denominator, the quotient shall be equal to a. For since $bb = acc + 1$, and $dd = aee - 1$, by prop. 6 it follows, that the next term $\frac{f}{g}$ shall be such, that $\frac{ff+1}{gg} = a$; and so on for every term.

Example. Let $a = 2$, $\frac{b}{c} = \frac{3}{2}$; then will $\frac{b}{ac} = \frac{3}{4}$, and $\frac{c}{b} = \frac{2}{3}$, and let $\frac{d}{e} = \frac{1}{1}$; then $\left.\frac{c}{b}.\ \frac{b}{ac}\right\}\frac{d}{c}$ are $\left.\frac{2}{3}.\frac{3}{4}\right\}\frac{1}{1}$. And the other terms are $\frac{7}{5}$. $\frac{41}{29}$. $\frac{239}{169}$. $\frac{1393}{985}$. &c.

LVII. Observations on the Electricity of the Air, made at the Chateau de Maintenon, during June, July, and October, 1753; *being Part of a Letter from the Abbé Mazeas, F. R. S. to the Rev. S. Hales, D. D., F. R. S. Translated from the French by James Parsons, M. D., F. R. S.* p. 377.

June 14, M. Mazeas accompanied the Marechal de Noailles to his castle of Maintenon. At his arrival, he set up an apparatus, consisting of an iron wire 370 feet long, raised to 90 feet above the horizon. It came down from a very high room in the castle, where it was fastened to a silken cord 6 feet long, and was carried from thence to the steeple of the town; where it was likewise fastened to another silken cord of 8 feet long, and sheltered from rain; and a large key was suspended by the end of this wire, to receive the electrical fluid.

Observ. 1. From June 17, the time of beginning his experiments, the electricity of the air was sensibly felt every day, from sun-rise, to 7 or 8 in the evening: except in moist weather, when he could perceive no signs of electricity. In dry weather, the wire attracted minute bodies, at no greater distance than 3 or 4 lines. He constantly observed that, in weather void of storms, the electricity of a piece of sealing-wax of 2 inches long, was above twice as strong as that of the air. Hence it would seem that, in weather of equal driness, the electricity of the air is always equal.

Observ. 2. When he grasped the wire closely in his hand, the electricity ceased instantly, and did not recover till 3 or 4 minutes after; whereas, during a storm we could deprive the wire of its electricity only for a moment; for it immediately returned with the same vigour. Hence it appears that the common electricity of the air has but a slow motion.

Observ. 3. He endeavoured to increase the electricity of the wire, by the addition of a 2d, which communicated with an electrical magazine, composed of pieces of iron, tin plates, gilt paper, and such like, sustained by silken cords; and he observed, 1. That the electrical fluid did not even then act with any more strength on minute bodies presented to the wire. 2. That in depriving this magazine of its electricity, it seemed to return the more slowly the more considerable the magazine was; whereas the contrary happens during a storm. This slowness, with which the common electricity of the air is propagated, made him despair of finding means capable of rendering its motion sensible.

Obs. 4.—It did not appear that hurricanes and tempests increase the electricity of the air, when they are not accompanied with thunder: for during 3 days of a very violent continual wind in the month of July, he was obliged to put the dust within 4 or 5 lines of the conductor, before any sensible attraction could be perceived. The direction of the winds, whether east, west, north, or south, does not make any sensible alteration in the electricity of the air, except when they are moist. In the most dry nights of that summer, he could observe no signs of electricity in the air; but it returned in the morning, as before said, when the sun began to appear above the horizon, and vanished again in the evening, about half an hour after sun-set. The strongest common electricity of the atmosphere was perceived in the month of July, on a very dry day, the heavens being very clear, and the sun extremely hot. The distance of 10 or 12 lines was then sufficient for the approach of the dust to the conductor, in order to see the particles rise in a vertical direction, like the filings of iron on the application of a magnet.

Obs. 5.—June 27, at 2 afternoon, he perceived some stormy clouds rising above the horizon, and immediately went up to his apparatus; and having applied the dust to the key, it was attracted with a force which increased in proportion as the clouds reached the zenith. When they had come nearly over the wire, the dust was so impetuously repelled as to be entirely scattered from the paper. He drew considerable sparks from it, though there was neither thunder nor lightning. These sparks were of a very lively red colour when attracted with the finger: they were white and smaller when he used a wire hafted in a glass tube: they were bluish, and much extended, when attracted by spirit of wine in a silver spoon.

Obs. 6.—He applied a piece of resin to the conductor, but could draw no sparks from it: however, all who were present heard a noise like that of hairs when burnt. It was the same with sealing-wax, woollen-cloth, linen, &c. He then took a quicksilvered glass, and applied to the clean side a piece of wire of 6 inches long, while the other end was put to the conductor; by which he drew a multitude of small whitish sparks, which soon ceased, but were succeeded by a noise like that which happened on applying the resin to the conductor.

When he applied the end of the wire to the silvered surface of the glass, while the other end touched the conductor, the quicksilver affected him so strongly, that notwithstanding his being so much accustomed to suffer these electrical shocks, he was not able to bear this. Hence he concludes, that the best method of increasing the electrical power is to make it fall on some metalline surface, intimately connected with a surface that is an electric per se.

Obs. 7.—When the stormy clouds were in the zenith of the wire, the electricity was increased to so high a point, that the silken thread attracted light bodies at the distance of 7 or 8 inches. This cord was 6 feet long, and in the first foot the electricity was nearly as strong as in the wire, but from thence it diminished in the rest of the length. He substituted a glass tube for the silken cord, and observed the same phenomenon, with this difference, that the electrical fluid penetrated it with greater difficulty.

Obs. 8.—The stormy clouds before mentioned remained about 2 hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminish the electricity, except about the end, when the clouds began to be dissipated.

About 6 o'clock in the evening he was told that there were signs of a new storm in the air: he went up, and while he was preparing matters, a young man of the town, 35 years old, subject to an epilepsy, was among the spectators.

He drew sparks on the epileptic person, who was present, from the first thunder-clap. At first he bore them; but in 2 or 3 minutes perceiving his countenance change; and fearing that an accident should happen to him, M. Mazeas begged he would retire. He was no sooner returned home than his senses failed him, and he was seized with a most violent fit. His convulsions were taken off with spirit of hartshorn; but his reason did not return in an hour and a half. He went up and down stairs like one who walks in his sleep, without speaking or knowing any person, settling his papers, taking snuff, and offering chairs to all that came in. When he was spoken to, he pronounced inarticulate and unconnected words. When he recovered his reason, he fell into another fit. His friends said, that he was more affected with this distemper when it thundered than at any other time; and that if it happened that he then escaped, which it rarely did, his eyes, his countenance, and the confusion of his expressions, sufficiently demonstrated the weakness of his reason. The next day he learned from the man himself, that the fear of thunder was not the cause of his disease; but that however he found a fatal connexion between the phenomenon and that distemper. He added, that when the fit seized him, he perceived a vapour rising in his breast, with so much rapidity, that he lost all his senses before he could call for help.

LVIII. A Treatise on the Precession of the Equinoxes, and in general on the Motion of the Nodes, and the Alteration of the Inclination of the Orbit of a Planet to the Ecliptic. By M. De St. Jaques Silvabelle. Translated from the French M.S. by J. Bevis, M.D. p. 385.

If the earth were perfectly spherical, the action of the sun on all the parts which compose it, would not produce any effect to make it turn round its centre; because the moment which would be produced on one side, would be always counterbalanced by an equal moment on the opposite side of the centre.

It would be the same if the earth were a spheroid flatted at the poles, and the sun was always in the equator, or in the 90th degree of declination: but in every other degree of declination its action on the excess of matter about the equator has a tendency to make the equator approach towards the sun's place, or to diminish the angle of the sun's declination, by making the earth's axis to turn round its centre in the plane of the circle of the sun's declination.

The earth has then, at every instant, 2 motions of rotation; one about the axe of the equator, called also the earth's axe; and this is the diurnal motion, which is uniform; the other motion of rotation is performed about the axe of the circle of the sun's declination, which is a diameter of the equator; and this motion is produced by the action of the sun on the redundant matter about the equator, and is continually accelerated from the continual application of the solar action producing it.

The point E, fig. 9, pl. 9, which is the intersection of the circumference of the equator and the circumference of the circle of declination, has 2 motions, whose directions are perpendicular to each other. Let Ee be the space which it runs through in an instant, in the circumference of the equator, by the uniform diurnal motion, and let Eε be the space it runs through in the same instant, in the circumference of the circle of declination, by an accelerated motion, as has been explained. The point E, in virtue of these 2 motions Ee and Eε, will not circulate either in the circumference Ee QE′ Q′E of the equator, or in the circumference EεPE′P′E of the circle of declination; but, forming the rectangular parallelogram Eee′ε, the diagonal Ee′ will be the elementary arc of the circumference Ee′qE′, in which the point E will circulate, and the angle eEe′ will be equal to the angle Qcq, and equal to the angle P′c′p′, which the pole P′ runs through in an instant in the circumference P′p′QqPQ′P′, whose plane is perpendicular to the plane of the circle of declination; and when the lines Ee, Eε are known at every instant, P′p′ will also be known, since the angle eEe′ is = to the angle Qcq = to the angle P′cp′.

The instantaneous motion of the pole, which is P′p′, or Pp, may be resolved into two, PR and PM, perpendicular to each other, and both to the earth's axe. The former causes the pole P to move parallel to the ecliptic ♈ ♋ ♎ ♑, and alters the place of the solstice ♋, and consequently also that of the equinoctial points ♈ and ♎; the latter, which is according to PM, alters the inclination of the earth's axe to the ecliptic.

To have the motion of the pole parallel to the ecliptic, or, which is the same, the motion of the node ♈, or the precession, in the same time that the sun passes from the equinox ♈ to the solstice ♋, take the integral of the lines PR, supposing PR generally to express the instantaneous precession for any declination of the sun s.

And to have the alteration of the inclination in the same time that the sun is passing from ♈ to ♋, take the sum, or the integral of the lines PM, supposing

PM generally to express the instantaneous alteration of the inclination of the earth's axe to the ecliptic for any given declination of the sun.

The sum of the lines PR is always the same, and has the same sign, or the same direction, during every quarter of the sun's revolution, whether he moves from ♈ to ♋, or from ♋ to ♎, or from ♎ to ♑, or from ♑ to ♈; so that the precession answering to any one quarter of the sun's revolution about the earth, or to 3 months, being known, that multiplied by 4 will be the annual precession; by 8 will give it for 2 years; by 16 for 4 years, &c.

Likewise the sum of the lines PM is ever the same for every quarter of the solar revolution; but it has alternately a contrary sign; that is, a contrary direction. During the quarter from ♈ to ♋, the alteration of the inclination of the earth's axe to the ecliptic is positive, and the angle of the inclination increases; but during the succeeding quarter, or from ♋ to ♎, the alteration of the inclination is negative, and the angle of the inclination diminishes: and as the diminution from ♋ to ♎ is equal to the augmentation from ♈ to ♋, it follows, that at the end of the semi-revolution the inclination of the earth's axe to the plane of the ecliptic will become again the same, having undergone an oscillation, which is completed in a semirevolution. It is the same, when the sun passes from ♎ to ♈. The angle of the inclination increases from ♎ to ♑, and decreases from ♑ to ♈, where it becomes again the same as it was at ♎.

And hence the inclination of the earth's axe to the ecliptic may be considered as constant, though subject to this oscillation, and indeed to several others, they being all regular, and performed in regular periods. The earth's inclination to the ecliptic being constant, and the motion of the pole which produces the precession, being always parallel to the plane of the ecliptic, the earth's pole moves in a parallel to the ecliptic, about $23\frac{1}{2}$ degrees distant from the pole of the ecliptic, and the terrestrial axe describes a conical surface. To this motion of the terrestrial axe, or pole, is to be ascribed the apparent motion of the stars about the pole of the ecliptic.

But hitherto we have not considered, that to the precession, thus caused by the sun, we are to add likewise that produced by the moon; and it remains, that we examine into the motion of the earth's pole, caused by the action of the moon on the redundant matter about the earth's equator. Now all that has been said concerning the sun, is alike applicable to the moon, which we may put in the place of the sun; the moon's orbit in the place of the ecliptic; and the time of the moon's revolution round the earth in the place of the revolution of the sun round the earth: and we shall find the motion of the earth's pole parallel to the lunar orbit, which is always the same at every quarter of the time of the revolution of the moon round the earth, and the oscillation of the earth's axe to the plane of the lunar orbit, which is completed in each semirevolution of the moon round the earth.

But whereas the plane of the lunar orbit, which is always inclined to the plane of the ecliptic in an angle of about 5 degrees, never continues in a constant position, like the plane of the ecliptic, so that its pole describes a small circle parallel to the ecliptic, at the distance of about 5 degrees from its pole; it follows, that the precession, with respect to the lunar orbit, is not the same as with respect to the ecliptic; and that the motion of the pole parallel to the lunar orbit should be referred to the plane of the ecliptic: which is done by resolving the motion of the pole, parallel to the plane of the lunar orbit, into 2 motions, the one parallel to the plane of the ecliptic, and the other perpendicular to it, and in the plane of the solsticial colure.

The former of these 2 motions gives the precession with respect to the ecliptic, and has its direction always the same way. The latter motion has 2 opposite directions, in the 2 semirevolutions of the pole of the lunar orbit round the pole of the ecliptic, and causes an oscillation of the terrestrial axe on the plane of the ecliptic, which is completed in a revolution of the pole of the lunar orbit round the pole of the ecliptic.

From what has been said, it follows, that there are 5 distinct motions of the pole of the earth; namely, 2 of precession, which are parallel to the plane of the ecliptic, and 3 of oscillation on the plane of the ecliptic. The 2 of precession are caused, the one by the sun, the other by the moon. That which is caused by the sun is constantly the same, at every quarter of the time of the revolution of the sun round the earth; that is, every 3 months: that which is caused by the moon, is constantly the same at every quarter of the time of the revolution of the moon round the earth; that is, about every 7 days.

Of the 3 motions of oscillation, one is caused by the sun, and is completed in the time of the semirevolution of the sun round the earth, taken from one equinox to the following one; that is, in 6 months. Another is caused by the moon; and each oscillation is completed in the space of a semirevolution of the moon round the earth; that is, in 14 days. The third is caused likewise by the moon, and arises from the plane of her orbit being different from the plane of the ecliptic, and from the pole of the lunar orbit making its revolution about the pole of the ecliptic in about $18\frac{2}{3}$ years. And this oscillation is completed in the time of the revolution of the pole of the lunar orbit about the pole of the ecliptic; that is, in about $18\frac{2}{3}$ years.

It will appear in the memoir, that there is a relation purely geometrical between the quantity of the nutation, during the time of the semirevolution of the pole of the lunar orbit, and the quantity of the precession, caused likewise by the moon in the same time. This relation is quite independent of the force of the moon, of the quantity of the earth's flatness, of the quantity of the terrestrial matter, and indeed of every thing of a physical nature that can enter into the problem.

We are content to examine the motions of the pole of the earth produced by the sun and the moon. The same method, and the same formulæ, will give likewise the motions of the terrestrial pole arising from any other planet, as Saturn, Jupiter, &c. but these motions are too minute to merit attention.

Whatever has been said of the action of the sun on the redundant matter about the earth's equator, is also applicable to his action on a simple ring placed at the equator, without adhering to the terrestrial globe; and the motion of the pole of such ring may be determined by the same method, and consequently the motion of its nodes on the plane of the ecliptic, and the alteration of the inclination of its axe to the same plane. And since these motions are the same, whether the ring be supposed entire, or a small portion of it only be considered, or a mere point of it, the motions of the nodes, and the alteration of the inclination of a moon, or a satellite of a planet, may thereby be known. And the formulæ differ in nothing from those of the motion of the nodes of the earth's equator, and of the alteration of the obliquity of the earth's axe to the plane of the ecliptic, but in this, that the action of the sun on the ring to make it turn, is exerted entirely on it; whereas, in the problem of the precession, this force must necessarily be distributed throughout the whole mass of the earth, on account of the adherence of the ring to the globe of the earth.

As to the division of the work, this memoir is divided into 4 sections. The 1st section treats of the motion of the pole of the terrestrial equator caused by the sun. The 2d section treats of the motion of the pole of the terrestrial equator caused by the moon. The 3d section treats of the motion of the pole of a ring, or of the orbit of a moon, caused by the sun. The 4th section contains the application of the formulæ found in the other sections.

But as there are some inaccuracies in this very long and intricate treatise, and as its objects may be better answered by the first part of Mr. Thomas Simpson's Miscellaneous Tracts, on the same subject, printed in 1757, it is deemed unnecessary to reprint this treatise, which will not admit of abridgment.

LIX. On the Ages of Homer and Hesiod. By George Costard, M. A. in a Letter to the Earl of Macclesfield, F. R. S. p. 441.

It seems to be an opinion pretty generally received, that Homer and Hesiod lived much about the same time. What that age was, is indeed not at all agreed on among writers; the only thing in which they conspire being, as Mr. C. thinks, to place both of them much earlier than they ought to have done.

Among the ancients, Velleius Paterculus says, that Homer lived 950 years before his time. This author dedicates his history to the consul Vinicius, who is placed in the fasti consulares A. V. C. 782, which is A. D. 30. So that, according to this computation, Homer must have flourished about the year before Christ 920. And with this account agrees pretty nearly the Parian marble. Herodotus, according to our present copies of him, places Hesiod and Homer not more than 400 years before his time. Herodotus, according to A. Gellius, was 53 years old at the beginning of the Peloponnesian war, or the year before Christ 431. And if to this we add 400 years, we shall have the year before Christ 831; about which time consequently, according to him, both Homer and Hesiod must have flourished.

Among the moderns, Petavius places Hesiod A. P. J. 3714, or about the year before Christ 1000: and in his Rationarium Temporum, he says, that Hesiod was contemporary with him, and that this ex ARCTURI ORTU, quem poeta iste describit, eruditi artis illius colligunt; and in the margin refers to Longomontanus in his Astronomia Danica. With Petavius agrees very nearly Palmerius, as cited by Dr. Hyde in his notes on Ulug Beigh, though Sir Isaac Newton, whose authority with some persons is decisive, tells us, that from the achronical rising of the same star it follows, that Hesiod flourished about 100 years after the death of Solomon. This again he places, in his short chronicle, in the year before Christ 979; from which, if we subtract 100 years, we shall have the year before Christ 879, when, according to him, both Hesiod and Homer, if contemporaries, must have flourished. In what manner Sir Isaac Newton computed this, or whether indeed he ever computed it at all himself, is not, at least publicly, known. It is probable he only followed some one else; and therefore without derogating in the least from his authority, or thinking it a failure in respect to the memory of the greatest man that ever lived, I shall consider a little how far the age of these poets may be determined, with any certainty, from this achronical rising of Arcturus.

Longomontanus, in his Astron. Danic. supposes Hesiod to have flourished about the year before Christ 776, when he makes the place of Arcturus ♍ 12° 16′, the place of the sun's apogee ♉ 20° 10′, and his place, 60 days after the winter solstice ♓ 1° 10′. In the year after Christ 1610, he says the place of Arcturus was ♎ 18° 47′; so that from the year before Christ 776, to the year 1610, Arcturus had moved through $36° 31' = 131460''$; which divided by 2386 the number of years elapsed, gives the annual motion of the fixed stars 55″. But as he makes the annual motion of the fixed stars 49″ 45‴, or 1° in $72\frac{1}{3}$ years; 55″ will, according to him, require about 2658 years. So that Hesiod, according to his computation, must have lived about the year before Christ 1048; unless, as he seems to suspect, that poet describes the rising of Arcturus, not as it was in his own time, but 272 years before. So that from hence we see nothing certain can be concluded with regard to his age.

Kepler, in his Epitom. Astronom. supposes that from the time of Hesiod to the year after Christ 1618, are 2400 years, and that the annual motion of the fixed stars is 51″, which, in 2400 years, gives 34°. From which, and several other assumptions, he concludes, that in Hesiod's time Arcturus rose achronically March 3, in the Julian year reckoned backward, when the sun was in ♓ 5° 11′.

Riccioli, in his Almagest, supposes, that Hesiod flourished about the year before Christ 775, when the place of the sun's apogee was ♉ 20°; and therefore the sun's true motion for 60 days was 61° 10′, which added to the place of the winter solstice, or the beginning of ♑, gives the sun's place ♓ 1° 10′, the point opposite to that point of the ecliptic which rose along with Arcturus, or ♍ 1° 10′. Hence he computes the place of Arcturus to have been ♍ 12° 15′. But at the end of the year 1644, the place of Arcturus, he says, was ♎ 18° 19′; therefore from the time of Hesiod, before assumed, to the end of the year 1644, that star had moved through 36° 4′. But this it would do, he says, in 2597 years. From which therefore subtracting 1644, there remains the year before Christ 953. He concludes therefore, as Longomontanus suspected before, that Hesiod speaks of the achronical rising of this star, not as it was in his own time, but two centuries before. Besides, as the refraction of Arcturus would accelerate his rising, and the sun's refraction would retard his setting; and as the time of the solstice was then known, at best, but in a very gross manner; he is of opinion, that this method is not much to be depended on; contrary to what Scaliger and Vossius both thought.

As there are however several errors in this computation, it may not be amiss perhaps to form another, on supposition, with Sir Isaac Newton, that Hesiod flourished about the year before Christ 879, or in round numbers the year 880, and let us see what will be the result of it. At the end of the year 1689, the place of Arcturus, in the British catalogue, was ♎ 19° 53′ 52″, or 6^s 19° 53′ 52″: and from the year before Christ 880, to the end of the year 1689, are 2569 years, the precession for which time is 1^s 5° 40′ 50″: this, subtracted from the place of Arcturus 6^s 19° 53′ 52″, gives his place, in the year before Christ 880, = 5^s 14° 13′ 2″. The latitude of this star is, in the same catalogue, = 30° 57′. Hence is computed the declination of Arcturus = 34° 22′ 40″, and his right ascension 180° 37′ 10″.

Where this observation on Arcturus was made, is not said; we may suppose it to have been at Ascra, where Hesiod's father lived, as he tells us himself. But as the situation of this place is not very well known, we may, without any sensible error, take Athens, whose latitude is made, by the best modern geographers, 38° 5′ north. Hence is computed the oblique ascension 148° 12′.

In the year before Christ 880, the time of the winter solstice was December 29, at 15 minutes past 6 o'clock in the morning, according to the vulgar reckoning; or, in the astronomical account, $28^d\ 18^h\ 15^m$; and 60 days after this brings us to Feb. 27, when the sun's place was $11^s\ 0°\ 6'\ 23''$; his declination south 11° 27′ 18″; his right ascension 332° 11′ 56″; whence we have his ascensional difference = 9° 8′ 15″. Hence is computed the time of Arcturus's rising, viz. $5^h\ 42^m\ 47^s$. By this it appears, that at Athens, in the year before Christ 880, and 60 days after the winter tropic, the star Arcturus rose at $19^m\ 20^s$ after sun-setting.

But if we would inquire the time when it rose achronically, in the proper sense of the word, we shall find it to be that year March the 3d. But though this is what is properly meant by achronical rising; yet as a star at that time is invisible, and consequently can be no rule for husbandmen, for whose use these observations were intended; there is another achronical rising, called the apparent one: this is when a star first appears above the eastern horizon after sunset; which therefore requires some certain depression of the sun in the opposite part of the heavens, more or less according to the magnitude of the star required, to become visible.

It was said before, that in the year before Christ 880, Feb. 27, Arcturus rose at Athens $19^m\ 20^s$ after sun-set; but whether this, though a bright star of the first magnitude, could be seen there so soon in the eastern horizon as even at 30 min. past sun-set, may well be questioned: and therefore Feb. 27, or the 60th day after the winter solstice, could not be there esteemed the day of the apparent achronical rising of Arcturus.

I have hitherto, says Mr. C., called it the star Arcturus; but it is not improbable that Hesiod meant the whole constellation Bootes. He calls it indeed ΑΣΤΗΡ, and that word, according to Macrobius, signifies only a single star. But whatever it might do in his time, it seems evident, that among the ancients, and especially the poets, that distinction was not always nicely observed. If this therefore should be the case with respect to Hesiod, the time of this rising of Arcturus will be something more indeterminate, as a constellation cannot rise all at once, nor is it now known how many stars this constellation in particular was, in those early times, supposed to consist of.

But further; it has been hitherto taken for granted, that Hesiod is to be understood as speaking of Ascra, or some place in the neighbourhood of it; but this also is uncertain: for it was no unusual thing with the ancients to set down in calendars, of this sort, observations on the risings and settings of the stars made in very distant times and countries; the latitudes of places being unattended to, and the slow motion of the fixed stars about the poles of the ecliptic unknown, and indeed unsuspected, or disregarded afterwards, when it became suspected. But though we should grant the place of observation to have been at, or near Ascra, yet there will still remain a difficulty with respect to the time. In the computation before given, it has been supposed, that Arcturus rose there achronically on the 60th day from the solstice, exclusive of the solstitial day itself; but as the particle μετὰ is sometimes taken inclusively, we may reckon the day of the solstice itself one of the number, which, consequently, will bring us only to Feb. 26. Besides, what has been said, has been built on the supposition that the day of the solstice was then precisely known; a thing however not hastily to be granted. The inaccuracy of observations, and the want of proper instruments, in times much later than this we are here speaking of, would incline us not to attribute too much to them, in a case of so much nicety. Since then we find the solstice fell out so early in the morning; either December the 28th or 29th might have been taken for the solstitial day; and accordingly 60 days after will be either February the 26th or 27th. But as the sun's change of declination, at that season of the year, is very slow; an error of a day or two, or more, either forward or backward, (a thing by no means impossible) will bring us to February 25 or 28, which is a difference of no less than 4 days.

But if such mistakes could be committed at this time, how little must we suppose the true time of the solstice known, so early as the year before Christ 880. Not however to assume too much, let us suppose a mistake of 2 days only, in the rising of Arcturus. By calculating as before, we shall find that A. C. 1689, the point of the ecliptic rising along with Arcturus, in the latitude of Athens, was ♎ $10^{\circ}\ 35'\ 55''$, the point opposite to which is ♈ $10^{\circ}\ 35'\ 55''$. But this point the sun entered that year March 20, when consequently Arcturus rose there achronically: but in the year before Christ 880, as before observed, Arcturus might be said to rise achronically there March 2: this gives a difference of 18 days in 2569 years; whence a difference of 2 days will give 285 years, which subtracted from the year before Christ 880, will give the year before Christ 595, for the time of Hesiod, and consequently of Homer too, if contemporary with him, for any thing that can be gathered to the contrary from the achronical rising of Arcturus.

Having now shown, in this manner, what little precision there is in this argument, I might, as I at first intended, take my leave of the subject, and refer the settling the age of these two poets to authorities of another nature. But as the favourers of their high antiquity may be startled to hear that their age may be brought down so low as the year before Christ 595, I shall add something in confirmation of this date to show that it is not so unreasonable as at first sight it may appear.

[Mr. C. then quotes a number of passages from the ancient poets, alluding to astronomical observations, which he thinks renders the last-mentioned date not improbable.]

As a further confirmation that we are not very wrong in placing the age of these two poets as we have done, it may be remarked, that in the description given by Hesiod of lucky and unlucky days, he tells us, τριηκάδα μηνὸς ἀρίστην. But the first person, among the Greeks, that called the last day of the month by that name, or that used the word ΤΡΟΠΑΙ, if we believe Laertius, was Thales. Neither Homer nor Hesiod therefore, if this observation be true, can be older than Olymp. 35, 1, or the year before Christ 637, when that philosopher was born. But as it must have been some time before he could apply himself to astronomical studies, and probably not till the middle part of his life, or about the year before Christ 600, the Odyssey could not well have been composed before.

But Pisistratus, as we are informed by Tully, first collected Homer's verses, and digested them in the manner we now have them. And Solon, according to Laertius, proved the right of the Athenians to the island Salamis, from these lines of the Iliad:

Αἴας δ' ἐκ Σαλαμῖνος ἄγεν δυοκαίδεκα νῆας,
Στῆσε δ' ἄγων, ἵν' Ἀθηναίων ἵσταντο φάλαγγες.

Solon, according to Laertius, flourished about Olymp. 46, and in the 3d year of it was archon, and published his laws. This was the year before Christ 590. What his age was at that time, he does not tell us, but that he was 80 at his death; which by Plutarch, in his life of that lawgiver, is placed Olymp. liii. 3, or the year before Christ 562. If so, he must have been about 52 the year that he was archon. And that he could not have been very young then, is plain, from the post and credit he was in. On the expiration of his archonship, as we are informed by Plutarch, he travelled for 10 years, and returned an old man, as indeed he was, being now about 62 years of age: this was the year before Christ 580. During this interval, it is highly probable, he had his interview with Crœsus, and brought back with him to Athens, Homer's poems, which he might meet with at Smyrna, or some other of the Ionian cities. On his return, he found his country torn with factions, and that Pisistratus had formed the design of making himself master of the state, which he soon afterwards effected. What year this was in is uncertain. The Oxford marble places it, as does Plutarch, in the archonship of Comeas, which is supposed to concur with Olymp. liv. 4, or the year before Christ 557. But Tatian, Clemens Alexandrinus, and Scaliger, among the moderns, fix the government of Pisistratus to Olymp. l. or 577 years before Christ. And this indeed agrees best with Plutarch; who says that Pisistratus, after seizing the administration, 'honoured and esteemed Solon, and often sent for him, and advised with him.'

In what year Pisistratus digested Homer's poems, is not said; but it was probably some time while he was in credit; and therefore it is likely about this very year 557 before Christ.

On the whole, says Mr. C. I think it may be concluded, with a good degree of probability, from what has been here laid down, that the Iliad and Odyssey were both composed about the time of Cyrus, or the year before Christ 558, if, as the ancients generally do, we make his reign to commence from his taking of Babylon. And since those that make Hesiod the elder of the two poets, place him but a few years earlier than Homer, not enough however to cause any observable change in the rising of the fixed stars; we may take the difference, at a medium, at 20 or 22 years; which will bring us to the year before Christ 580, for the time when Hesiod flourished.

The only difficulty that I think can be made to this, is, how to reconcile it with the express testimony of Herodotus to the contrary. In his life of Homer, he places him 622 years before the expedition of Xerxes into Europe; but in his history he says, both Homer and Hesiod were not more than 400 years before his time; that is, since there were but 50 years between the Peloponnesian war and the battle at Salamis, little more than 450 years before the same expedition. Scaliger, in his notes on Eusebius, corrects the former passage of Herodotus by the latter; and, instead of ἑξακόσια, reads τέτρακόσια; which will place Homer about the year before Christ 902, consistent enough with Paterculus and the marble, but different from his history by 71 years. Whether this correction of Scaliger's be right, or not, I shall not here stand to inquire; but I am apt to think the word τετρακοσίοισι itself, in Herodotus, is corrupt.

The Greek chronology, like that of other nations, has been generally carried up too high; the natural consequence of ignorance, and a defect of memoirs. This is only now to be corrected by persons of learning and abilities, capable of examining and comparing things with each other.

LX. An Additional Remark to one of Mr. W. Watson, F.R.S. in his Account of the Abbe Nollet's Letter on Electricity. By T. Birch, D.D. Sec. R.S.*

Mr. Watson, in a note upon his account of the ninth letter of the Abbé Nollet concerning electricity, read before this Society on the 17th of May 1753,

* Dr. Tho. Birch was born at London in 1705. His parents were quakers, and they intended him for trade; but the love of learning prevailed, in which he was permitted to pursue his inclination, on condition that he should provide for himself. Hence he became usher successively in three schools kept by quakers. Having quitted the Society of the Friends, however, in 1730 he was ordained deacon, and the next year priest; about which time he obtained a living in Gloucestershire, and afterwards that of Ulting in Essex. In 1734 he became domestic chaplain to Lord Kilmarnock, who was executed in 1746 for his share in the rebellion. In 1735 Dr. Birch was elected F.R.S. and F.A.S.

takes notice, that as the electrical attraction has been observed so early, as to be mentioned by Theophrastus; so its luminous appearance, though only considered as a meteor, is mentioned by Plutarch in his life of Lysander, Pliny, and other ancient as well as some modern authors. Seneca particularly affirms, that Gylyppo Syracusas petenti visa est stella super ipsam lancem constitisse: and that in Romanorum castris visa sunt ardere pila, ignibus scilicet in illa delapsis. Cæsar, in his history of the African war, says, in a violent stormy night, Legionis pilorum cacumina sua sponte arserunt: and Livy mentions two similar facts. To these may now be added one from Mr. Fynes Moryson, who in his Itinerary, observes, that at the siege of Kingsale by the lord deputy Montjoy, where Mr. Moryson attended him in the camp, on the 23d of December 1601, all the night was clear, with lightning, as in the former nights were great lightnings with thunder, to the astonishment of many, in respect of the season of the year, 'that this night our horsemen set to watch, to their seeming, did see lamps burn at the points of their staves, or spears, in the midst of these lightning flashes.'

LXI. Extract of a Letter of the Rev. Joseph Spence, Prof. of Modern History in the University of Oxford, to Dr. Mead, F.R.S. Dated Byfleet near Weybridge, Surrey, Decemb. 7, 1753. p. 486.

I have lately received a letter from Signor Paderni at Portici; in which, speak-

and at the same time obtained the degree of M.A. from Aberdeen. In 1744 he was presented to the rectory of St. Michael, Wood-street; and about 2 years after, to that of St. Margaret Patteus. In 1752 he was elected Secretary to the R.S.; and the next year had the degree of D.D. conferred on him by the Archbishop of Canterbury. In 1761 he was presented to the rectory of Depden in Essex. But in 1766 he died suddenly by a fall from his horse.

Dr. Birch was moderately learned, and of rather slow parts; but he was an exceedingly industrious and indefatigable compiler, and a very useful secretary to the R.S. He had a considerable share in compiling the General Dictionary, historical and critical, 10 vols. folio: and he published the lives of Mr. Boyle, Archbishop Tillotson, Henry Prince of Wales, and other works of a similar kind. He also wrote an Inquiry into the Share which Charles the 1st had in the Transactions of the earl of Glamorgan, 8vo, 1747. But by far his most useful work, was the History of the Royal Society, in 4 vols. 4to, 1756, &c. This work he was enabled to compile by his situation of Secretary to the R.S., which gave him access to the archives of that learned body, whence he extracted and published what may be deemed the real Transactions of the Society, being the minutes and records from the books kept by the committee, by whom all the real business of the Society is, or ought to be, conducted. Thus we have in this work a curious and useful detail of the Society's concerns, from the beginning of the institution to the end of the year 1687; and probably would have been continued to his own time had his life been longer spared. And it would be a most useful and meritorious service, if that curious work were continued to the present times, by some other secretary of the Society; for no person can execute that task, but such as can have access to the minute books of the committee.

Dr. B. bequeathed his books, MSS., and a legacy of £500 to the British Museum, the money to go towards increasing the stipend of the two assistant librarians.

ing of the publication of the antiquities found at Herculaneum, he says, Spero che il primo tomo non tarderà molto tempo ad uscire; and then mentions some particular things that had been lately discovered among the ruins; a little brass bust of some unknown philosopher, of the manner excellent, and is perfectly well preserved: a statue of an orator, in marble; and another brass bust, on a term, of a youth, with very beautiful hair, and the whole excellent. The artist has put his name to the latter, ΑΠΟΛΛΩΝΙΟΣ ΑΡΧΙΟΥ ΑΘΗΝΑΙΟΣ ΕΠΟΙΗΣΕ. He says, that the workmen were then just entering on some nobleman's house, as appeared by the rich Mosaic pavements, &c. and that they were in hopes it would prove a very good new mine.

LXII. On the Value of an Annuity for Life, and the Probability of Survivorship. By Mr. James Dodson. p. 487.

The writers on the subject of annuities on lives have justly distinguished them into 2 kinds: in the first, the annuitant is entitled to receive a payment, if he be alive on the day on which it becomes due; but if he dies on the preceding day, or sooner, his heirs have no claim to any part of the payment, so to have become due; but in the second, if the annuitant dies at any intermediate time between the days of payment, his heirs are to receive a part of the annuity, proportional to the time elapsed between the preceding day of payment and the annuitant's decease. This latter kind of annuities have been distinguished from the former, by the words, secured by a grant of lands; because, where lands are leased for lives, the conditions are generally such as are above described.

The values of the first kind of annuities have been investigated on principles purely arithmetical; but in order to perform the latter, fluxions have been used, Mr. D. conceives, without any necessity: but as the investigation of the former may be usefully made a part of the latter, he first recites the method of performing that, and then proceeds to attempt the other on the same principles.

If, with De Moivre, we suppose the decrements of life to be equal (viz. that out of a number of persons, alive at a given age, equal to the number of years that a person of that age has a possibility of living, there will die one in each year, till they are all extinct); then, out of a number of chances equal to that number of persons, which may, for instance, be 36, all but one are favourable, in the first year, to any individual; and consequently it is 35 to 1 that he receives one payment of the annuity, by living till it becomes due; that is, the probability of his receiving it, is $\frac{35}{36}$, and of not receiving it, $\frac{1}{36}$.

Again; since, by supposition, there dies but 1 person in the first year, and 1 in the 2d; there are but 2 chances, in the 36, against his receiving the 2d payment, by living till it becomes due; and consequently $\frac{34}{36}$ will be the probability of his receiving that also; the probability of his dying in that year being $\frac{1}{36}$, as before. Thus too it may be proved, that the probability of his receiving the 3d, 4th, 5th, &c. payment, will be $\frac{33}{36}$, $\frac{32}{36}$, $\frac{31}{36}$, &c. and therefore, if the annual payments were each 1l. and if the interest of money was not to be considered, we might conceive these several probabilities, as the present worths of the several payments, and the sum of them would be the value of an annuity of the first kind.

But since the interest of money necessarily enters the process, and since the payments become due at the end of the 1st, 2d, 3d, &c. year, the first of these payments must be discounted for 1 year, the 2d for 2 years, the 3d for 3 years, &c. and the sum of their present worths will be the value of an annuity of the first kind, to continue during the life of a person who may possibly live 36 years; and this sum may be found by an easy and well-known process, from the common tables of compound interest and annuities, which need not be inserted here.

The annuity secured by land must necessarily be of greater value than the above; because, though the annuitant dies before the payment becomes due, yet his heirs are to receive a part of it; the annuitant therefore, in this case, has not only the probability $\frac{35}{36}$ of receiving the first payment, but he has also an expectation on part of the probability $\frac{1}{36}$, which in the first case was wholly against him. Now it may be esteemed an equal chance, supposing him to die in the first year, whether that decease happens before the expiration of half that year, or after it; and if it happens before, he is to receive less than half the annual payment; but if after it, more.

The annuitant may therefore be supposed to have an equal chance, if he fails of receiving the whole first payment, yet of receiving half of it; and consequently half of the probability, $\frac{1}{36}$, which was before totally against him, will in this case be favourable to him; and his expectation of receiving either the whole, or at least half of the first payment, will be $\frac{35}{36} + \frac{1}{72}$. So since the probability of his dying in the 2d year, is also $\frac{1}{36}$; we may in the same manner prove, that one-half of it will in this case become favourable to him; and consequently that $\frac{34}{36} + \frac{1}{72}$ will be the probability of his receiving the whole, or at least half, of the 2d payment. It appears therefore that for every year which he has the possibility of living, he will in this case have the probability $\frac{1}{72}$, or half of $\frac{1}{36}$, in his favour, more than he had in the former case; and therefore, if the present worths of the constant sum $\frac{1}{72}$l. (supposed to be due at the end of 1, 2, 3, &c. years) be found, and added to the value of the annuity, according to the former case, the sum will be the value of an annuity, secured by land, to continue during the life of a person who may possibly live 36 years.

Now since the sums by which the former annuity is to be increased, consist of the present worths of that fraction of a pound sterling, whose numerator is unity, and denominator twice the number of years that the annuitant can possibly live, supposed to be due at the end of each of 1, 2, 3, &c. years; it will follow that their amount, or the difference between the values of the 2 annuities, will be equal to the quotient found by dividing the value of an anuity of 1l. certain, for as many years as the annuitant can possibly live, by twice that number of years: and therefore, if to the value of an annuity for life, of the first kind, we add the quotient so found, the sum will be the value of an annuity of the second kind, for the same life.

When Mr. D. had thus investigated the value of this annuity, he compared the result with that M. de Moivre had deduced from fluxions, and published in N° 473 of the Phil. Trans.; and found that they agree to more than a sufficient exactness, for computations of this nature. He then annexes this comparison.

The probability of any order of survivorship, that can happen among 3 persons, and consequently that of one person's surviving 2 others, may likewise be investigated on similar principles, without the assistance of fluxions; but as this problem admits of 6 cases, and the algebraic process is of a length too great for the designed limits of this essay, Mr. D. omits it.

LXIII. On the Pheasant of Pennsylvania, and the Otis Minor. By Mr. George Edwards.* † p. 499.

What is called the pheasant in Pennsylvania, and other provinces of North America, belongs to that genus of birds, which in England we call heathcocks,

* This bird is the *Tetrao Umbellus* of Linneus, and is extremely well figured in the 1st volume of Edwards's Gleanings of Natural History, pl. 248.

† From the Memoirs of his Life, published in 1776, it appears that Mr. George Edwards was born at Stratford, a hamlet belonging to Westham in Essex, on the 3d of April 1694. He passed some of his early years under the tuition of a clergyman named Hewit, who was then master of a public school at Layton-Stone, a few miles distant from the village where he was born. After quitting the school, he was placed with another minister of the established church at Brentwood; and being designed by his parents for business, was put apprentice to a tradesman in Fenchurch-street.

On the expiration however of his apprenticeship, he declined entering into business, and conceived a design of travelling, in order to improve his taste, and enlarge his mind. He first visited Holland, and afterwards Norway, and having gratified his curiosity with the view of these regions, returned, and passed some time in his native place. He then went to France, where, on visiting Versailles, he experienced great disappointment at finding that the Menagerie, once so celebrated, had at that time no living creature in it, having been totally neglected since the death of Louis the 14th. On his return to England Mr. Edwards closely pursued his favourite study of Natural History, employing himself in making drawings of such animals as happened to fall under his notice; and the accuracy and elegance of his delineations was such as to make them highly interesting to those who cultivated similar pursuits. He therefore was induced to turn to advantage what was begun only for amusement, and obtained a sufficient subsistence by the sale of his drawings. In December 1733, by the recommendation of Sir Hans Sloane, he was chosen librarian of the College of Physicians; an office peculiarly suited to his taste, as it gave him constant access to many works on the subject of Natural History, which he might otherwise have found a difficulty of inspecting. In 1743 the first volume moor-game, or grouse. It is nearly as large as a pheasant, is of a brownish colour on the head and upper side, and white on the breast and belly; it is beautifully variegated with lighter and darker colours on the back, and spots of black on the under side. Its legs are feathered down to the feet. This bird is wholly unknown to the curious of our country. It was sent to England, a year or two ago, by Mr. John Bartram, with a letter giving some account of it, and other matters; out of which letter is extracted what follows: 'our pheasant was wholly unknown to Catesby, it being more northern than Carolina. They have been common in Pennsylvania, but now most of them are destroyed in the lower settlements, though the back Indian inhabitants bring them to market. When living, they erect their tails like turkey-cocks, and raise a ring of feathers round their necks, and walk very stately, making a noise a little like a turkey, when the hunter should fire. They thump in a very remarkable manner, by clapping their wings against their sides, as is supposed, standing on a fallen tree. They begin their strokes at about 2 seconds of time distant from each other, and repeat them quicker and quicker, till they sound like thunder at a distance, which lasts about a minute, then ceases for 6 or 8 minutes, and begins again. They may be heard near half a mile, by which the hunters find them. They exercise their thumping in a morning and evening in the spring and fall of the year. Their food is berries and seeds. Their flesh is white, and good. I believe they breed but once a year in the spring, and hatch 12 or 14 at a sitting; and these

of his History of Birds was published; the reception of which was so highly favourable as to induce him to continue it in a similar manner, till in 1751 the fourth volume came from the press, accompanied by a dedication to the Creator of the universe, in devout gratitude for all the good things he received in this world.

In 1758 he continued his labours under a new title, viz. Gleanings of Natural History, containing, as before, various kinds of rare birds, quadrupeds, and other animals: a 2d volume appeared in 1760, and the 3d and last in 1764. The whole work therefore consists of seven volumes in 4to, containing engravings and descriptions of no less than 600 subjects in Natural History. To the work, thus completed, Linneus added a list of his own trivial names. Linneus indeed appears to have entertained a very high esteem for Mr. Edwards, and to have considered his work as of the highest importance in Natural History, and, in the Systema Naturæ, publicly declares its superiority over other productions of a similar kind.

By the time Mr. Edwards had finished his Natural History, the decay of his sight, together with other infirmities of age, induced him to resign all further employment, and to retire to a small house which he purchased at Plaistow, Essex, where he continued to pass the remainder of his life, and died on the 23d of July, 1773, having completed the 80th year of his age.

Mr. Edwards, we are informed, was of middle stature, rather inclined to corpulence; of a liberal disposition and a cheerful conversation. All his acquaintance experienced his benevolent temper, and his poor neighbours frequently partook of his bounty.

In consequence of the merits of his publications in Natural History, Mr. Edwards was elected a F.R.S. He was also a F.A.S., as well as a member of several Academies in different parts of Europe.

keep together till the following spring. They cannot be made tame. Many have, to their disappointment, attempted it by raising them under hens; but, as soon as hatched, they escaped into the woods, where they either provide for themselves, or perish.'

Mr. Brooke, surgeon of Maryland, says, 'They breed in all parts of Maryland, except near the eastern shores. They lay their eggs in nests made of dry leaves by the side of a fallen tree, or at the root of a standing one; they lay from 12 to 16 eggs, and hatch in the spring. I have found their nests, when I was a boy, and have endeavoured to take the old one, but never could: she would let me put my hand almost on her before she quitted her nest; then she would flutter just before me for 100 yards, or more, to draw me off from her nest, which could not afterwards be easily found. The young ones leave the nest as soon as hatched, and, I believe, live at first on ants and worms; when they are a few days old, they hide themselves among the leaves, that it is hard to find them. When they are grown up, they feed on the berries, fruits, and grain, of the country. Though the pheasant hatches many young at a sitting, and often sits twice a year, the great number and variety of hawks among us, feeding on them, prevents their increasing fast. The beating of the pheasant, as we term it, is a noise chiefly made in the spring by the cock birds. It may be distinctly heard a mile in calm weather. They swell their breasts like a pouting pigeon, and beat with their wings, which sounds not unlike a drum. They shorten each sound in a stroke, till they run into one another undistinguished.'

Lahontan, in his voyage to North America, vol. i, p. 67, speaking of the fowls about the lakes of Canada, mentions this same pheasant as follows: "Their flapping makes a noise like a drum, all about, for the space of a minute; then the noise ceases for half a quarter of an hour, after which it begins again. By this noise we were directed to the place, where the unfortunate moor-hens sat, and found them on rotten mossy trees. By flapping one wing against the other, they mean to call their mates, and the humming noise, thus made, may be heard a quarter of a league off. This they do only in the months of April, May, September, and October; and, which is very remarkable, a moor-hen never flaps in this manner but on one tree. It begins at break of day, and gives over at 9 in the morning, beginning again an hour before sun-set, and flaps its wings till night." This is all the light I could gather, relating to the pheasant of North America.

The otis minor,* anas campestris, canne petiere, or the field-duck, was taken in the west of England, and laid before the Royal Society about 3 years ago: and as no gentleman present knew the bird, Mr. Hauksbee sent it to Mr. E.

* This bird is the *otis tetrax* of Linneus, and is figured in the first volume of Edwards's Gleanings, pl. 251.

who accordingly gave in what account of it he could collect from ornithologists, having never seen the bird till then. He found figures of it in Bellon, Gesner, Aldrovand, Willoughby, Johnson, and others, with descriptions under the various names here given. Modern authors agree that this bird was unknown to the ancients.

LXIV. On a Particular Species of Coralline. By Mr. J. Ellis, F.R.S. p. 504.

Among the observations lately made on the marine productions, Mr. E. finds that many corallines, as well as corals, are composed of a great number of tubes, which proceed from animals; and as these tubes are made of different materials in different species, so are they disposed in a variety of different forms. Some are united compactly together, as in the red coral, see pl. 10, letter A; and in some species of the white, as at letter B; in both of which they appear combined together, forming irregular ramifications, like trees: others rise in tufts, like groupes of the tubular stalks of plants, distinct from each other. Two sorts of these the fishermen frequently take up at sea in their nets, particularly near the buoy of the Nore, at the opening of the river Thames; when these are first taken out of the sea, and immediately put into a basin of sea-water, you may observe, that each tube has its proper polype sitting on it, of a most beautiful crimson colour. Letter D,* gives the figure of the largest kind, called (in Ray's Synopsis, ed. 3, p. 31) adianti aurei minimi facie planta marina; and letter C is a smaller kind, called (in Ray's Synopsis, ed. 3, p. 39) fucus dealensis fistulosus laringæ similis. He calls this species corallina tubularia melitensis, cum scolopendris suis, tentaculis duobus duplicato-pinnatis, instructis.†

On taking the tubes and animals of this curious Maltese coralline out of the spirits of wine, where they had been preserved, he perceived a small slimy bag, in which they seemed to be inserted, and to take their rise from, as may be observed at letter d. What has been the use of this bag is uncertain, unless it was the matrix of several of these scolopendras in their embryo state. The tubes, which are built by the inclosed animals, as they rise in height, gently increase in diameter; the texture of their outside coat is formed of an ash-coloured earthy matter, of different shades in different strata, and closely united to an inner coat, which is of a tough, horny, transparent, and very smooth substance; the cavity, or inside, of the tube, is perfectly round, though the animal is of a long compressed figure, like a leech extended It appears, from the marks of its feet on

* The species referred to at letter D is the *tubularia indivisa* of Linneus. That referred to at letter C is the *tubularia muscoides*, Linn.

† This is the *sabella penicillus* of Linneus. It is however more properly referred to the genus amphitrite, and is the *amphitrite ventilabrum* of the Gmelinian edition of the Systema Naturæ.

the inside of the tube, that it can turn itself freely about, and move up and down, the better to attack and secure its prey.

This scolopendra has two very curious and remarkable tentacula, or arms, the left much larger than the right; these are doubly feathered, as may be seen, in the magnified part, at c; the number of feet on each side of the body of this animal exceeds 150.

Plate 10, will best explain the rest; where b is the belly part of the animal, in its natural size, hanging out of its tube. I is the same side of the whole animal a little magnified. a is the back part of the head of the animal, sitting in its tube. H is the back part of the whole animal a little magnified. e shows the inside of the tube with the strata, or rings, seen through the horny inner coat.

The coralline called (in Ray's Synops. ed. 2, p. 2, and ed. 3, p. 36, N° 15) fruticulus marinus, cauliculis crassiusculis teretibus rigidis, pennatus,* which I have named the herring-bone coralline, and which is very common on oysters all the winter season, shows remarkably, by the help of a common magnifying glass, the tubulary structure, not only of some of the corals and corallines, but of the keratophytons, or sea feather; only with this difference, that the tubes of the herring-bone coralline are of a spongy elastic nature, and always remain open; whereas the others, being of a more soft and viscid nature, by time, and the heat of the climate, are compressed together, and harden, some into stone, and some into horn or wood.

At E is the natural appearance of the herring-bone coralline; and F and G the root, and one of the upper branches, are magnified, to show the tubes.

LXV. Observations on the late severe Cold Weather. By William Arderon, F.R.S. p. 507.

The observations were taken by thermometers exposed to the open air, in the garden, which varied sometimes 40 or 50 degrees in 24 hours; the cold coming as it were by fits, in an unusual manner.

Dec. 30, at 11 o'clock at night.—All the spirits in Hauksbee's thermometer retired into the ball, and Fahrenheit's stood at 20 degrees: at this time he let down a Fahrenheit's thermometer into the river, to the depth of 4 feet, during 12 minutes, and when taken up it stood at 33 degrees.

This same evening he exposed an open glass jar full of water, in the garden, to be frozen; and in the morning it was all solid ice, rising in the middle, in figure like the frustum of a cone. He exposed also, in the same place, an open glass of ale, which froze even to the bottom, in a very odd manner; for the

* *Sertularia halecina.* Linn.

watery or weaker parts were frozen into plates of ice, sticking to each other by their edges, the more spirituous parts remaining between them, in their interstices, unfrozen; which being drained off into another glass, the taste was almost as strong as brandy, with a high flavour of the hop.

Dec. 31. This evening the cold was the most intense observed this season; for at 10 o'clock Fahrenheit's thermometer stood at 15 degrees.

Jan. 1. This afternoon it began to thaw, and in the night froze again, by which, next morning, the buildings in general appeared as if they had been white-washed on the outside, being cased all over with ice; and the insides of garrets and outhouses were covered in the same manner.

Jan. 31. He exposed a glass of proof spirits, impregnated with the essence or oil extracted from the peel of oranges, at 10 in the evening, in the garden, when Hauksbee's thermometer stood at 93°; at 8 next morning, he found it no way affected by the frost; nor did there seem any difference either in taste or smell. Feb. 6. At 8 o'clock, he exposed in the garden a drinking glass of water, which was completely frozen over in one minute's time; and in 15 minutes the ice was above $\frac{1}{10}$ of an inch in thickness. Fahrenheit's thermometer then stood at 21 degrees. A coarse grey thread, 2 feet in length, being dipped in water, froze in 4 seconds, so stiff, that he took it by one end, and held it upright, as if it had been a piece of wire.

If any part of the human skin, the finger, for instance, was wet with spittle, and immediately pressed on a piece of iron, in the open air, it would be frozen so fast, as to stick to it; and, if plucked away hastily, would endanger the tearing off the skin from the flesh. He tried the same experiment on lead, but the sticking was much less; and to wood the finger did not stick at all. In some places the ice was $\frac{1}{8}$ of an inch thick, for several days together, within-side of the windows, and that even in rooms where fire was kept; and when the weather became warmer, it did not fall in drops, but vanished imperceptibly into the air, by which it had been brought thither. These plates or cases of ice were sometimes an assemblage of an infinite number of particles not much unlike the scales of fishes; sometimes they resembled small spines, or the crystal shootings of various kinds of salts; and sometimes they represented a variety of landscapes with trees and plants, from 1 to 3 or 4 inches in length, in so beautiful a manner, as neither pen nor pencil can express.

LXVI. A Letter from M. de L'Isle, of the Royal Academy of Sciences at Paris, to the Rev. James Bradley, D.D. Dated Paris, Nov. 30, 1752. *Translated from the French.* p. 512.

This letter contains a comparison of Dr. Bradley's observations of the planet Mars, with some corresponding observations made at the Cape of Good Hope by Mons. de la Caille; from which he concludes, that the horizontal parallax of that planet, at the time of its opposition to the sun, is $27\frac{1}{4}''$.

And according to the ratio of the distance of the sun and Mars from the earth at that time, he concluded the horizontal parallax of the sun to be about $10\frac{1}{3}''$. This is what he had been able to conclude from Dr. B.'s observations of Mars, with respect to the parallax of the sun. Having made the same calculations from his own observations, and those of other astronomers, which he could collect; he found very nearly the same parallax of the sun, by taking a medium among all the observations of each astronomer.

LXVII. Description of a Piece of Mechanism contrived by James Ferguson, for exhibiting the Time, Duration, and Quantity, of Solar Eclipses, in all Places of the Earth. p. 520.

This machine may be seen described in Mr. Ferguson's Astronomy, art. 405, where he calls the machine an eclipsareon, the figure of which is exhibited in pl. 13 of that book.

LXVIII. On the late Hard Weather. By the Rev. H. Miles, D. D., F. R. S. p. 525.

This paper contains a few remarks on the very cold weather in Feb, 1754. The coldest day was on the 6th, about 7 in the morning, when the thermometer was at 15°. He observes that the time of the greatest cold, is usually from an hour to an hour and a half before sun-rise.

LXIX. A Catalogue of the 50 Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1753, pursuant to the Direction of Sir Hans Sloane. p. 528.

This is the 32d presentation of this kind, completing to the number of 1600 different plants.

LXX. An Account of some Experiments on a Machine for Measuring the Way of a Ship at Sea. By Mr. J. Smeaton, F. R. S. p. 532.

In the Philos. Trans. N° 391, Mr. Henry de Saumarez gives an account of a machine for measuring a ship's way more exactly than by the log. This machine consists of a first mover, in the form of the letter Y. On the 2 arms of the Y are fastened 2 vanes, inclined in such a manner that when the Y is hauled through the water by a rope, fastened to its stem or tail, it may turn round, and of consequence endeavour to turn the rope round. The other end of the rope, being fastened to the end of a spindle capable of moving freely round, will be made to do so by the rotations of the Y, communicated to the rope. A motion being thus communicated to a spindle within the ship, this spindle may be made to drive a set of wheel-work, which will register the turns of the Y; and the value of a certain number of these turns being once found, by proper experiments, they are easily reducible into leagues and degrees, &c. The only difficulty then is, whether this Y will make the same number of rotations in going the same space, when it is carried through the water fast, as when it is carried slow. On this head Mr. de Saumarez, as well in the paper above-cited, as in a subsequent one published in Philos. Trans. N° 408, has given an account of several trials, which he has made of it, from which it appears, that this machine in part answers the end proposed, and is in part defective; the errors of which he supposes to proceed from the sinking down of the Y into the water on a slow motion; the axis of its rotation being then more oblique to the horizon, than in a quick one.

In a machine constructed like this, it is evident that the end of the spindle, to which the rope is fastened, must be of sufficient strength and thickness, not only to bear the force or stress, that the hauling of the Y through the water will lay on it, in the greatest motion of a ship; but also to bear the accidental jerks of the waves. The thickness of the spindle then being determined by these conditions; it is also manifest, that to prevent the spindle from being pulled out of its place by the draft of the rope, there must be a shoulder formed on it, which must be greater than the part of the spindle before described, for the spindle to bear against. The size that Mr. Saumarez proposes to give to his Y, is 27 inches the whole length; 15 inches for the length of the arms, which are to be opened to a right angle; 8 inches for the length of each vane; $4\frac{1}{2}$ inches broad, and the stems and shank to be $\frac{2}{3}$ of an inch thick. According to these dimensions, the resistance that this part of the machine will meet with, in passing through the water, will, in the swift motions of the ship, be very considerable; consequently the necessary bulk of the pivot-end of the spindle, and its shoulder, will occasion a considerable friction in its turning, and retardation to the rotation of the machine.

To cure these defects, as much as possible, instead of the Y before described, Mr. S. made trial of a single plate of brass, of about 10 inches long, $2\frac{1}{2}$ broad, $\frac{1}{30}$ of an inch thick, and cut into an oval shape. This plate being set a little atwist, and fastened by one end to a small cord, in the manner of the Y, is likewise capable of making a rotation, in being drawn through the water; but with this difference, that as this is but a small thin plate drawn edgewise through the water, its resistance, in passing through it, is much less; of consequence, a much smaller line is sufficient to hold it, which again considerably diminishes the resistance; and this of course proves a double diminution of friction in the spindle; first, as the pressure upon it is less; and, secondly, as it allows the spindle and shoulder to be of a less diameter. To break the jerks of the waves, next to the end of the spindle he fixed a spiral spring of wire, to which the cord was fastened; which, by this means, was capable of playing backwards and forwards, and giving way to the irregularities of the sea: and lest the plate should lay fast hold of any thing, or any extraordinary jerk should damage the spindle or spring, a knob or button was fastened on the cord, at a small distance from the spring, which stopped on a hole in a piece of wood, and prevented the spring from being pulled out to above a certain length; so that all addition of force, beyond this, could only tend to break the cord, and carry away the plate. The spindle, being thus guarded from accidents, will allow of a still further diminution of its size; so that, at last, he ventured to make the spindle-pivot no more than $\frac{1}{40}$ of an inch diameter, and that of the shoulder $\frac{1}{8}$, being of tempered steel, and sufficiently smooth. The hole, in which the pivot, and against which the shoulder worked, was of agate likewise, well polished.

Being thus provided, in May 1751, he procured a boat, on the serpentine river in Hyde-park, to try how far the turns of the machine would be consistent with themselves, when the same space was measured over with different velocities. The course was determined at each end, by observing the coincidence of two trees, in a line nearly at right angles to the river. We however rowed beyond the mark, that the machine might be in full play when the course was begun: the spindle was stopped at the beginning and end; the numbers read off were as follow:

The space between the marks was, by estimation, about half a mile.

1st	rowing up the river,	in 11 min.	the plate made 615 revol.
2d	down	14	645
3d	up	18½	612
4th	down	9½	603
5th	up	18	620
6th	down	10	600

It is observable, that the greatest difference among the above observations, is between the 2d and 6th, being 645 and 600; the difference being about a 4th part of the whole; the times being 14 minutes and 10, both in going down the river; whereas those observations, which differ most in point of time, viz. the 3d and 4th, being performed in 18½ minutes, and 9½ minutes, have their revolutions more nearly alike, being 612 and 603; which differ only by a 68th of the whole. From these observations he was led to think, that the different velocities, with which a vessel moves forwards, would make no material difference in the number of rotations of the plate; or at least that those differences would be less than the irregularities arising from other causes, even in trials nearly similar.

The next trial of this machine was on board a small sailing vessel, in company with Dr. Knight, and Mr. William Hutchinson, an experienced seaman. The expedition was on the river Thames, and some leagues below the Nore. The intention of the trial here, was to find in general, how far it agreed with the log, and how it would behave in the swell of the sea; a comparison with the measure of a real distance being here impracticable, on account of the tides and currents. The method of trial was this: the whole log-line was suffered to run out, being 357 feet between the first knot and the end. The person who hove the log gave notice, at the extremes of this measure, that the person who attended the dial of the machine might stop the spindle at the beginning and end; while a third observed, by a second's-watch, the time taken up in running these 357 feet. By these means, we were enabled to ascertain the comparative velocity moved, and the number of turns of the plate at each trial, corresponding to 357 feet by the log; which, if the machine and log were both accurate, ought to have been always the same. The particulars of these experiments are contained in the following table.

Turns of the plate.		Seconds of time during the running out of 357 feet of log-line.	Turns of the plate.		Seconds of time during the running out of 357 feet of log-line
83	In the river at anchor by the tide	124	70	Before the wind at sea	56
82	The same repeated	134	70	The same	52
81	Sailing in the river	98	66	Before the wind in the river	55
79	In the river at anchor by the tide	135	64	The same	53
76	Sailing in the river	115	64	The same	60
74	At sea upon a wind	64	64	The same	43
74	The same repeated	69	63	At sea upon a wind	53
71	Sailing in the river	71	62	The same	52
70	The same	66	62	Sailing in the river	45
70	Before the wind at sea	77			

It appears from these trials, made in different positions of the vessel with regard to the wind, both in the river and at sea, as well by the tides at anchor, as in sailing, that the turns of the plate corresponding with the space of 357 feet by the log, were from 62 to 83; and the times in which this space was run, were from 45 to 135 seconds, the greater number of revolutions answering to the greater number of seconds, or slower movement of the vessel. On finding this considerable disagreement between the log and plate, when swift and slow motions are compared, Mr. S. did not suppose that they proceeded from a retardation of the plate in swift motions, but from the hauling home of the log in slow ones. For instance, the log, to do its office accurately, ought to remain at rest in the water, whatever be the motion of the vessel. But even the keeping the line straight, and much more the suffering the log to haul the line off the reel, as practised by many, will make the log in some measure follow the vessel, and will be greater in proportion as the time of continuance of this action is greater; and therefore the log will follow the ship twice as far in going one knot, when the ship is twice as long in running it. The consequence of this is, that a vessel always runs over a greater space than is shown by the log-line; but that this error is greater, in proportion as the vessel moves slower. It is this reason probably that has induced the practical seamen to continue the distance between their knots shorter than they are directed by the theory.

Afterwards, in the same summer, Mr. S. made such another expedition, in a sailing vessel, along with Capt. Campbell of the Mary yacht, and Dr. Knight. Having prepared two of these machines as near alike as possible, he determined to try, how far they were capable of agreement, when exposed to the same inconveniencies, and used together. During the trial of these machines, one made 86,716 revolutions, and the other made 88,184. During this space, they were compared at 10 several intervals. The revolutions between each interval differed from the proportion of these numbers, in the first comparison, $\frac{1}{19}$ of the whole interval. The errors of each interval, in the other comparisons, were, in order, $\frac{9}{17}$, $\frac{1}{19}$, $\frac{1}{80}$, $\frac{1}{54}$, $\frac{1}{14}$, $\frac{1}{80}$, $\frac{1}{67}$, $\frac{1}{14}$, $\frac{1}{16}$; the greatest errors being where the spaces were the shortest. In other respects, the plates seemed to perform their duty in the water well enough, though the sea was as rough in this voyage as our small vessel could well bear.

Lastly, being for some time on board the Fortune sloop of war, commanded by Alexander Campbell, Esq. in company with Dr. Knight, for the purpose of making trial of his new invented sea-compasses, I had frequent opportunities of making use of these machines, by comparing them with each other, with the log, and with real distances; and having, by repeated trials, pretty well ascertained the number of turns of the plate, equal to a given space, by the help of the log, in the manner before described, when the ship was on a middle velocity, he found the spaces, so measured, nearly consistent with themselves, and with the truth; but all this while the winds and weather were very moderate. It afterwards happened, that they ran 18 leagues in a brisk gale of wind, which drove them sometimes at the rate of 8 knots an hour, as appeared by heaving the log. During this run he observed, that the resistance of the water to the line and plate, was very considerable, and increased the friction of the spindle so much, as to prevent it from beginning to turn, till the plate had twisted the line to such a degree, that when it did set a-going, it would frequently run 150 or 200 turns at once. He also observed, that the wind coming across the course of the ship, blew the cord a good deal out of the direction of the spindle, and caused the line to rub against the safeguard hole, for the button to stop against, as above described; which undoubtedly occasioned considerable friction in that place. But the most untoward circumstance was, that being in a rough, but short chopping sea, and sailing obliquely across the waves, the plate would frequently be drawn from one wave to another through the air, without touching the water; and would jump from one wave to another; the unevenness of the surface, joined to the quickness of the motion, not permitting the plate to follow the depression of the water. This evil he endeavoured to remedy, by placing on the line, at a small distance before the plate, some hollow bullets, such as are made for nets, in order to keep the plate so low down in the water, as to be below the bottom of the waves. This, in part, he found they did; but at the same time they added so much resistance, in their passing through the water, that the inconvenience was as great one way as the other.

On making up the account of this run, the number of rotations were less, by full one-third, than they ought to have been, compared with former observations, which afforded a convincing proof, that this instrument was considerably retarded in quick motions. The length of the line made use of was about 20 fathoms, which he found necessary, that the water, disturbed by the body of the ship, might be tolerably settled before the plate was drawn through it; but this length of line was also an inconvenience, as it met with greater resistance in the water.

On the whole, it seemed that such an instrument is capable of measuring the way of a ship at sea, when its velocity does not exceed 5 sea miles an hour, to a degree of exactness exceeding the log. It therefore may be useful in the mensuration of the velocities of tides, currents, &c. and also in measuring distances at sea in taking surveys of coasts, harbours, &c. Thus far it seems capable of performing, on the supposition that it cannot be brought to a greater degree of perfection. But this he was very far from supposing: on the contrary, he thinks that it may be brought to answer the end of measuring the way of a ship at sea universally.

LXXI. Observations of some Eclipses of Jupiter's Satellites at Lisbon in 1753. *By J. Chevalier.* p. 546.

LXXII. Observation of a Solar Eclipse at Lisbon, Oct. 26, 1753. *By J. Chevalier.* p. 546.

An account of the above two articles is contained in the following.

LXXIII. An Account of some Astronomical Observations taken at Lisbon by M. J. Chevalier in the Year 1753. *By J. Short, M. A., F. R. S.* p. 548.

This gentleman mentions two emersions of the satellites of Jupiter, viz. one of the first, and another of the third, both observed, in a very clear air, with a Gregorian telescope 6 feet long. Dr. Bevis, from a great number of observations, has computed formulæ of tables for the times of the immersions and emersions of the first satellite of Jupiter, and which times we have seldom found to differ from the observations above 10^s: by comparing therefore the time of the emersion of the first satellite observed by this gentleman, with the time computed from these formulæ, the difference of longitude between London, at St. Paul's, and the place of observation at Lisbon, comes out to be $36^m\ 6^s$; and by several former corresponding observations the difference had been found to be $36^m\ 10^s$.

M. Chevalier further mentions the observation of the eclipse of the sun last October, through a telescope of 15 palms. He saw both the beginning and end, in a very clear air; and says that the greatest quantity of the eclipse was 11 digits and 5′, which he measured with a micrometer; but unluckily he has not given us either the diameter of the sun, or that of the moon, which he might have measured, (for the eclipse was annular) though he was at the pains of measuring all the digits, both in the increase and decrease of the eclipse. He further takes notice, that at the time of the greatest obscuration, the light of the sun was remarkably diminished; and that they were able to see Jupiter, Venus, and some stars of the first and 2d magnitude; but he could not see Mercury, on account of his proximity to the sun: and that a reflecting speculum, of 3 palms in diameter, which could melt lead, when placed in its focus, and instantly set wood in a flame, produced the same effects, even when the sun was 7 digits eclipsed; but that, about the time of the greatest obscuration it was not able to burn wood, though held in its focus for some time: and that at the same time the air became very cold, the wind blowing hard from the north; and that some vapours, or fog, were seen to rise out of the river and adjacent harbour.

LXXIV. Of an Instrument for Measuring Small Angles, the first Account of which was read before the Royal Society, May 10, 1753. *By Mr. John Dollond. Dated April* 4, 1754. p. 551.

Before entering on particulars relating to this micrometer, it will be proper to make a few preparatory observations on the nature of spherical glasses, so far as may be necessary to render the following explanation more easily understood.

Obs. 1.—It is a property of all convex spherical glasses, to refract the rays of light which are transmitted through them, in such a manner, as to collect all those that proceed diverging from any one point of a luminous object, to some other point; whose distance from the glass depends chiefly on its convexity, and the distance of the object from it.

Obs. 2.—The point where the rays are thus collected, may be considered as the image of that point from which they diverge. For if we conceive several radiant points thus emitting rays, which, by the refractive quality of the glass, are made to converge to as many other points; it will be an easy matter to understand how every part of the object will be truly represented. As this property of spherical glasses is explained and demonstrated by all the writers on optics, it being the very foundation of the science, the bare mention of it is sufficient for the present purpose.

Obs. 3.—It will be necessary however to observe further, that the lines connecting every point in the object with its corresponding points in the image, do all intersect in a certain point of the axis or line passing through the poles of the glass, where its two surfaces are parallel, and may be properly called its centre. Whence it appears, that the angles subtended by the object and its image, from that point, must be equal: and therefore their diameters will be in the same ratio as their distances from that point.

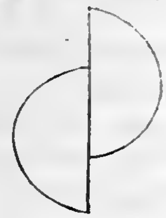
Obs. 4.—As the formation of the image by the glass depends entirely on the property above mentioned, viz. its collecting all the light, incident on it from the several points of the object, into as many other points at its focus; it follows, that any segment of such a glass will also form an image equal, and every way similar, to that exhibited by the whole glass; with this difference only, that it will be so much darker, as the area of the segment is less than that of the whole glass.

Obs. 5.—The axis of a spherical glass, is a line connecting the centres of the spheres, to which the two surfaces are ground; and wherever this line passes through the glass, there the surfaces are parallel. But if it happens that this line does not go through the substance of the glass, such a glass is said to have no internal centre; but it is conceived to be in its plane produced till it meets the axis: and this imaginary point, though external to the glass, is as truly its centre, and is as fixed in its position to it, as if it were actually within its substance.

Obs. 6.—If a spherical glass, having its centre or pole near its middle or centre of its circumference, should be divided by a straight line through the middle; the centre will be in one of the segments only. For how exact soever a person may be supposed to be in cutting it through the centre; yet it is hard to conceive how a mathematical point should be divided in two: therefore the centre will be internal to one of the segments, and external to the other. But if a small matter be ground away from the straight edge of each segment, both their centres will become external; and so they will more easily be brought to a cc incidence.

Obs. 7.—If these two segments should be held together, so as to make their centres coincide; the images, which they give of any object, will likewise coincide, and become a single one. This will be the case when their straight edges are joined to make the glass, as it were, whole again: but let the centres be any-how separated, their images will also separate, and each segment will give a separate and distinct image of any object to which they may be exposed.

Obs. 8.—Though the centres of the segments may be drawn from their coincidence, by removing the segments in any direction whatever; yet the most convenient way for this purpose is, to slide their straight edges one along the other, till they are removed, as the figure in the margin represents them: for thus they may be moved without suffering any false light to come in between them. And by this way of removing them, the distance between their centres may be very conveniently measured, viz. by having a Vernier's division fixed to the brass work, that holds one segment, so as to slide along a scale on the plate to which the other part of the glass is fitted.



Obs. 9.—As the images of the same object are separated by the motion of the segments; so those of different objects, or different parts of the same object, may be made to coincide. Suppose the sun, moon, or any planet, to be the object; their two images may, by this contrivance, be removed till their opposite edges are in contact: in which case the distance between the centres of the two images will be equal to the diameter of either; and so of any other object whatever.

Obs. 10.—This divided glass may be used as a micrometer, 3 different ways. In the first place, it may be fixed at the end of a tube, of a suitable length to its focal distance, as an object-glass; the other end of the tube having an eye-glass fitted as usual in astronomical telescopes. 2dly, It may be applied to the end of a tube much shorter than its focal distance, by having another convex glass within the tube, to shorten the focal distance of that which is cut in two. Lastly, it may be applied to the open end of a reflecting telescope; either of the Newtonian, Gregorian, or Cassegrain construction. And though this last method is much the best, and most convenient, of the three; yet as the first is the most natural, as well as the easiest to be understood, it will be proper to explain it fully, and to demonstrate the principles on which this micrometer is constructed, by supposing it made use of in the first way: which being done, the application of it to other methods will be readily understood.

Having thus, by the foregoing observations, given a general idea of the nature and effects of this divided object-glass, Mr. D. proceeds to demonstrate the principles from which the measures of the angles are to be obtained by this instrument, by the following propositions.

Prop. I. Suppose a divided object-glass fixed at the end of a tube, according to the first method, and the tube directed to the object intended to be measured; and suppose also the segments removed from their original position, as directed under Obs. 8, till the opposite edges of the two images are seen in contact at the focus of the eye-glass: then the angle subtended by the distance between the centres of the segments, from the focus of the eye-glass, where the edges are seen in contact, is equal to the angle subtended by the diameter of the object from that same point.

Demons. Let the line AB, fig. 7, pl. 8, represent the diameter of the object to be measured; and the points C, D the centres of the two glass segments: also G the focus where the images of the extremities of the object coincide. It is evident, from Obs. 3, that AG and BG are straight lines, that pass through the centres of the segments, and connect the extreme points of the object with their corresponding points in the images; and therefore, as the diameter of the object and the distance between the centres of the segments, are both inscribed between these two lines, they must needs subtend the same angle from the point where those lines meet; which is at G.

The focal distance CG, or DG, is variable, according to the distance of the object from the glass: so that it decreases as the distance of the object from the glass increases; and when the object is so far off, that the focal length of the glass bears no proportion to its distance, then will it be least of all; as CF or DF; and the point F is called the focus of parallel rays. Any other focus, as G, being the focus of a near object, is called a respective focus; as it respects a particular distance: but the focus of parallel rays respects all objects that are at a very great distance; such as is that of all the heavenly bodies.

Prop. 2.—The distance HE of the object from the glass, is to EF, the focal distance of parallel rays, as the distance HG of the object from its image, is to EG, the distance of the image from the glass: that is, HE : FF :: HG : EG.

The demonstration of this proposition may be gathered from any treatise of dioptrics; it being a general rule for finding the respective focus to any given distance, when the focus of parallel rays is known.

Prop. 3.—The angle subtended by the diameter of the object, from the glass, is equal to that subtended by the opening of the centres of the segments, from the focus of parallel rays. That is, the angle AEB equal to the angle CFD.

Demons.—It appears, by inspection of the figure, that AB : CD :: HG : EG. And by the last prop. HE : EF :: HG : EG. Then, as the two last terms of these two analogies are alike; the two first terms of one will be in the same proportion as the two first terms of the other; which gives the following proportion: AB : CD :: HE : EF. Whence the truth of the proposition is evident.

From this proposition it appears, that the angle subtended by the diameter of the object from the glass, is found without any regard to the distance of the object, or to the distance of the respective focus, where the image is seen; as the measure depends entirely on the focus of parallel rays, and the opening of the segments. We may hence also derive a rule for the quantity of the angle, without considering the length of the glass. Let an object, whose diameter is known, be set up at some known distance; the angle it will subtend from the glass may then be found by trigonometry: then let it be measured by this micrometer, and the distance between the centres of the segments, found on the scale already mentioned, will be the constant measure of the same angle, in all other cases; because the distance of the object makes no alteration in the measure of the angle, as has been demonstrated: and thus having obtained the distance between the centres of the segments, which answers to any one angle, all other distances may be computed by the rule of three.

All that has been hitherto said relates to the first method of using this micrometer; that is, by fitting it to the end of a tube suited to its focal length, and by viewing the images with a proper eye-glass, in the manner of an astronomical telescope. But the length of the tube, in this way, would be very troublesome; and therefore it will be proper to consider other methods for an easier management. He therefore proceeds to the 2d method, mentioned in Obs. 10, which is, by using another object-glass to shorten the focus of that which serves for the micrometer. To facilitate the understanding of this method, it will be necessary to premise the following observation.

Obs. 11.—Rays of light, which are brought to such convergency as to form the image of an object, proceed, after that, diverging in the manner they did when they issued from the object before they were transmitted through the glass, and therefore they may be again collected by another spherical glass, so as to form a 2d representation of the same object; which may again be repeated by a 3d glass, &c. So that the first image may be considered as an object to the 2d glass, and the 2d image will be an object to the 3d, and so on. Though these images may be very different in respect to their magnitudes, yet they will be all similar; being true representations of the same object: this will hold good, though the 2d glass should be put so near the first, as to receive the rays before the image is formed: for as the rays are tending to meet at a certain distance, the 2d will receive them in that degree of convergency, and, by an additional refraction, bring them to a nearer focus; but the image will still be similar to that which would have been made by the first glass, if the 2d had not been there.

On this principle all refracting telescopes are made; some of which are a combination of 4, 5, or 6 glasses. The first glass forms an image of the object; the 2d repeats the image, which it receives from the first; and so on, till the last glass brings a true representation of the object to the eye. The same may be said of reflecting telescopes: for a spherical mirror acts in the same manner, in that respect, as a spherical glass.

Now let this be applied to the subject in hand. Suppose the focal distance of the divided object-glass to be about 40 feet; and suppose the segments to be opened wide enough to bring the opposite edges of an object in contact: then let another object-glass, uncut, be fixed within the tube, of a proper degree of convexity, to shorten the focus of the other as much as may be required; suppose to 12 feet: by what has been just now observed, this glass will represent the two images in the same form which would have been exhibited by the divided glass, if this other glass had not been there. For though the images are not yet formed, when the 2d glass receives the rays; yet, as those rays are converging towards it, the 2d glass must represent those images in the same position, and form, as the tendency of the rays requires. For while the segments are fixed in their position to each other, their images will also be fixed in their position; and let them be repeated ever so many times, by refraction through spherical glasses, or by reflexion from spherical mirrors, they can suffer no alteration in their position to each other. By this means the telescope may be shortened at pleasure, though the scale for the measure of the angles will remain the same. The only inconvenience which the shortness of the telescope introduces, is a want of sufficient distinctness; which will so far hinder the exactness of the observation, as the contact of the edges cannot be so accurately determined, as they might be with longer telescopes.

This difficulty is entirely removed by fixing the divided glass at the end of a reflecting telescope: for the reflections and refractions, which the rays must undergo in passing through the telescope, will no way alter the position of the images which the rays, that have passed through the segments, are tending to: for, as has been already observed, a number of reflections and refractions may repeat the images, and alter their magnitudes; but can make no alteration in their proportions.

Therefore this way of fixing the divided glass to a reflecting telescope, which was the 3d method proposed, is by far the best; as such telescopes of moderate and manageable lengths, when well made, are capable of magnifying considerably, and showing objects to great advantage. This micrometer's being applicable to the reflecting telescope with so much certainty, is no inconsiderable advantage: for any one will easily understand, that to measure the diameter of a planet exactly, it is necessary that the planet be magnified, and shown distinctly, which could not be obtained in the common way without very great lengths; such as rendered it very difficult, not to say impracticable, to take exact measures. Besides, the common micrometer is limited in this respect on another account; viz. because the diameter of the planet cannot be measured without having the whole planet within the field of the telescope, which confines the magnifying power within very narrow bounds; whereas, by this method, nothing more is required, than to see the contact of the edges, which allows the magnifying power to be increased at pleasure.

In the common micrometer, the object is to be taken between 2 wires, so that the contact of its edges with those wires cannot be observed at one view; and the least motion of the telescope, while the observer is turning his eye from one wire to the other, must oblige him to repeat the observation; whereas, by this method the contact of the edges of the images is not at all affected by the motion of the telescope. Whence the comparison of this micrometer with the common sort, in this respect, stands thus: the one requires great steadiness in the telescope, but yet it is applicable to none but such as are very difficult to keep steady; the other does not require such steadiness, though it is applicable to short telescopes, which are easily managed. These advantages not only add to the certainty of the observation, but assist vastly in the expedition; for an observer may make 20 observations in this way, where he could scarcely, with much fatigue, be sure of one with the common micrometer. Expedition in making observations must be allowed a very great advantage, in this climate, where the uncertainty of the weather renders astronomical observations so precarious, that no opportunities, even the most transient, should be let slip. An instance of this was given in to the R. S., in an account of the eclipse of the sun last October. As the motion of the telescope gives the observer no great inconvenience in this method; neither does the motion of the object at all disturb his observation, such a motion as that of the heavens is. This gives him leave to take the diameter of a planet in any direction; or the distance between two stars or planets, let their situation be how it will; in which respect the common micrometer is absolutely defective; as it can give no angles, but such as are perpendicular to the line of their motion; though the diameters of the planets, in other directions, is very much wanted; it being highly probable, from the laws of motion, and what we see in Jupiter, that such planets as revolve round their axes, have their polar diameters shorter than their equatorial ones.

The distances of Jupiter's satellites from each other, or from Jupiter's body, cannot be measured, with any certainty, in the common way, as their position is always very far from being at right angles with the line of their motion: neither can the moon's diameter, which must be taken from horn to horn, scarcely ever be obtained that way, because it very rarely happens, that the diameter to be measured lies at right angles to the line of her motion. The same may be said of the distance between two stars, But this micrometer gives angles, in every direction, with equal ease and certainty; the observation being also finished in an instant, without any trouble or fatigue to the observer. For as there are no wires made use of this way in the field of the telescope; so the observer has no concern about any illumination. The largeness of the scale deserves also to be taken notice of, as it may, in this micrometer, be increased almost at pleasure, according as the smallness of the object requires. Another inconvenience attending the common micrometer is, the variation of the scale, according to the distance of the object. As the telescope must be lengthened, or drawn out farther, for short distances; the scale, which depends on that length, is thereby increased; which renders the measure of the angle very uncertain: whereas, in this micrometer, the scale is the same at all distances: so that the angle may be measured with the utmost certainty, without any regard to the distance of the objects.

On the whole, it may be concluded, that this micrometer is a complete instrument in its kind; having many advantages above the common sort, without any of their disadvantages: and there is no doubt but, when brought into practice, it will tend much to the advancement of astronomy.

LXXV. Of an Earthquake felt at York, April 19, 1754. *By Mr. David Erskine Baker.* p. 564.

This small shock felt by Mr. B. at York, lasted about 3 seconds. It gave an undulating motion to the buildings, made the windows, &c. rattle, and was preceded by a rumbling noise, like a carriage over a pavement. Its direction seemed to be from S.W. to N.E., and it was felt at several other places, at many miles distance.

*LXXVI. An Investigation of some Theorems which suggest some Remarkable Properties of the Circle, and are of Use in Resolving Fractions, whose Denominators are certain Multinomials, into more Simple ones. By Mr. John Landen.** p. 566.

That the principal theorems, below investigated, will be of considerable use in the doctrine of fluxions, by rendering, in many cases, the business of computing

* Mr. Landen, who was born at Peakirk near Peterborough, in Northamptonshire, in 1719, was in a great measure a self-taught mathematician, a branch of learning in which he rose to the first rank of eminence. He became a respectable contributor to the mathematical part of the Ladies' Diary so early as the year 1744; to which work he continued his contributions, either in his own name, or under various fictitious ones, till within a very few years of his death. His first paper in the Phil. Trans. above printed for the year 1754, is no mean specimen of his taste and skill in that line. Besides this, and several other valuable papers printed at different times in these Transactions, he published some curious and separate works himself. As, 1. Mathematical Lucubrations in 1755; containing a variety of tracts relating to the rectification of curve lines, the summation of series, the finding of fluents, &c. 2. A Discourse on the Residual Analysis, 1758; being a new branch of the algebraic art, of very extensive use, both in pure mathematics and natural philosophy. 3. The 1st book of the same Residual Analysis, in 1764; explaining the principles, and applying them in a number of curious speculations. 4. Animadversions on Dr. Stewart's Computation of the Sun's Distance from the Earth, 1771. 5. Mathematical Memoirs, 1780, vol. i.; on a variety of subjects; with an appendix containing a very extensive collection of forms for finding fluents. 6. Several Observations on converging Series, in 1781, 1782, 1783. 7. And lastly the 2d vol. of the Mathematical Memoirs in the latter end of 1789; containing, besides a solution of the general problem concerning rotatory

fluents more easy, will, on perusal, be obvious, Mr. L. says, to every one acquainted with that branch of science.

Art. 1.—Supposing $\frac{n\dot{x}}{\sqrt{x^2-1}} = \frac{\dot{y}}{\sqrt{y^2-1}}$, where $\dot{x}$ and $\dot{y}$ denote the fluxions of the variable quantities x and y respectively, and n an invariable quantity; it is proposed to find, in terms of y and z, the equation of which z is a root, and $z^2 - 2xz + 1 = 0$, a divisor.

Taking the fluents of the given fluxionary equation, we have, supposing $x = 1$ when y is $= 1$, hyp. log. of $(x + \sqrt{x^2-1})^n =$ hyp. log. of $y + \sqrt{y^2-1}$, or $(x + \sqrt{x^2-1})^n = y + \sqrt{y^2-1}$: whence, by substituting for x its value $\frac{z^2+1}{2z}$ (found by the equation $z^2 - 2xz + 1 = 0$), we have $z^n = y + \sqrt{y^2-1}$: therefore $z^n - y$ is $= \sqrt{y^2-1}$; and, squaring both sides, $z^{2n} - 2yz^n + y^2 = y^2 - 1$. Consequently $z^{2n} - 2yz^n + 1$ is $= 0$; which, supposing n a positive integer, is the equation sought.

Now it is obvious, n being such an integer, that this equation will have as many trinomial divisors, of the form $z^2 - 2xz + 1$, as there are values of x corresponding to a given value of y: which values of x, when y is not greater than 1, nor less than -1 (the only case I propose to consider), will not be readily obtained from the equation $(x + \sqrt{x^2-1})^n = y + \sqrt{y^2-1}$ found above: but, if we multiply the given fluxionary equation by $\frac{1}{\sqrt{-1}}$, we get $\frac{n\dot{x}}{\sqrt{1-x^2}} = \frac{\dot{y}}{\sqrt{1-y^2}}$; of which the equation of the fluents is $n \times$ circ. arc rad. 1, cosine $x =$ circ. arc rad. 1. cosine y; where x is $= 1$ when y is $= 1$, agreeable to the supposition made above, when we took the fluents of the given fluxionary equation by logarithms. Therefore if A be put for the least arc whose cosine is y, and C for the whole circumference, radius being 1; y being the cosine of A, A + C, A + 2C, A + 3C, &c. x will be the cosine of $\frac{A}{n}$, $\frac{A+C}{n}$, $\frac{A+2C}{n}$, &c. .... to $\frac{A+(n-1)\times C}{n}$.

Consequently, expressing the last-mentioned cosines, or the several values of

motion, an investigation of the motion of the equinoxes, in which Mr. L. has, first of any one, pointed out the cause of Sir Isaac Newton's mistake in his solution of this celebrated problem. Mr. L. as he had chiefly completed this work during some intervals from the stone, with which disorder he was severely afflicted in the latter part of his life, so he just lived to see it printed, and received a copy of it the day before his death, which happened Jan. 15, 1790, at Milton, near Peterborough, being 71 years of age.

About the year 1762, Mr. L. became agent and land-steward to Earl Fitzwilliam; an employment which he resigned only 2 years before his death. And in 1766 he was elected F.R.S. Though Mr. L. was doubtless one of the greatest mathematicians that this or any country has produced; his merit in this respect was not more conspicuous than his moral virtues. As his compositions were profound, and elegantly clear and simple; so his manners and deportment were manly, dignified, genteel, and benevolent. The strict integrity of his conduct, his great humanity, and readiness to serve every one to the utmost of his power, procured him respect and esteem from all his acquaintance.

x, by p, q, r, s, &c. $z^{2n} - 2yz^n + 1$ will be $= (z^2 - 2pz + 1) \times (z^2 - 2qz + 1) \times (z^2 - 2rz + 1)$, &c. (n), when n is a positive integer, (as we shall always suppose it to be), let z be what it will.

Hence may be easily deduced a demonstration of that remarkable property of the circle first discovered by Mr. Cotes: but as that property has already been demonstrated by several mathematicians, Mr. L. omits taking any further notice of it, and proceeds in the investigation of some other useful theorems which had never been published.

Art. 2.—If y be $= 1$; then, A being $= 0$; p, q, r, &c. will be the cosines of $\frac{0}{n}, \frac{c}{n}, \frac{2c}{n}, \frac{3c}{n}$, &c. (n) respectively: therefore p will be $= 1$; and, if n be an even number, one of the cosines q, r, s, &c. will be $= -1$, one of the arcs $\frac{c}{n}, \frac{2c}{n}, \frac{3c}{n}$, &c. being then $= \frac{c}{2}$.

Art. 3.—If y be $= -1$; then, A being $= \frac{c}{2}$; p, q, r, s, &c. will be the cosines of $\frac{c}{2n}, \frac{3c}{2n}, \frac{5c}{2n}$, &c. (n) respectively: therefore, if n be an odd number, one of those arcs will be $\frac{c}{2}$, whose cosine is -1.

Art. 4.—If, in the equations $z^{2n} - 2yz^n + 1 = 0$, and $z^2 - 2xz + 1 = 0$, we substitute $v - 1$ for z, they become $(v-1)^{2n} - 2y \times (v-1)^n + 1 = 0$, and $(v-1)^2 - 2x \times (v-1) + 1 = v^2 - (2 + 2x) \times v + 2 + 2x = 0$. Consequently

$$\left.\begin{array}{l} v^{2n} - 2nv^{2n-1} + \ldots + 2n \times \frac{2n-1}{2} v^2 - 2nv + 1 \\ \ldots \mp 2yn \times \frac{n-1}{2} v^2 \pm 2ynv \mp 2y \\ + 1 \end{array}\right\} =$$

$(v^2 - \overline{2 + 2p} \times v + 2 + 2p) \times (v^2 - \overline{2 + 2q} \times v + 2 + 2q) \times (v^2 - \overline{2 + 2r} \times v + 2 + 2 + 2r) \times$ &c. (n); where, of the two signs prefixed to the terms where y is a factor, the upper or lower takes place, according as n is an even or an odd number. Whence, by the nature of equations, it follows, that $(2 + 2p)$ $(2 + 2q) \times (2 + 2r)$, &c. is $= 2 \mp 2y$. But this equation vanishing when y is $= 1$ and n an even number, or when y is $= -1$ and n an odd number, it will be proper to consider those two cases more particularly.

Art. 5.—First, let us suppose $y = 1$, and n an even number: then p being $= 1$, and one of the other cosines q, r, s, &c. $= -1$ (*Art.* 2); we shall have $v^{2n} - 2nv^{2n-1} + \ldots + n^2 v^2 = (v^2 + 0) \times (v^2 - 4v + 4) \times (v^2 - \overline{2 + 2q})$ $\times (v + 2 + 2q) \times (v^2 - \overline{2 + 2r} \times v + 2 + 2r)$, &c. Therefore dividing by v^2, $v^{2n-2} - 2nv^{2n-3} + \ldots + n^2 = (v^2 - 4v + 4) \times (v^2 - \overline{2 + 2q} \times v + 2 + 2q$ $\times (v^2 - \overline{2 + 2r} \times v + 2 + 2r)$, &c. that factor in which the value of the cosine q, or r, &c. is -1, being expunged.

Consequently n^2 is $= 4 \times (2 + 2q) \times (2 + 2r) \times (2 + 2s)$, &c. when the factor, whose value is nothing, is expunged.

Art. 6.—Let us now suppose $y = -1$, and n an odd number: then one of the cosines p, q, r, &c. being $= -1$ (*Art.* 3),

$v^{2n} - nv^{2n-1} + \ldots\ldots + n^2v^2$ will be $= (v^2 + 0) \times (v^2 - \overline{2 + 2p} \times v + 2p) \times (v^2 - \overline{2 + 2q} \times v + 2 + 2q)$ &c. Therefore, dividing by v^2,

$v^{2n-2} - 2nv^{2n-3} + \ldots\ldots + n^2$ will be $= (v^2 - \overline{2 + 2p} \times v + 2 + 2p) \times (v^2 - \overline{2 + 2q} \times v + 2 + 2q)$, &c. and consequently $n^2 = (2 + 2p) \times (2 + 2q) \times (2 + 2r)$, &c. when the factor, whose value is nothing, is expunged.

Art. 7.—Substituting in the equations $z^{2n} - 2yz^n + 1 = 0$, and $z^2 - 2xz + 1 = 0$, $(\frac{a + \omega}{a - \omega})$ instead of z, we have

$$(\frac{a + \omega}{a - \omega})^{2n} - 2y \times (\frac{a + \omega}{a - \omega})^n + 1 = \frac{(a + \omega)^{2n} - 2y \times (a + \omega)^n \times (a - \omega)^{2n} + (a - \omega)^{2n}}{(a - \omega)^{2n}} = 0,$$

and $(\frac{a + \omega}{a - \omega})^n - 2x \times \frac{a}{a - \omega} + 1 = \frac{(a + \omega)^2 - 2x \times (a + \omega) + (a - \omega) + (a - \omega)^2}{(a - \omega)^2}$

$= \frac{(2 + 2x) \times (\omega^2 + \frac{1 - x}{1 + x} a^2)}{(a - \omega)^2} = 0.$ Consequently,

$(a + \omega)^{2n} - 2y \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n}$ will be $= (2 + 2p) \times (2 + 2q) \times (2 + 2r)$, &c. $\times (\omega^2 + \frac{1 - p}{1 + p} a^2) \times (\omega^2 + \frac{1 - q}{1 + q} a^2) \times (\omega^2 + \frac{1 - r}{1 + r} a^2)$, &c.

But, by *Art.* 4, $(2 + 2p) \times (2 + 2q) \times (2 + 2r)$, &c. is $= 2 \mp 2y$, the upper or lower of the two signs prefixed to y taking place, according as n is an even or an odd number.

Therefore $(a + \omega)^{2n} - 2y \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^n$ is $= (2 + 2y) \times (\omega^2 + \frac{1 - p}{1 + p} a^2) \times (\omega^2 + \frac{1 - q}{1 + q} a^2) \times (\omega^2 + \frac{1 - r}{1 + r} a^2)$, &c.

Now p being the cosine of any number of degrees, radius being 1, $\frac{1 - p}{1 + p} a^2$ will be the square of the tangent of half so many degrees, radius being a: therefore, denoting that tangent by b; and the tangents of half the arcs described with the radius a, whose cosines, when the radius is 1, are q, r, s, &c. being denoted by c, d, e, &c. respectively; we have $(a + \omega)^{2n} - 2y \times (a + \omega)^n + (a - \omega)^n + (a - \omega)^{2n} = (2 \mp 2y) \times (\omega^2 + b^2) \times (\omega^2 + c^2) \times (\omega^2 + d^2)$, &c. But when y is $= 1$, and n an even number; or $y = -1$, and n an odd number; $2 \mp 2y$ being $= 0$; nothing can be determined from that equation: therefore, in those cases, recourse must be had to what is done above.

Art. 8.—Let us suppose $y = 1$, and n an even number: then the equation $(a + \omega)^{2n} - 2y \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n} = (2 + 2p) \times (2 + 2q) \times (2 + 2r)$, &c. $\times (\omega^2 + \frac{1 - p}{1 + p} a^2) \times (\omega^2 + \frac{1 - q}{1 + q} a^2) \times (\omega^2 + \frac{1 - r}{1 + r} a^2)$, &c. becomes $(a + \omega)^{2n}$

$- 2 \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n} = 4 \times (2 + 2q) \times (2 + 2r)$, &c. $\times$ $\omega^2 \times (\omega^2 + \frac{1-q}{1+q} a^2) \times (\omega^2 + \frac{1-r}{1+r} a^2)$, &c. p being $= 1$ (art. 2) and $\frac{1-p}{1+p} a^2 (= b^2) = 0$. Moreover, one of the other cosines q, r, s, &c. being $= - 1$ (art. 2) some one of the factors $2 + 2q$, $2 + 2r$, $2 + 2s$, &c. will vanish; which factor being expunged from the product $4 \times (2 + 2q) + (2 + 2r)$, &c. and restored to the divisor $\omega^2 + \frac{1-q}{1+q} a^2$, or $\omega^2 + \frac{1-r}{1+r} a^2$, &c. from which it was taken, that divisor will become $4a^2$; and the product $4 \times (2 + 2q) \times (2 + 2r)$, &c. will then (by art. 5) be $= n^2$.

Consequently $(a + \omega)^{2n} - 2 \times (a + \omega)^n + (a - \omega)^n + (a - \omega)^{2n}$, will be $=$ $n^2 \times \omega^2 \times 4a^2 \times (\omega^2 + c^2) \times (\omega^2 + d^2)$, &c. where the factor $4a^2$ takes place instead of $\omega^2 +$ sq. of the tang. of 90°.

If y be $= 1$, and n an odd number, p will be $= 1$, and $b = 0$; but no one of the cosines q, r, s, &c. will be $= - 1$, as when n is an even number. Therefore, in this case, the equation $(a + \omega)^{2n} - 2y \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n} = (2 + 2y) \times (\omega^2 + b^2) \times (\omega^2 + c^2)$, &c. becomes $(a + \omega)^{2n} - 2 \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n} = 4 \times \omega^2 \times (\omega^2 + c^2) \times (\omega^2 + d^2)$, &c.

Art. 9. By taking the square root of $(a + \omega)^{2n} - 2 \times (a + \omega)^n \times (a - \omega)^n + (a - \omega)^{2n}$, and of its two values just now found, we have, when n is an even number, $(a + \omega)^n - (a - \omega)^n = 2an\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. $2a$ taking place instead of $\sqrt{\omega^2 + \text{sq. of the tang. of } 90^\circ}$.

And, when n is an odd number, $(a + \omega) - (a - \omega) = 2\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. Whence the following construction is inferred.

Art. 10. Describe, about the centre C (pl. 11, fig. 1 and 2), with the radius a, the circle PA′A″A‴, &c.; draw the diameter PCQ, and the tangent B‴PB⁵; divide the semicircumference PA′Q into as many equal parts PA′, A′A″, A″A‴, &c. as there are units in the integer n; draw the secants CA′B′, CA″B″, &c. and, taking on CQ any point O, draw K‴OK⁵ parallel to B‴PB⁵; likewise draw B′K′, B″K″, B‴K‴, &c. parallel to PQ, and call CO, ω.

Then will q be the cosine of twice the angle PCA′, r the cosine of twice PCA″, s the cosine of twice PCA‴, &c. if the radius be 1.

Therefore PB′ = OK′ will be $= c$, PB″ = OK″ $= d$, &c. and CK′ $= \sqrt{\omega^2 + c^2}$, CK″ $= \sqrt{\omega^2 + d^2}$, &c. Consequently $\text{OP}^n - \text{OQ}^n$ being $= (a + \omega)^n - (a - \omega)^n$, and $n \times \text{PQ} \times \text{CO} \times \text{CK}' \times \text{CK}''$, &c. $= 2an\omega \times \sqrt{\omega^2 + c^2} \times \sqrt{\omega^2 + d^2}$, &c. when n is an even number; $\text{OP}^n - \text{OQ}^n$ will then be $= n \times \text{PQ} \times \text{CO} \times \text{CK}' \times \text{CK}''$, &c. where the diameter PQ takes place instead of the infinite quantity $\text{CK}^{\frac{1}{2}n}$.

But if n be an odd number, $\text{OP}^n - \text{OQ}^n$ will be $= 2 \times \text{CO} \times \text{CK}' \times \text{CK}'' \times \text{CK}'''$, &c.

Art. 11. It is evident that, of the factors CK′, CK″, CK‴, &c. the first and last, the second and last but one, &c. are respectively equal to each other.

Therefore, omitting the squares of the factors below PQ, and the squares of their values, $OP^n - OQ^n$ is $= n \times PQ \times CO \times CK'^2 \times CK''^2 \times CK'''^2$, &c. and $(a+\omega)^n - (a-\omega)^n = 2an\omega \times (\omega^2 + c^2) \times (\omega^2 + d^2)$, &c. when n is an even number; or $OP^n - OQ^n$ is $= 2 \times CO \times CK'^2 \times CK''^2 \times CK'''^2$, &c. and $(a+\omega)^n - (a-\omega)^n = 2\omega \times (\omega^2 + c^2) \times (\omega^2 + d^2)$, &c. when n is an odd number.

Art. 12. If we suppose $y = -1$, and n an odd number, it will appear, by proceeding much in the same manner as in art. 8, that $(a+\omega)^{2n} + 2 \times (a+\omega)^n \times (a-\omega)^n + (a-\omega)^{2n}$ is $= n^2 \times 4a^2 \times (\omega^2 + b^2) \times (\omega^2 + c^2) \times (\omega^2 + d^2)$ &c. where the factor $4a^2$ takes place instead of ω^2 + sq. of the tang. of 90°.

If y be $= -1$, and n an even number, $(a+\omega)^{2n} + 2 \times (a+\omega)^n \times (a-\omega)^n + (a-\omega)^{2n}$ is $= 4 \times (\omega^2 + b^2) \times (\omega^2 + c^2)$, &c.

Whence, by extracting the square root of both sides of those equations, we have, when n is an odd number, $(a+\omega) + (a-\omega)^n = 2an \times \sqrt{\omega^2 + b^2} \times \sqrt{\omega^2 + c^2}$, &c. $2a$ taking place instead of $\sqrt{\omega^2 + \text{sq. of the tang. of } 90°}$; and, when n is an even number, $(a+\omega)^n + (a-\omega)^n = 2 \times \sqrt{\omega^2 + b^2} \times \sqrt{\omega^2 + c^2}$, &c. Hence we infer this construction.

Art. 13. Having described about the centre C (fig. 3 and 4) with the radius a, the circle $Pa'A'a''A''$, &c. draw the diameter PCQ, and the tangent $b''Pb^4$; divide the semicircumference $Pa'Q$ into as many equal parts Pa', $a'A'$, $A'a''$, &c. as there are units in $2n$; draw the secants $Ca'b'$, $Ca''b''$, &c. and, through any point (O) in CQ, draw $k''Ok^4$ parallel to $b''Pb^4$; likewise draw $b'k'$, $b''k''$, &c. parallel to PQ; and call CO, ω.

Then, if the radius be 1, p will be the cosine of twice the angle PCa', q the cosine of twice PCa'', &c. therefore $Pb' = Ok'$ will be $= b$, $Pb'' = Ok'' = c$, &c. and $Ck' = \sqrt{\omega^2 + b^2}$, $Ck'' = \sqrt{\omega^2 + c^2}$, &c.

Consequently $OP^n + PQ^n$ being $= (a+\omega)^n + (a-\omega)^n$, and $n \times PQ \times Ck' \times Ck''$, &c. $= 2an \times \sqrt{\omega^2 + b^2} \times \sqrt{\omega^2 + c^2}$, &c. when n is an odd number; $OP^n + OQ^n$ will then be $= n \times PQ \times Ck' \times Ck''$, &c. where the diameter PQ takes place instead of the infinite quantity $Ck^{\frac{1}{2}n+\frac{1}{2}}$.

But if n be an even number, $OP^n + OQ^n$ will be $= 2 \times Ck' \times Ck''$, &c.

Art. 14. It is obvious that, of the factors Ck', Ck'', &c. the first and last, the second and last but one, &c. are respectively equal to each other: therefore the squares of the factors below PQ, and the squares of their values, being omitted, $OP^n + OQ^n$ is $= n \times PQ \times Ck'^2 \times Ck''^2$, &c. and $(a+\omega)^n + (a-\omega)^n = 2an \times (\omega^2 + b^2) \times (\omega^2 + c^2)$, &c. when n is an odd number; or $OP^n + OQ^n$ is $= 2 \times Ck' \times Ck''^2$, &c. and $(a+\omega)^n + (a-\omega)^n = 2 \times (\omega^2 + b^2) \times (\omega^2 + c^2)$, &c. when n is an even number.

Art. 15. Writing, in the equation $(a+\omega)^{2n} - 2y \times (a+\omega)^n \times (a-\omega)^n + (a-\omega)^{2n} = (2 \mp 2y) \times (\omega^2 + b^2) \times (\omega^2 + c^2)$, &c. (found by art. 7) $a - u$ for ω, the same becomes $(2a-u)^{2n} - 2yu^n \times (2a-u)^n + u^{2n} = (2 \mp 2y) \times$

$(u - 2au + a^2 + b^2) \times (u^2 - 2au + a^2 + c^2)$, &c. $= (2 \mp 2y) \times (u^2 - 2au + \beta^2) \times (u^2 - 2au + \gamma^2) \times (u - 2au + \delta^2)$, &c. if instead of $\sqrt{a^2 + b^2}$, $\sqrt{a^2 + c^2}$, &c. (the secants of the arcs of which b, c, d, &c. are tangents) we put β, γ, δ, &c.

And, by a like substitution in the equations in art. 11 and 14 it appears, that $(2a - u)^n - u^n$ is $= 2an \times (a - u) \times (u^2 - 2au + \gamma^2) \times (u^2 - 2au + \delta^2)$, &c. or $2 \quad (a - u) \times (u^2 - 2au + \gamma^2) \times (u^2 - 2au + \delta^2)$, &c. according as n is an even or an odd number: and that $(2a - u)^n + u^n$ is $= 2an \times (u^2 - 2au + \beta^2) \times (n^2 - 2au + \gamma^2)$, &c. or $2 \times (u^2 - 2au + \beta^2) \times (u^2 - 2au + \gamma\ )$, &c. according as n is an odd or an even number.

LXXVII. An Extraordinary Disease of the Skin, and its Cure. Extracted from the Italian of Carlo Crusio. By Rob. Watson, M.D., F.R.S. p. 579.

A young woman of 17, the daughter of a citizen of Naples, was brought to the royal hospital June 22, 1752, and was placed in one of the wards assigned to the care of Dr. Crusio; who was informed by her, that her complaint was an excessive tension and hardness of the skin over all her body, by which she found herself so bound and straitened, that she could hardly move her limbs. He found her skin hard to the touch, like wood, or a dry hide; however, he observed some difference in the degrees of the hardness; for in some places it was greater; as in the neck, forehead, and particularly in the eye-lids; so that she could neither raise nor entirely shut them. It was also very great in the lips, tongue, and on each side of her body; but the muscles under the skin seemed not to be affected, because the joints could be bent; and if in any place there was any difficulty in moving the limbs, this arose not from any defect in the muscles, but from the hardness and tension of the skin and cellular membrane, which did not yield to their contraction and relaxation. Her skin had lost its natural warmth, but was sensible, when it was pressed by the nails, or a pin; the patient then saying, that she felt a pain, as if the skin were tearing. Her pulse was deep, and obscure; but equal, and regular. Her respiration was free, and uninterrupted; her digestion good, and she found no inconvenience after eating, except a greater constriction round the belly. The alvine excretions were easy and proper; but the urinary sometimes exceeded the quantity of what she drank, and appeared loaded with salts; both which circumstances perhaps proceeded from the sensible and insensible perspiration being entirely wanting. For she never sweated, though ever so much exercised. Her sleep was natural; she had never had the menstrual evacuation. Her disorder began first in her neck, when she could not move it as usual; then she found the skin of her face and forehead grow hard; and so successively, she found all the external parts of her body grow hard, and tense. She never had had any other disease, except a little fever some years before.

Respecting the indication of cure of this extraordinary disease, as the skin was observed to have lost its natural softness and flexibility; and to have become hard, contracted, and imperspirable; it was concluded that the immediate cause of such a morbid change was a preternatural contraction of the nervous or fibrous parts of the skin, by which its excretory ducts and exhaling vessels were constringed, and did not supply a due quantity of the oily and aqueous fluids necessary to soften and lubricate the parts. Hence it was thought fit to put the patient into a bath of warm milk and water, and to direct her to stay in it a considerable time, that the warmth and moisture might relax and soften the hardness of her skin: but she could not bear to continue in the bath, on account of the great oppression which it occasioned, and because the troublesome constriction of her skin was much increased by it. She was therefore put to bed, and well covered with clothes, in hopes to promote a sweat, but all was in vain; for her skin remained as hard and dry as before. However, this treatment was repeated for 6 days; but, on going into the bath for the 7th time, she was seized with convulsions in the muscles of her legs and arms. This was very unexpected, and made it necessary to discontinue this method of cure. But as it was imagined that it was the weight and pressure of the water which gave her so much uneasiness, a method was thought of to avoid this inconvenience, and at the same time to procure for the patient the benefit, that might arise from the relaxation and softening of the skin and pores by the absorption of an external humidity, which was judged to be necessary to the cure. Now the vapour of warm water has a great power of insinuating itself into the pores, and between the fibres of bodies; and, by that means, of relaxing and softening the hardest substances, as is observed in dry leather; which, suspended in the steam of boiling water, becomes much more soft and pliable, than if it had been immersed for a longer time in the hot water itself. A vapour bath was therefore ordered, and contrived in such a manner, that the steam of the boiling water might entirely surround the body of the patient, or be directed to any particular part, as occasion should require. She bore the vapour without any inconvenience, and was constantly kept in bed in the intervals between the several applications of it. The 6th time of using this kind of bath, she began to perspire a little, and from day to day the perspiration became more general, and at last universal: then the skin began to be less rough, but not less hard; and the urine was more thin and diluted than before. Her diet was prescribed to be of the most soft and relaxing nature, and principally consisted of whey. As she was judged to be of too full a habit, and as she had not the regular menstrual discharge, she was ordered to lose 12 oz of blood from the foot; and it was thought that this evacuation might contribute to produce a general relaxation, and by consequence make the circulation of the blood, and other fluids, more free through their respective canals. It was surprising to see what difficulty the surgeon found in opening the vein, on account of the hardness of the skin; insomuch that, in the operation, the lancet yielded, and bent. However, at last it pierced the skin and the vein, but not without much pain. The blood issued forth with impetuosity, and the wound was some time before it healed; but at length it formed an elevated and hard scar.

By continuing the emollient diet and vapour bath, in about 40 days the skin of her legs, where the hardness appeared the latest, began to soften. But as often as she exposed herself to the fresh and cool air, the skin, which had begun to be soft and flexible, was observed to become hard again, and imperspirable. It was therefore thought proper, towards the end of September, to place her in a warm room, where the air was kept of an equal degree of heat. This had the desired effect: for by staying in her room, and from time to time repeating the vapour bath, and by drinking, at her meals, a decoction of the woods, the perspiration was constant and moderate; and the softness of the skin, which began in the legs, extended itself upwards, and was in some degree perceptible in the arms.

Five months had elapsed since the beginning of this treatment, when it was believed that, without some more efficacious medicine, capable, by its motion, weight, figure, and divisibility, of circulating with the blood, and of penetrating into the most remote and subtil recesses of the vessels, it would be impossible to open the obstructions, which were formed in the vascular structure of the skin, and which, by hindering the fluids from circulating through their respective canals, had deprived them of that humidity, which nature has made necessary for their flexibility and softness. It was therefore thought proper to make her take small doses of pure quicksilver; and that the mercury might the more easily be determined to the skin, the patient was ordered to be constantly kept in a warm air, to have the surface of her body rubbed with a flannel, and to continue the use of the vapour bath. But, by way of preparation for this mercurial course, she was gently purged, and blooded a second time, that the plenitude being diminished, the mercury might better circulate through the finest vessels. Here it is to be observed, that the surgeon, in this 2d blood-letting, did not meet with that resistance, in piercing the skin, which he had experienced in the first. The patient, thus prepared, began in December, 1752, to take daily 6, and afterwards 12 grs. of pure quicksilver, in a drachm of cassia, drinking after it half a pint of a decoction of sarsaparilla. In this course she continued 4 months with chearfulness, and without any inconvenience; and within 2 months from the beginning of it there appeared a somewhat viscid sweat, and the skin grew more flexible, and yielding. About the end of March, 1753, she had an efflorescence over all her skin, which by degrees became pustular, and was very troublesome by its heat and itching. The use of mercury was then discontinued, and she took no medicine but half a pint of an infusion of sarsaparilla in the morning, and an emulsion of melon and poppy-seeds in the evening. Then the heat and itching abated, and the pustules suppurated. Signor Crusio says, that he had the pleasure to see many small globules or particles of mercury separated in the ripe pustules. This is something so unusual and surprising, that we shall scarcely be inclined to give our assent till we are forced to it by further experience and observation; especially as we know that the most careful and sensible men are often mistaken; but that it is very rare that any thing happens out of the ordinary course of nature.*

About the middle of May following, her skin was quite clear of pustules, and was become perfectly soft and flexible, being capable of being moved, raised, extended, and of performing all its natural functions. This softness and flexibility of the skin was general, except in the forehead and lips; which however afterwards recovered their natural state. But there still remained an unusual degree of tension in some of the muscles, which lie immediately under the skin, particularly in those of the hand and radius; on which account, a milk diet was prescribed, to supply the blood with a proper matter for filling the cells of the adipose membrane; which membrane, by having sustained a long pressure between the diseased skin and muscles, was become deprived of its proper mucilaginous and oily juices, designed by nature to keep the parts soft and flexible, and to facilitate the motion of the muscles.

LXXVIII. Experiments on the Use of the Agaric of Oak in Stopping Hæmorrhages.—— I. *The Event of Experiments made by Agaric on the Amputation of the Legs of 2 Women in Guy's Hospital. By Mr. Samuel Sharp, Surgeon of that Hospital, and F.R.S.* p. 588.

The styptic powers of the agaric were tried on 2 women, whose legs were amputated below the knee. One of them was 62 years of age, and had been very much impaired by a long illness, and continual pain. During the operation she bled with great impetuosity; and it was with difficulty, that the hæmorrhage was stopped, notwithstanding Mr. S. pressed the agaric, with all his force, against the extremities of the *tibialis antica*, and *tibialis postica*, the 2 largest arteries. The tendency to bleed, after the operation was such, that Mr. S. found it necessary to apply the tourniquet, and keep a tight stricture on the femoral artery. She complained grievously of the pain arising from the stricture; on which it was a little loosened, and soon after a hæmorrhage ensued from one of

* This remark seems to have been introduced into the text by the editor of the original Transactions.

the 2 large arteries, which was immediately taken up, and tied with a needle and ligature. In order to discover this vessel, the agaric was removed; and though the tourniquet was quite slack, the other large artery did not bleed one drop. This happened about $1\frac{1}{2}$ hour after the operation. After the vessel was tied, the same agaric was again laid on the same part, without screwing the tourniquet, and the patient became much easier; but, in about $3\frac{1}{2}$ hours, the other large vessel burst open; and though assistance was on the spot, and it was immediately tied up, she was so exhausted by the sudden loss of blood, that she died in about twenty minutes. It is conjectured, that, by the 3 hæmorrhages, *viz.* the first during the operation, and the 2 after the operation, she lost between 20 and 30 oz. of blood. Mr. S. examined the limb after death, but found no singular appearance in the vessels, or the adjacent parts.

The other woman was 24 years of age. She lost very little blood in the operation, and had continued extremely well ever after. The agaric seemed in this instance, to have answered the most sanguine expectations. The following are the particulars of this case, as related by Mr. Warner:

II. *The History of a Case relating to the Effects of the Agaric of Oak in Stopping Hæmorrhages. By Joseph Warner, Surgeon to Guy's Hospital, and F.R.S.* p. 590.

Saturday, December 9, 1752, Catharine Spong, aged 24, had her leg amputated, about 4 inches below the knee, at 12 o'clock to-day, on account of an incurable ulcer, with which she had been afflicted for 13 years. She lost very little blood by the operation. Immediately after the amputation, a piece of agaric, of a proper size was applied to the mouths of the principal arteries. Two other pieces of agaric were applied to the mouths of 2 smaller arteries, which appeared at some distance from the principal ones. On the pieces of agaric, dossils of lint were applied, and over all a pledgit of tow spread with the common digestive; all which were kept on by the common bandages made use of in the like cases, and applied with the same degree of tightness as usual.

For an hour and a quarter after the operation, the ligature and tourniquet were kept on moderately tight, at a convenient distance above the knee, at the end of which time it was slackened, so as to have no degree of pressure on the femoral artery, as the dressings and rollers appeared very little tinged with blood. The patient was much easier than Mr. W. had ever observed, after the use of the needle and ligatures. Her pulse appeared very little disturbed till about 4 o'clock this afternoon, when the symptomatic fever began to come on, attended now and then with convulsive twitches of the stump, and thigh; for which reasons, the ligature was somewhat tightened. At 7 o'clock this evening the ligature and tourniquet were quite loosened; soon after which, the convulsive twitches became less frequent, and less severe. These convulsive twitches she had been long used to, and, by her own account, they were more severe before the operation, than they have been since. She had but little rest to night.

Sunday morning, at ½ after 10 o'clock, she appeared as well as could be expected, her pulse was calm, and she had no particular complaints. At 12 o'clock at night she fell asleep, and so continued till after 7 o'clock the next morning.

Monday morning she appeared well, her pulse was calm, and she had no particular pain. Monday night she slept but little, but was very easy the whole night.

Tuesday morning she appeared well, her pulse quiet. That morning at 11 o'clock, she was dressed in the usual manner: the wound had a very good aspect; she had suffered no particular pain in the parts where the agaric was applied, and was, in all respects, as well as could be expected. At 7 o'clock in the evening she was perfectly easy; the convulsive twitches, which she at first complained of, were then quite removed.

Wednesday morning, she continued well, and perfectly easy; had no return of her convulsive twitches, nor was there any appearance of blood through the rollers, or dressings.

Thursday, Dec. 14, she continued very well. Her wound was dressed that morning, at ½ after 11 o'clock, when there appeared a very proper discharge of matter, not in the least tinged with blood. The whole of the agaric, with the rest of the dressings came off, without giving pain. She had the day before 2 or 3 convulsive twitches of the stump, and thigh, but they were slight. Her pulse was good.

II. *A short History of the Effects of the Agaric of the Oak in Stopping Bleedings after some of the most capital Operations in Surgery; with an Account of the Manner of its acting on the Vessels. By Joseph Warner, F.R.S. and Surgeon to Guy's Hospital.* p. 593.

The success which attended the application of the agaric in the instance of the young woman, the particulars of whose case have been stated in the preceding paper, induced Mr. Warner to try its effects in 4 other cases, the histories of which are as follows:

Case 1.—Jonathan Lee, aged 51, had his leg cut off, below the knee, on the 7th of May, 1754. He was extremely reduced, in consequence of the disease; and the whole mass of blood was become so much impoverished, and altered from its natural state, as to appear like serum, both in texture and colour. During the operation, the screw-tourniquet was applied to the thigh with a degree of tightness sufficient to prevent the course of the blood.

Immediately after the amputation, bits of agaric were applied to the mouths of the vessels, and on them soft layers of lint; all of which were covered with a pledgit of tow spread with digestive, and were properly secured on by the common bandage. About 3 or 4 minutes after he was rolled up, and put to bed, Mr. W. discovered the blood to discharge freely through the dressings; on which, he tightened the tourniquet, in expectation of stopping the bleeding, but it appeared evidently to increase it. Seeing this uncommon effect, Mr. W. quite slackened the tourniquet; on which, the bleeding immediately ceased. This he was led to from a supposition, that the veins had probably suffered so great a compression from the instrument, as to be incapable of returning that blood, which was carried to the neighbouring parts by the collateral arteries arising from the principal trunk above the ligature. But whether this was the true reason, or not, he would not take upon him to determine: however the fact was, that the bleeding immediately ceased, and did not return again.

The patient was dressed on the 4th day after the operation, and the whole of the agaric was removed. Since that time he had been treated in the common method, without any further use of the agaric. The patient had very little fever, or pain, after the operation. He had a fair prospect of doing well.

Case 2.—Elizabeth Hillier, a very lusty woman, 38 years of age, had her breast cut off on the 7th of May, 1754. The wound was large, and bled freely from several considerable arteries. Mr. W. made use of no other method to stop the bleeding, than the application of pieces of agaric to the mouths of the vessels, which were properly secured on by a flannel roller, after being first covered with lint, and a pledgit of tow spread with digestive. The symptomatic fever was very slight: she had been quite free from those painful spasms which constantly arise from the use of the ligature: there had not been the smallest loss of blood since the operation.

Her wound was dressed on the 4th day, when the whole of the agaric came away: it was afterwards treated in the common method. She was in a very fair way of recovery.

Case 3.—George Whitmore, aged 12 years, had his leg cut off, below the knee, on the 13th of May, 1754. The agaric and dressings were applied as in the preceding cases, which has answered perfectly well in all respects. The tourniquet was quite removed in 10 minutes after the operation; he had very little fever, restlessness, or pain. His wound was dressed on the 5th day, and the whole of the agaric was removed. He was as well as could be expected.

Case 4.—Richard Barnat, aged 54, had his leg cut off, below the knee, on the 21st of May, 1754. Mr. W. made use of no other methods to stop the bleeding than the agaric, which was applied as in the preceding cases. Immediately after the operation, the patient was put to bed, and the tourniquet let quite loose. He has not sustained the smallest loss of blood since the operation. The pain and fever had been very inconsiderable, and he seemed to be in a very fair way of doing well.

LXXIX. Of a new Pyrometer, with a Table of Experiments made with it. By Mr. J. Smeaton, F. R. S. p. 598.

This instrument is capable of receiving a bar 2 feet 4 inches long, and might be made capable of receiving bars of a much greater length, of some kinds of materials, but not of others, on account of the flexibility brought on them by a degree of heat not greater than boiling water.

The measures taken by this instrument are determined by the contact of a piece of metal with the point of a micrometer-screw. The observation is the best judged of by the hearing, rather than that of the sight or feeling. By this method Mr. S. found it very practicable, to repeat the same measurement several times, without differing from itself above $\frac{1}{20000}$ part of an inch. This principle of determining measures by contact is not wholly new, but has been employed on several occasions, as he was informed by the late Mr. Graham; but the present manner of applying it he believes is so: and the degree of sensibility arising from it exceeds any thing he had met with. As the method will easily appear by the draught (see pl. 9, fig. 10 and 11) he avoids a further description of it in this place.*

As no substance has hitherto been discovered in nature, that is perfectly free from expansion by heat, I chose to construct this instrument in such a manner, that the bar, which makes the basis of the instrument, shall in each experiment suffer the same degree of heat, as the bar to be measured: of consequence, the measures taken by the micrometer are the differences of their expansion. The expansion then of the basis between two given degrees of heat being once found, the absolute expansion of any other body, by adding or subtracting the difference to or from the expansion of the basis, according as the body to be measured expands more or less than the basis, will also be determined.

When the instrument is used, it is immerged, together with the bar to be measured, in a cistern of water; which water, by means of lamps applied underneath, is made to receive any intended degree of heat, not greater than that of boiling, and so communicates the same degree of heat to the instrument, the bar, and to a mercurial thermometer immerged in it, for the purpose of ascer-

* I have lately seen an instrument at Mr. Short's, made by the late Mr. Graham, for measuring the minute alterations, in length, of metal bars; which were determined by advancing the point of a micrometer-screw, till it sensibly stopped against the end of the bar to be measured. This screw being small, and very lightly hung, was capable of agreement within the 3 or 4000th part of an inch.—Orig.

taining that degree. That this may be truly the case, the water should be frequently stirred, that there may be no difference of heat in the different parts of the water: this being done, the height of the quicksilver appearing stationary, the contact with the screw of the micrometer also remaining the same, for a space of time, it is to be supposed, that the heat of the 3 bodies will be the same as the heat of the water, however different they may be in specific gravity, &c. The whole difficulty is now reduced to this problem, viz.

To find the absolute expansion of the basis between any two given degrees of heat, not greater than that of boiling water.

For this purpose, let there be prepared a bar of straight-grained white deal, or cedar, which, it is well known, are much less expansible by heat than any metal hitherto discovered: let the bar be adapted to the instrument in like manner as the other bars intended to be measured; but that the softness of the wood may not hinder the justness of its bearings, let its ends be guarded with a bit of brass let into the wood at the points of contact: to prevent, as much as may be, the moisture or steam of the water from affecting the wood; let it first be well varnished, and then, being wrapped round with coarse flax from end to end, this will in a great measure imbibe the vapour, before it arrives at the wood. Let the cistern also be so contrived, that the instrument being supported at a proper height in it, the bar to be measured may on occasion be above the cover, while the basis remains in the water: thus will the cover also be a defence against the moisture. Let the water in the cistern be now brought to its lower degree of heat, suppose at or near the freezing point, the basis having continued long enough in the water to receive the same degree of heat, and the wooden bar having been previously kept in an adjacent room, not subject to sudden alterations of temperature by fire, or other causes; let the bar be applied to the instrument, and the degrees of the micrometer and the thermometer read off, and set down: let the wooden bar be then restored to its former place, till the water is heated to the greater degree intended, suppose at or near that of boiling water; the lid being now shut down, and the chinks stopped with coarse flax, to prevent the issuing of the steam as much as possible, let the wooden bar be again brought forth, applied to the instrument, and the degrees of the micrometer and thermometer read off, as before: the difference of degrees of the micrometer, corresponding to the difference of degrees of the thermometer, will express the expansion of the basis between those degrees of heat; that is, on the supposition that the wooden bar was of the same length, at the time of taking the second measure, as at the first; indeed a measure can hardly be taken without any loss of time, as the whole of the instrument, when the hot measure is to be taken, is considerably hotter than the wooden bar; and, in case of boiling water, the steam being very repellent and active, the bar is liable to be sensibly affected in its length, before the measure can be taken, both by heat and moisture, which both tend to expand the bar; but as the quantity is small, and capable of being nearly ascertained, a wooden bar, thus applied, will answer the same end as if it was unalterable by heat or moisture. To know therefore the quantity of this alteration, let the time elapsed between the first approach of the bar to the instrument, and the taking of the measure, be observed by a second-watch, or otherwise; after another equal interval of time, let a second measure be taken; and after a third interval, a third; and a fourth; the three differences of these four measures will be found nearly to tally with three terms of a geometrical progression, from which the preceding term may be known, and will be the correction, which, if applied to the measure first taken, reduces it to what it would have been if the wooden bar had not expanded during the taking of it. From a few observations of this kind, carefully repeated, the expansion of the basis may be settled; and this once done, the making experiments on other bars will become very easy and compendious.

The basis of this instrument, as well as other parts of it, is brass. He chose this substance, rather than any other whose expansion was greater or less; because he found, from some gross experiments previously made, that the expansion of brass was nearly a medium between those bodies which differ most in their expansion: a considerable convenience arises from this circumstance; because as the measures, taken in common experiments, are their difference from brass, the dependence on the thermometer will be less, as these differences are less.

The bar of brass which compose the basis, is an inch broad by half an inch thick, and stands edgewise upwards; one end is continued of the same piece at right angles, to the height of $3\frac{1}{2}$ inches, and makes a firm support for the end of the bar to be experimented; and the other end acts on the middle of a lever of the second kind, whose fulcrum is in the basis; therefore the motion of the extremity of the lever is double the difference between the expansion of the bar, and the basis. This upper part of the lever rises above the lid of the cistern, so that it and the micrometer-screw are at all times clear of the water. The top of the lever is furnished with an appendage which he calls the feeler: it is the extremity of this piece which comes in contact with the micrometer-screw. It hence appears, that having the length of the lever from its fulcrum to the point of suspension of the feeler, the distance between the fulcrum and the point of contact with the bar, the inches and parts that correspond to a certain number of threads of the micrometer, and the number of divisions in the circumference of the index-plate; the fraction of an inch expressed by one division of the plate may be deduced; those measures are as follows.

From the fulcrum of the lever to the feeler . . . . 5.875 inches.
From the fulcrum to the place of contact. 2.895
Length of 70 threads of the screw. 2.455
Divisions in the circumference of the index-plate. . . . 100

Hence the value of one division will be the $\frac{10}{57863}$ part of an inch; but if the screw be altered $\frac{1}{4}$ of one of these divisions, when the contact between the screw and feeler is well adjusted, the difference of contact (if he may so call it) will be very perceivable to the slightest observer; and consequently $\frac{1}{23145}$ part of an inch is perceivable in this instrument.

There is one thing still remains to be noticed, and that is, the verification of the micrometer-screw, which is the only part of this instrument that requires exactness in the execution; and how difficult these are to make, perfectly good, is well known to every person of experience in these matters; that is, that the threads of the screw may not only be equidistant, in different places, but that the threads shall be equally inclined to the axis in every part of the circumference.

The result of the experiments made with this instrument, agrees very well with the proportions of expansion of several metals given by Mr. Ellicott; which were deduced from his pyrometer published in the Philosophical Transactions: and, considering the very different construction of the two instruments, they abundantly tend to confirm each other.

References to the Figures.

Fig. 10, pl. 9, represents the instrument independent of the cistern in which it is used. ABCD, is the main bar or basis of the instrument. EF, is the bar to be measured, lying in 2 notches; one fixed to the upright standard AB, the other to the principal lever HI. The end E of the bar EF, bears against the point of G, a screw of use in examining the micrometer-screw. The other end of the bar F bears against a small spherically protuberant bit of hard metal fixed at the same height as G, in the principal lever HI. K, is an arbor fixed in the basis, which receives at each end the points of the screws HL, on which the lever HL turns, and serve as a fulcrum to it. O, is a slender spring, to keep the lever in a bearing state against the bar; and P, is a check, to prevent the lever from falling forward when the bar is taken out. N, is the feeler, something in the shape of a T, suspended, and moveable up and down on the points of the screws IM, which, as well as LH, are so adjusted, as to leave the motion free, but without shake. QR, is the handle of the feeler, moveable on a loose joint at R; so that, laying hold of it at Q, the feeler is moved up and down without being affected by the irregular pressure of the hand. The extremity S of the feeler is also furnished with a bit of protuberant hard metal, to render its contact with the point of the micrometer-screw more perfect. T, is the micrometer-screw; V is the divided index-plate, and W a knob for the handle. The micrometer-screw passes through two solid screwed holes at D and Y. The piece YZ is made a little springy, and endeavours to pull the screw backwards from the hole at D; of consequence keeps the micrometer-screw constantly bearing against its threads the same way, and so renders its motion perfectly steady and gentle. X, is the index, having divisions on it, answering to the turns of the screw. This piece points out the divisions of the plate, as the face of the plate points out the divisors on the index. When the instrument is used, lay hold of the knob at Q with one hand, and, moving the feeler up and down, with the other move forward the screw I, till its point comes in contact with the feeler; then will the plate and index V and X show the turns, and parts.

Fig. 11, represents the instrument immerged in its cistern of water, ready for use. AB, is the cistern; C, the cover; which, when the instrument fig. 10 is raised on blocks, goes on between the bar EF and the basis BC. D, a handle to take off the cover, when hot; E, the mercurial thermometer; F, the cock to let out the water. GH, a hollow piece of tin, which supports seven spirit lamps, which are raised higher or lower by the screws I and K, in order to give the water in the cistern a proper degree of heat.

A TABLE of the expansion of metals; showing how much a foot in length of each grows longer by an increase of heat corresponding to 180 degrees of Fahrenheit's thermometer, or to the difference between freezing and boiling water, expressed in such parts of which the unit is equal to the 10000th part of an inch.

1	White glass barometer tube	100	11	Brass wire	232
2	Martial regulus of antimony	130	12	Speculum metal	232
3	Blistered steel	138	13	Spelter solder, viz. brass 2 parts, zink 1	247
4	Hard steel	147	14	Fine pewter	274
5	Iron	151	15	Grain tin	298
6	Bismuth	167	16	Soft solder, viz. lead 2, tin 1	301
7	Copper hammered	204	17	Zink 8 parts, with tin 1, a little hammered	323
8	Copper 8 parts, mixed with tin 1	218	18	Lead	344
9	Cast brass	225	19	Zink or spelter	353
10	Brass 16 parts, with tin 1	229	20	Zink hammered half an inch per foot	373

It is now several years, says Mr. S. since I first observed the very considerable expansion of the semi-metallic substance called zink, spelter, or tootanag; and proposed it as more fit for the purpose of making compound pendulums, and metalline thermometers, than brass; as its expansion seemed considerably greater, and its consistence, when gently hammered, not much inferior. With the same view I have made trial of several other metallic compositions, besides what is above set down; but they all proved much inferior to zink in expansion, and most of them in consistence.

It seems, that metals observe a quite different proportion of expansion in a fluid, from what they do in a solid state; for regulus of antimony seemed to shrink in fixing, after being melted, considerably more than zink.

LXXX. On the Sex of Holly. By Mr. John Martyn, F. R. S. Professor of Botany in the University of Cambridge. p. 613.

The holly, agrifolium, or aquifolium, is described by all authors as bearing hermaphrodite flowers; but Mr. M. thinks that this tree is male and female in different plants. He had in his garden at Streatham in Surry, 6 pretty large plants, with differently variegated leaves, in full flower, 3 males, and 3 females, growing in pairs, and a male growing by itself, in another part of the garden. The female is that which has been described by authors, and he did not know that any one had described, or even taken the least notice of the male.

The male flower, as well as the female, is monopetalous, cut deeply into 4 segments, with a very small empalement, divided also into 4 parts. It has 4 conspicuous chives, which sustain yellow summits, in which is great plenty of farina; but it has nothing like either stile, or ovary. The female flower has, besides its essential part, the ovary, 4 short filaments, which have hitherto been taken for chives, or male organs of generation; but as he could not perceive that they bear any summit, or yield any farina or fecundating dust, he rather believes that they are tubes, which assist in conveying the impregnating particles to the seeds; which opinion seems in some measure confirmed by the germ being placed in the lower part of the seed, according to Cæsalpinus, who ranges this tree among those quarum semina cor in inferiore parte habent.

Ray has placed it among the arbores flore, fructui contiguo; but Mr. M. thinks it ought to be removed to the arbores flore a fructu remoto. It must also be removed from the tetrandria tetragynia of Linneus to the diœcia tetrandria. But if the 4 filaments in the female flower should be found, on a more accurate observation by better eyes, to be real chives, and to contain a fecundating dust, it will belong to the polygamia. But whether the tree, which he verily believes to be purely female, is really so, or hermaphrodite, he is certain that the other is purely male; and even in this case his observation is new.

Mr. W. Watson's Opinion on Mr. Martyn's Paper on the Sex of the Holly. p. 615.

I first examined, in company with Mr. Miller, the holly trees in the botanical garden at Chelsea. We there found, as Mr. Martyn had, that the flowers were of different sexes; but not as those in the Dr.'s garden, male and female on different plants, but female and hermaphrodite on different plants. I afterwards, both at Hampstead, and at the duke of Argyll's at Whitton, observed several trees bearing male flowers, others female flowers. Hence it appears, that not only Mr. Martyn's observation of the holly being male and female in different trees is well founded, but also that it is male, female, and hermaphrodite, on different trees; and I should not wonder, if on a still further examination, as in the mulberry, that the male and female flowers of the holly should be found, not only on different, but on the same tree: or even, as in the empetrum, or berry-bearing heath, that some holly-trees should be found bearing only male flowers, others bearing only female flowers, others only hermaphrodite flowers, others both male and female, others both male and hermaphrodite, others female and hermaphrodite, others still bearing flowers male, female, and hermaphrodite on the same tree. The holly therefore, as Dr. Martyn has justly observed, should be removed, in the system of Linneus, from the tetrandria tetragynia; but not to the diœcia tetrandria, but rather to the class polygamia, and to the order trioicia.

LXXXI. A Continuation of the Account of the Weather in Madeira. By Dr. Thomas Heberden. p. 617.

This paper contains first a medium of the greatest, least, and mean height of the barometer and thermometer, at Funchal in Madeira, for each month of the years 1751, 1752, 1753, which have but very small differences and changes.

By collecting the respective sums of the daily heights of the instruments throughout the year, and extracting the mean altitude, it is found that the mean altitude of the barometer for each day, is 29.915 inches, and of the thermometer, 68°.918. The greatest barometrical variation, during 4 years and 4 months, has been $\frac{9}{10}$ of an inch only, viz. from 29.3 to 30.2. The greatest thermometrical variation, during the said time, has been 20°, viz. from 60° to 80°; but it may be observed, that it never rose so high but once; occasioned by a very strong léste or levant wind; the extreme height, without such an accident, being never more than 78°.

The quantity of rain which fell in the 7 years, from 1747 to 1753, inclusive, amounts to 214.346 inches. Therefore the mean quantity for each year is 30.62 +.

LXXXII. On Father Kircher's Opinion concerning the Burning of the Fleet of Marcellus by Archimedes. By James Parsons, M. D., F. R. S. p. 621.

Dr. P. says, that though the machines invented by Archimedes when Marcellus besieged the city of Syracuse, as described by Livy, Plutarch, and Polybius, were wonders, surpassing the comprehensions of the generality of mankind, yet what was most discredited, was Archimedes's setting fire to the ships, by a burning speculum. Indeed so distinguished a genius, if he could not destroy them in that manner, must know, that he might have thrown combustible matter, sufficient to burn the galleys, from his projectile machines; for we cannot imagine that he was ignorant of every kind of these, and not even of the wildfire of the Greeks. But, however, to account for his burning the fleet, by a speculum, was the difficult point.

When philosophers began to increase their catoptrical experiments, which they did very early, they found the focus, of every speculum that was concave, so short, that they were easily inclined to conclude that Archimedes could not set fire to the fleet by a speculum; and hence the fact became entirely discredited, till Kircher, and his pupil Schottus, whose characters and works the learned world is well acquainted with, resolved to consider not only the story of Archimedes, but also that of Proclus, who is said to have destroyed a fleet at Constantinople in the same manner. Kircher however, notwithstanding the incredulity of the learned of his time, was not deterred from giving attention to the matter himself; which led him to make innumerable experiments, to see whether it were possible to be done or not, before he would give any opinion about it; and at length, when he had commended the parabolical speculum, which he and others were inclined to think the most likely to succeed in such an enterprize; he was inclined to think that Archimedes made use of such a speculum.

But soon afterwards he became dissatisfied with this notion, and beginning to make new attempts, he fell upon one which lessened his former good opinion of the parabolical speculum, and made him more sensible of the inconveniencies attending it, or those of any other form, that had any great degree of concavity; and in short engaged him entirely in favour of his new thought, which was put in execution in the following manner:

He erected a frame, on which he placed 5 plane specula, of equal given dimensions, with such inclinations as made them all throw their reflected rays on the same place, at more than 100 feet distance. When he had set the first speculum, he went and laid his hand on the place where he caused the rays to fall, and found it warm; when he added those of the 2d, the heat was doubled; the 3d increased the heat in the same proportion; and the 4th being added, the heat was scarcely to be borne; but the 5th made it intolerable. Whence he concludes, that, by multiplying those specula, the heat might be so increased, as to set fire to combustible matter at greater distances, according to the number applied.

Schottus gives the same account of Kircher's experiment. He accompanied him in all his trials, as well as in his journey to Syracuse, after he had brought his plane mirrors to answer his purpose; and, on viewing the place, they both concluded, that the galleys of Marcellus could not be farther than 30 paces from Archimedes. And yet Schottus declared, that if a concave speculum could be constructed, as large as the rotunda, it could not have a sufficient focus to effect what both Archimedes and Proclus are said to have done.

Thus we see Kircher had scientifically established the problem, for the construction of a burning machine, consisting of any number of plane specula; which was afterwards further confirmed by Buffon, as appears in 2 letters; printed in these Trans.* If so, we cannot suppose he could have seen what either Kircher or Schottus had written about it.

LXXXIII. On several Bones of an Elephant found at Leysdown in the Island of Sheppey. By Mr. Jacob, Surgeon at Feversham. p. 626.

Three or 4 years before Mr. J. had sent the acetabulum of an elephant, which was discovered sticking in the clay, which was partly washed away from the cliff,

* See page 344 of the 9th vol. of these Abridgments.

at Leysdown, in the isle of Sheppey, a mile eastward of the cliffs of Minster. This, with other parts, as one of the spinal vertebræ, a thigh-bone 4 feet long and numberless other fragments, too rotten to be then taken up entire, he saw; all which lay below high-water mark: and as the place, and some adjacent land, soon after, became his property by a purchase he made, he went, attended by workmen, in search of more, and found an elephant's tusk; and, as it lay entire to appearance, took its dimensions; which were, in length, 8 feet; and in circumference, in the middle, 12 inches: but it fell all to pieces, when they endeavoured to raise it. He found also part of a scapula, its sinus almost entire, and 3 inches diameter. He found also some pieces of grinders. The pyrites however abounds so much in the clay where this animal was embedded, that he despaired of finding any whole bones: but he thinks these fragments are sufficient to show, that the elephant was as large as that mentioned by Tentzelius, in these Transactions.

The apex of the tusk, which Mr. J. preserved, together with the acetabulum, were both found within 20 feet of the other bones mentioned, and were, as Mr. J. apprehended, in better condition then than at the above date, from their being taken up immediately on being discovered, and not left to be exposed to the injury of the weather, and violence of the tides; which soon affects bodies so exposed, after having lain under ground for ages.

LXXXIV. On the Animal Life of those Corallines, that look like Minute Trees, and grow upon Oysters and Fucuses all round the Sea-coast of this Kingdom. By Mr. John Ellis. p. 627.

The doubts still remaining on the minds of many learned men, of the animal nature of corallines, on account of their beautiful ramifications, and regular plant-like appearances, determined Mr. E. to persuade Mr. Ehret to accompany him to the sea-side, that he might there be an eye-witness of what he had advanced, and to make exact drawings of the several different objects, as they appeared to him through the microscope.

Accordingly, June 3, 1754, they set out, and arrived at Lewes in Sussex that evening, and the next morning at Brighthelmstone. The weather being very calm, and few fucuses or corallines being thrown ashore on the beach, they hired a fisherman, the next day, to take up some oysters from an old oyster-ground, that had been long disused, lying about 3 or 4 leagues off to sea, and where, by his description, the shells were covered with great varieties of these minute tree-like corallines; with directions that, as soon as he took them out of the sea, he should immediately put them into a bucket of sea-water; but unfortunately he put the oysters into a fisherman's basket; by which means many varieties were dead, though they received them 2 hours after they were taken out of the sea, and had them put immediately into sea-water: however, by the oysters lying on each other, some of the corallines were kept so moist, as to be perfectly alive and brisk. In order to distinguish them more easily, they plucked them off the oysters, and placed them in white earthen plates, and poured as much sea-water over them as would just cover them. After letting them rest for a little while, to recover themselves, they could easily discover, with a magnifying glass of an inch focus, which were alive, and which not: accordingly, Mr. E. cut off small pieces of several of the liveliest, and placed them in watch-glasses filled with sea-water; these, after resting a little while, he placed, one after another, on the stage of the microscope. The unusual sight so amazed his friend (who had his doubts), that he could scarcely believe his own eyes; for he had hitherto imagined, with many others, that these corallines were vegetables, and only the receptacles of animals, as many other plants are, and not the proper cases, skins, or coverings, of their bodies.

The first coralline* that offered itself to their view, was N° 1, pl. 12, where it is represented, in its natural appearance, climbing on the podded fucus a, with irregular thread-like ramifications, as at b; one of which is exhibited magnified at A, in which is observed a broad dark line in the middle of the transparent stem and branches. This is part of the tender body of the animal, and seems as a support for its several heads and stomachs, with the many hands or claws belonging to each: for at the top of each of the branches we may observe a polype with 20 tentacula, or claws, which do the office of hands, its mouth being in the centre of them, and its stomach underneath, inclosed in a fine transparent cup. The fine outlines represent the horny skin, or outer coat, that serves this compound animal as a defence, in the same manner as the shells of testaceous or crustaceous sea-fish. The skin or covering of the arms, that support the cups, is formed in small rings, which gives the animals the more freedom to move about dextrously in seizing their prey.

At letter B is the microscopical representation of a still smaller coralline † than the former; the size of it a little reduced is expressed at fig. 2. This creeps up, and twines round other corallines by small vermicular tubes, and sends out its curious slender arms irregularly: these arms, in the microscope, look like rows of the smallest beads of a necklace: to the top of each of these is fixed a cup, for the reception of the polypes, the brim of which is curiously indented. These they saw alive, and extending themselves about in various directions.

Fig. N° 3, represents part of another coralline,‡ just as it appeared expanded in a plate of sea-water. It is called, in Ray's Synopsis, ed. 3, corallina ramosa cirris ob-

* Sertularia geniculata, Linn.
† Sertularia volubilis. Linn. Gmel.
‡ Sertularia antennina. Linn.

sita; and by Doody, in Ray's Synopsis, ed. 2, fruticulus elegans geniculatus cirris obsitus. Letter c expresses a branch of this coralline magnified; where you may observe, on each capillary side-branch, rows of small polypes, each with 8 tentacula, or claws, rising out of little sockets. The upper division or tube of these little branches, as at b, appears full of joints, one to each polype; but they could easily perceive that all the polypes were connected together, and communicate with the principal stem, or body, which is inclosed in the middle tube. The under small tube of the capillary side-branch at c, which runs parallel with the upper one b, and adheres to it, appeared clear, hollow, and jointed.

This coralline arises from a tuft of small irregularly-matted tubes, like a sponge growing to an oyster-shell, as at g; the smaller branches e are inserted in circles round the larger branch f, at equal distances, like the plant called horsetail, or equisetum. As they were observing these corallines, they perceived, on one of them, a different-shaped polype, which pushed itself out of a small funnel-shaped pipe: this was inserted in a cell, whose brim or border was surrounded by little spines. These cells composed that spongy rough matter, which incrusts almost all marine substances, but chiefly fucuses. Fig. 4 represents these cells on a fucus; letter D expresses the cells and polypes, with 12 tentacula to each, as they appear magnified; where the animals are seen raising and expanding themselves. When they are disturbed, they draw themselves within their sheath or pipe, which closes on them, and sink together into their cells.

The curious denticulated coralline* at N^{o} 5, has very much the appearance of a plant, at first view, even when it is magnified, as at E. This gave a further corroborating proof, that these extraordinary species of beings are animals: for they observed that the smaller polypes, that extend themselves out at the opening of every opposite denticle, or little projecting tube, are united at the bottom, or lower part, to the fleshy substance of the main body, that passes through the middle of each branch, or stem, and are so many different bodies united in one; acting like so many sets of hands, placed in form of a circle, collecting food, each for a mouth in the centre, to convey nourishment to so many stomachs, which are fixed in the swelling part, or bottom, of each denticle. This great supply of nourishment from all sides, gives that great increase, and variety of ramifications, to this wonderful class of many-bodied animals.

Besides these small polypes, which compose the branches, these corallines send forth, from several parts, many vesicles, of different shapes, at certain seasons of the year, according to their different species. These vesicles are protruded from the outer skin or horny covering of these branched polypes, and from the

* Sertularia rosacea. Linn.

inner or fleshy part arises a large polype; one of which occupies each of these vesicles.

Thus a coralline full of vesicles looks like a plant full of blossoms, which, after they have arrived at their perfect state, fall off, with their capsules or vesicles, and become new-detached animals, to provide for themselves; in the same manner as the falling seeds produce other plants.

On examining this coralline, they found that the animals in the vesicles were dead; but immediately afterwards they had an opportunity of discovering the vesicular polypes alive, in another coralline;* which are described at fig. 6, and at letter F as they appeared magnified. This species Mr. E. called the sea-oak coralline, from its being most frequently found creeping on, and adhering to the largest species of the quercus marinus, or sea-oak fucus.

The vesicles of the denticulated coralline, letter E, are described as they appeared full of spines at the top, and closed up, as at letter G. The vesicles of the same species are more frequently found as described at I, where the spines are not unfolded: from this appearance, he called it the pomegranate-flowering coralline, because they nearly resemble the opening blossom of the balaustine, or double flower of the pomegranate.

The branches of this coralline are often observed to end in vermicular tubuli, as at H, which are much of the same form with those it begins with; so that these animals can, and do, change their shapes, for the several ends and purposes of their being; and this in a most surprising manner.

He had further an opportunity of examining some of those kind of corallines, which he called celleferous, from their having rows of cells disposed in plant-like ramifications. The small black spots in each cell, which he had conjectured before to be the embryo of a future testaceous animal, (Vid. Phil. Trans. vol. 48, tab. 6, p. 115) he found now to be the contracted bodies of dead polypes; for they here saw some of these polypes† alive, and extending themselves out of their cells, as at K, fig. 7; and on reviewing them, when they were dead, found they made the appearance of blackish spots in each cell, as at L, fig. 7. So that they had reason to suppose that this species of polypes that form these corallines, do change into testaceous bodies.

LXXXV. Extract of a Letter from Camillo Paderni, Keeper of the Museum Herculaneum, to Tho. Holles, Esq. Dated at Naples, April 27, 1754. p. 634.

The place where they are digging at present, is under Il Bosco di Sant' Agostino, but a little distant from the royal palace at Portici. Its depth is 125 Nea-

* Sertularia pumila. Linn. † Sertularia scruposa. Linn.

politan palms,* one of which is more than the mercantile canna of Rome. All the buildings discovered in this site are noble: many of the pavements are of mosaic, variously and finely made; others are of different-coloured marbles, disposed with a beautiful symmetry; and most of them are already taken up. In one of these buildings there has been found an entire library, composed of volumes of the Egyptian Papyrus, of which 250 have been taken out; and the place is not yet cleared or emptied, it having been deemed necessary to erect props first, to keep the earth, which lies above it, from falling in upon it. These volumes of Papyrus consist of Latin and Greek manuscripts; but from their brittleness, occasioned by the fire and time, it is not possible to unroll them, being now decayed and rotten. There have been found some of those small tables, which they covered with wax and the palimpseston, and then wrote on them with the stylus: but all these are become a kind of cinder; and have also suffered by the damps; from both which circumstances they are now so tender, that they break with the touch.

In the same place there have been found 3 small busts; one of Epicurus, another of Zeno, and the third of Humachus; with the names of each inscribed on the basis, in Greek letters. A little distant from the preceding site has been discovered another noble building, with a square court belonging to it; the inside of which alone has been hitherto examined. This square is formed with fluted columns made of brick stuccoed. In the angles were 4 terms of marble, with busts on them, in bronze, of the finest manner, having the name of the Greek workman on one of them. In the centre, between the terms, was a small fountain, formed by a vase shaped like a cockle-shell, and supported by a small fluted column. There have been also found 3 other busts, large, and in bronze, likewise of the most excellent workmanship. Within these few days the following things have been taken out of the same site; viz. a female statue, 6 palms high, perhaps a goddess, though without any attribute, and but of middling workmanship; 2 most beautiful candlesticks, 6½ palms high, exquisitely wrought in chased work; other candlesticks, much damaged by the fire and time; many fragments in bronze, which, not having any particular merit, it is needless to describe, except two small figures of fawns, that are finely executed. In the same place was discovered a large fountain, lined throughout with lead: round it were 11 heads of lionesses, out of which the water flowed. Pipes of lead are very often met with; and scarcely a day passes but something is brought.

LXXXVI. Experimental Examination of a White Metallic Substance said to be found in the Gold Mines of the Spanish West-Indies, and there known by the

* A Neapolitan palm is said to be 11⅝ inches English.—Orig.

Appellations of Platina, Platina di Pinto, Juan Blanca. By William Lewis, M.B., F.R.S.* p. 638.

PAPER I.

Exper. 1.—The substance brought into England under the name of platina appears a mixture of dissimilar particles. The most conspicuous, and by far the largest part of the mixture, are white shining grains, of seemingly smooth surfaces, irregular figures, generally planes with the edges rounded off. On examining these with a microscope, the surface appeared in some parts irregular, the prominencies smooth, bright, and shining; the cavities dark-coloured and roughish. A few of them were attracted, though weakly, by a magnetic bar. These grains are the true platina. The heterogeneous matters intermixed among them, in the several parcels, were,

1. A blackish dust, separable by a fine sieve. This was further divided, by a magnetic bar, into 2 different substances: the part attracted was of a fine sparkling black colour, much resembling the black sand from Virginia: the part not attracted was of a dark brownish hue, with several bright moleculæ, which appeared to be fragments of the grains of platina. 2. Among the larger grains, separated by a coarse sieve, were sundry irregular dark-coloured particles, some blackish, others with a cast of brownish red, in appearance resembling fragments of emery or loadstone. Several were attracted weakly by the magnet. 3. There were a few rough yellow particles, resembling gold, which on further examination they were found to be, though probably not entirely free from platina. 4. A few globules of quicksilver, containing gold, with some particles of platina intermixed and pretty strongly adhering. 5. Some fine transparent particles, probably spar. 6. A very few irregular particles, of a jet black colour. These broke easily, and looked like the finer kinds of pit-coal. Laid on a red-hot iron, they emitted a yellowish smoke, and smelt like burning coal. †

* This memoir, with its continuations in the 50th vol. of the Phil. Trans. is one of the most valuable chemical papers which had hitherto been presented to the R. S. Some years afterwards, these papers were reprinted by Dr. Lewis, accompanied with an account of all that was then known respecting platina, in his Commercium Philosophico-Technicum, a work containing much useful information, and, at the same time, suggesting various improvements in the arts connected with chemistry.

Beside these, Dr. L. published a Course of Practical Chemistry; a translation, with notes, of Neumann's Chemistry; a New Dispensatory; and an Experimental History of the Materia Medica. The last 2 works are of the highest merit in their kind, exhibiting (to use the words of an able critic on this subject) correct descriptions of drugs, with useful experiments in their treatment by different menstruums, while the author is very chaste in ascribing virtues, and in repeating from former writers. And from his own experience, as well as that of the most skilful London practitioners, he gives a sounder judgment of the real virtues of medicinal substances than had been given before.

When Dr. Lewis died, or what was his age, at the time of his decease, or any other particulars concerning his life, we have not been able to learn.

† From the experiments of later chemists, it appears that the ores of platina contain several distinct

Remarks.—1. It appears from the foregoing observations, that this mineral has not come to us in its native form; being probably taken out of the mines in large masses, which have been broken, and treated with mercury, to extract the gold, of which possibly it at first contained a considerable quantity. The quantity left by the workmen is extremely small; some pounds of the mixture having yielded only a few grains. A moderate fire renders more of these golden particles discoverable, than can be seen at first; the mercury evaporating, by which several of them were concealed. 2. Some part of the brownish powder is probably adventitious, as well as the mercury; being worn off from the stampers and mills employed for comminuting the mineral, and triturating it with the mercury. 3. The roughness and dark colour of the cavities of the grains of platina, seem to proceed from a substance similar to the black dust adhering in them. It is probably owing also to this heterogeneous magnetic matter, that some of them are attracted by the loadstone.

Exper. 2.—Some of the purer grains of platina, by gentle strokes of a flat hammer, on a smooth anvil, bore to be considerably flattened, without breaking or cracking about the edges: some quickly cracked, and discovered internally a close granulated texture. All are reducible, by rude strokes in an iron mortar, though with difficulty, into powder. They seemed to be rather more brittle when ignited, than when cold.

Exper. 3.—The specific gravity of platina, with its heterogeneous admixtures, as brought to us, was found to be to that of water, as 16.995 to 1.000. The quantity weighed for this purpose was no less than 2000 Troy grains.

The larger grains of platina, separated as much as possible from the other matters by the sieve, and cleansed by heating, boiling in aqua fortis, mixing them with sal ammoniac, and forcing off the salt by fire, and afterwards washing them; weighed in air 642, in water 606.75: whence their gravity turns out 18.213. The microscope still discovered a considerable portion of blackish matter in their cavities. These trials were several times repeated on different parcels of platina: the result was nearly the same in all.

Remark.—The gravity of this mineral, great as it appears to be from the foregoing experiments, would probably turn out still greater on a further purification of the platina, since it is manifestly mixed with some of the lighter heterogeneous matters.

Exper. 4.—A quantity of platina, containing its usual admixture of magnetic dust, was kept for some time of a moderate red heat in an iron ladle. The bright particles became somewhat duller coloured; the magnetic ones were no

metallic substances. See Wollaston in Phil. Trans. for 1804 and 1805, as before quoted at p. 103 of this vol. of these Abridgments.

longer attracted. In other respects there was no sensible alteration. 2. An oz. of platina was urged with a strong sea coal fire, in a blast-furnace, for above an hour: the heat was so vehement, that the black-lead crucible vitrified, and the slip of Windsor brick, which covered it, melted and ran down. The grains of platina were found superficially cohering into a lump, of the figure of the bottom of the crucible, of a brighter colour than at first. On a slight blow, they readily fell asunder again, and seemed not to have altered their shape. 3. In several repetitions of the experiment, the platina began to cohere in a moderate white heat: the grains were at this time very easily separable, and seemed to cohere the more strongly in proportion as the heat was raised. In the most intense fires, which the common vessels could not long support, the platina did not melt, or soften, or alter its figure, or lose sensibly of its weight. The colour was constantly brightened by a strong heat, and generally rendered dusky by a small one: on quenching it, when violently heated, in cold water, the grains, which composed the internal part of the lump, acquired a violet or purple colour.

Exper. 5.—1. As the power of fire on metallic, as well as earthy substances, is remarkably promoted by the immediate contact of fuel, and the impulse of air on the subject; platina was exposed to its action in those circumstances. A crucible, having a bed of charcoal in it, was laid on its side, in a good blast-furnace, with its mouth towards the nose of the bellows; and 4 ounces of platina spread on the charcoal. The fire was vehemently urged for above an hour, during which an intense white flame passed through the crucible, and issued at an aperture made for that purpose. The crucible was vitrified: the grains of platina only superficially cohered, and became brighter, as in the preceding experiment, without seeming to have softened or altered their shape. 2. The experiment was several times repeated, and varied: once, common salt was thrown on the fuel before the crucible, and its fumes strongly impelled on the platina: some platina was likewise placed before the nose of the bellows in violently-excited sea-coal fires, so strong as almost instantly to melt off a piece of the end of a forged iron rod, without effect; except that once there were a very few globular drops, about the size of very small shot: these broke easily on the anvil, and looked, both internally and externally, like platina.

Remark.—It is probable, that the fusion here was owing to some accidental admixture, possibly iron: for the unmelted grains, exposed afterwards to a fire rather more intense, suffered no sensible alteration.

Exper. 6.—Platina was likewise exposed to the fire in conjunction with several substances, which are found to promote the fusion of other bodies, or to occasion considerable alterations in them. 1. Platina mingled with powdered charcoal, with compositions of charcoal, soot, common salt, and wood ashes, substances employed for changing iron into steel; suffered no change in weight or appearance, whether urged with an intense fire, or cemented for many hours in a weaker one. 2. Platina was injected into melted borax, and urged with an intense fire for several hours, without undergoing any alteration. Nor had black flux, common salt, pure fixed alkaline salts, or caustic alkalies, any sensible effect. 3. Vitreous matters were no more powerful than the saline. Platina was kept in strong fires, for several hours, with common green glass, with glass of antimony, and with glass of lead, without seeming to be in the least acted on by either. 4. Platina was likewise stratified with plaster of Paris, a powerful flux for the most difficultly-fusible metallic body hitherto known, forged iron; as also with quicklime, and with calcined flint; with as little effect as in the former trials.

Exper. 7.—Nitre, which reduces all the known metallic bodies, except gold and silver, into a calx, was mixed with an equal weight of platina, the mixture injected into a strongly ignited crucible, and the fire kept up for a considerable time; no deflagration happened; and the platina, freed from the salt by repeated ablutions with water, proved of the same weight and appearance as at first.*

Exper. 8.—1. An ounce of platina was spread on twice its weight of sulphur, with which some powdered charcoal had been previously mixed to prevent its becoming fluid in the fire so as to suffer the platina to subside. The crucible, having another with a hole in the bottom inverted into its mouth, was kept in a cementing furnace for several hours; when the sulphur was found to have entirely exhaled, leaving the platina separable from the charcoal by washing, without alteration or diminution. 2. We likewise varied the experiment, injecting repeatedly pieces of sulphur on platina strongly heated; and constantly found that pure sulphur had no more effect on this mineral, than on gold itself. 3. As fixed alkaline salts enable sulphur to dissolve gold; platina was exposed to the fire with a mixture of sulphur and alkali, called hepar sulphuris. After a considerable heat had been continued for some time, and the matter occasionally stirred, very little of the platina was found remaining in its proper form; the greatest part being taken up by the sulphureo-saline mixture, so as to dissolve along with it in water.

General Remarks.—It appears, from the foregoing experiments and observations, 1. That probably this mineral is originally found in large, hard masses, composed of true platina, a substance similar to the black Virginia sand, and another ferruginous matter of the emery kind, some spar, and particles of gold. 2. That these masses are, not without great labour, reduced into small grains, which are afterwards ground with mercury, in order to extract the gold. 3.

* See Tennant on the Action of Nitre on Gold and Platina in the Phil. Trans. for 1797.

That the pure platina is a white metallic substance, in some small degree malleable; that it is nearly* as ponderous as gold, equally fixed and permanent in the fire, equally indestructible by nitre, unaffected by sulphur, dissoluble by hepar sulphuris. That it is not to be brought into fusion by the greatest degree of fire procurable in the ordinary furnaces, whether exposed to its action in close vessels, or in contact with the fuel; by itself, or with the addition of inflammable, saline, vitreous or earthy fluxes.†

PAPER II.

The more obvious properties of this extraordinary mineral, and its habitus to fire, singly, and in conjunction with the various substances called by the chemists fluxes, made the object of the first paper. In this, it is proposed to examine the effect of acid spirits, simple and compound, applied after various manners; in order to determine not only its relation or habitus to them, but likewise its less obvious agreement or disagreement with the metallic bodies, whose history is more known.

The platina employed in the following experiments was previously freed from its fine dust by a sieve; from the mercury, by ignition; and from the golden and some of the other heterogeneous particles, by the eye assisted with glasses.

Exper. 1.—*Platina with the Vitriolic Acid.* 1. Several parcels of platina were digested for some hours, in a gentle heat, with spirit of vitriol, both concentrated, and diluted with different proportions of water. No solution happened, nor any sensible alteration, either in the liquors or the platina. 2. Three ounces of well-dephlegmated spirit of vitriol were boiled with one ounce of platina, in a tall, narrow-necked glass, for some hours. The liquor remained nearly of the same quantity as at first; and no change could be perceived either in it, or in the platina. 3. The glass being cut off a little above the liquid, the heat was gradually increased, till the liquor, which now began to evaporate, had, in 5 or 6 hours, totally exhaled, and the platina become dry, and red-hot. When cooled, washed with water, and exsiccated, it was found exactly of the same weight as at first, and its grains not divided, or apparently altered.

Remark.—Platina appears therefore entirely to resist the vitriolic acid; which, by one or other of the above processes, dissolves or corrodes every other known metallic body, except gold.

Exper. 2.—*Platina with the Marine Acid.* 1. Weak and strong spirits of salt being digested, separately, with $\frac{1}{3}$ their weight of platina, in a gentle

* More ponderous than gold when duly purified, its specific gravity being then 23.000; whereas that of gold is only 19.3.

† White arsenic excepted; for on exposure to a sufficient degree of heat, with such an addition, it may be brought into fusion.

heat, for several hours, the liquors remained uncoloured, the platina unaltered, and undiminished. The heat was afterwards increased, and the liquors kept strongly boiling till they had totally exhaled, without occasioning any sensible change in the platina. 2. Three ounces of a mixture of 2 parts decrepitated sea-salt, and 3 parts of vitriol highly calcined, were pressed smooth into a crucible; an ounce of platina spread evenly on the surface, and covered with some more of the mixture; the crucible closely luted, and kept in a moderate red heat for several hours. On examining it when cold, the saline mixture was found to have melted, and formed a smooth, uniform lump. The platina, which had sunk to the bottom, being separated from the mixture by washing, proved of the same appearance as at first, though a little deficient in weight. 3. The experiment was repeated with what is called the regal cement, a less fusible mixture, composed of common salt and colcothar each one part, and 4 parts of powdered red bricks. An oz. of platina, surrounded, as above, with 6 oz. of this composition, and cemented in a close-luted crucible with a red heat, for 20 hours, was still found unaltered in appearance, though there was some deficiency, as before, in the weight.

Remark.—The marine acid, when thus detained in the fire by the combination of other bodies, till strongly heated, and then set at liberty in the form of fume, dissolves or corrodes all the known metallic substances, gold alone excepted. As the platina, in these experiments, retained its original polished surface, without any mark of corrosion; it was presumed, that this mineral likewise had resisted the marine fumes; and that the deficiency was owing to some of the smaller grains having been washed off, along with the ponderous colcothar or metallic matter of the vitriol; an accident not easily avoided.

4. Platina was therefore treated with mercury-sublimate, a combination of the highly-concentrated marine acid with a volatile substance, which in a proper degree of heat it readily forsakes, to unite with other metallic bodies. An oz. of platina was spread on 3 oz. of powdered sublimate; the glass covered, and set in sand: after a moderate fire for some hours, the sublimate was found to have entirely arisen, leaving the platina of its original weight, as well as appearance. 5. Fifty grains of a mixture of one part of platina and 2 of gold, well nealed, and cautiously hammered into a thin plate, were surrounded with regal cement, the vessel covered, closely luted, and kept for a considerable time in a red heat. On examining the metal, it was found to retain the whiteness and brittleness, which gold constantly receives from so large a proportion of platina; and to have lost in weight about $\frac{1}{2}$ gr. or $\frac{1}{100}$ part.

Remark.—The loss here appears to have proceeded, not from the platina, but from alloy in the gold employed, which was above standard, but not perfectly fine: for the metal cemented a second time, with fresh mixture, suffered no further diminution. If the marine acid were capable of dissolving platina, instead of $\frac{1}{100}$, nearly $\frac{1}{3}$ would have been exeded. This experiment therefore determines, with certainty, the resistance of platina to the marine fumes; and that the regal cement, so called from its being supposed to purify gold from all heterogeneous metallic matters, is incapable of separating platina from it.*

Exper. 3.—*Platina with the Nitrous Acid.* 1. Spirit of nitre diluted with water, proof aqua fortis, and the strong nitrous spirit, were digested separately, with $\frac{1}{4}$ their weight of platina, in a gentle heat, for several hours. During the digestion, some bubbles were observed, as if a solution was beginning; but the liquors acquired no colour; and the platina, washed and dried, was found to have neither altered its appearance, nor lost of its weight. The fire being afterwards increased, and the acid spirits kept strongly boiling till they had entirely evaporated, no change could be observed in the platina. 2. Platina was likewise treated with nitrous mixtures, by processes similar to those in which it had been exposed to the marine fumes. After cementation for many hours, in a red heat, with a mixture of 3 parts calcined vitriol, and 2 of melted nitre, the grains were recovered not only unaltered, but without any deficiency in weight.

Remark.—From these experiments it is plain, that platina, equally with gold, resists the force of the vitriolic, marine, and nitrous acids, though applied in such a manner, as to be capable of perfectly dissolving all other known metallic bodies.

Exper. 4.—*Platina with Aqua Regia.* 1. Aqua regia,† which perfectly dissolved gold, poured on platina, began to act on it in the cold, and, by the assistance of a moderate heat, slowly dissolved it; acquiring at first a yellow colour, which deepened by degrees, as the menstruum became more saturated, into a dark brownish red. A few drops of the saturated solution tinged a large quantity of water of a fine golden colour. 2. The experiment was several times repeated with different aquæ regiæ, made by dissolving sea-salt and sal ammoniac, separately, in 4 times their weight of aqua fortis; and by abstracting the nitrous spirit from the same proportion of each of the salts. With all these menstrua the solution seemed to succeed equally.

3. In order to determine the quantity of menstruum necessary for the solution; 3 oz. of an extremely strong aqua regia, diluted with water, were poured on one oz. of platina, in a retort, to which was adapted a recipient. A gentle heat being applied, the menstruum acted violently, and red fumes arose in abundance. When about $\frac{2}{3}$ of the liquor had come over, the action was scarcely, if at all, sensible, though the fire was considerably raised. The distilled liquor, which appeared of a light redish colour, being poured back again into the retort,

* When these experiments were made, the marine acid in its oxygenized state (oxymuriatic acid) was unknown. The metal of platina, called platinum, is soluble in that preparation of the marine acid. † Termed in the New Chemical Nomenclature, nitromuriatic acid.

the solution began afresh; the vapour, which now came over, appeared pale, compared with the first. The cohobation was repeated 4 times, the distilled liquor proving paler and paler every time. At length, both the fumes and action ceased, though the fire was raised, and a considerable part of the platina remained undissolved. The solution was therefore poured off, some more of the menstruum added, the distillation and cohobation renewed, and this occasionally repeated, till the whole was taken up, excepting a little blackish matter, of which hereafter. The quantity of strong aqua regia, employed for dissolving the oz. of platina, was 5 oz.; but the last parcels appeared from their yellow colour not to be fully saturated, and on trial were found to take up near 50 grs. of fresh platina.

Remark. It appeared, that by this method of managing the process, 1 part of platina was soluble in about $4\frac{1}{2}$ of aqua regia: but that when the digestion was performed in open vessels in the common manner, and the fumes, which arise copiously during all metallic solutions, suffered to exhale, more than half as much again of the menstruum was requisite. This process might therefore possibly be applicable to advantage, in making solutions of metals in the way of business.

Examination of Solution of Platina.

Exper. 1.—As the vitriolic acid carries down metallic bodies, gold not excepted, from their solutions in other menstrua; this acid was mixed with solutions of platina.

1. When the solution of platina was previously diluted with water; the addition of dephlegmated spirit of vitriol occasioned no precipitation, or change of colour, though a large quantity of the acid was, at different times, dropped in, and the mixture suffered to stand for several days.

2. Dephlegmated spirit of vitriol, added to an undiluted solution of platina, immediately rendered it turbid, and threw down a dusky-coloured precipitate. The precipitate was not re-dissolved on the affusion of water; nor was the precipitation prevented by adding water immediately after the acid had been dropped in.

Exper. 2.—Solutions of platina, evaporated by a gentle warmth, to a proper pitch, and then set to shoot, yielded crystals, of a dark, almost opaque, red colour, in form of leaves, like flowers of benzoin, but thicker. The crystals, washed with proof spirit, became somewhat paler, but still remained of a high colour, resembling the deeper chives of saffron. Exposed to the fire, they seemed to melt, emitted white fumes, and at length fell into a dusky ash-coloured calx.

Exper. 3.—Solutions of platina, dropped on hot marble, immediately corroded it; but did not, like solutions of gold and some other metals, communicate any colour. Nor did they give any stain to the skin, to feathers, ivory, or other like animal substances, which liquors containing gold tinge purple.

Exper. 4.—As a minute proportion of gold contained in liquors is discoverable by their striking a purple colour with tin,

1. Some bright plates of pure tin were put into a solution of platina diluted with water. The plates, in a little time, looked of a dark olive colour, and soon after were covered with a reddish brown matter: the liquor became at first darker coloured, and afterwards by degrees, as the precipitate fell, nearly colourless; without exhibiting the least appearance of a purplish hue.

2. Platina was digested in a quantity of aqua regia insufficient to dissolve the whole; and the residuum dissolved in a fresh parcel of the menstruum. The two solutions, treated as above, yielded somewhat different phenomena, but no tendency to a purplish cast could be perceived in either. The latter, which looked yellow from not being fully saturated, was, when diluted with water, almost colourless. Yet, on the addition of the tin, it became yellow again, then red, and at length of a dark brownish red considerably deeper than the other more saturated solution. On standing for some time, it grew perfectly clear, depositing a paler, yellowish precipitate.

3. To determine whether platina was capable of preventing a small proportion of gold from discovering itself on this trial, one drop of a solution of gold was let fall into several ounces of a solution of platina diluted with water. On adding some plates of tin, the whole became immediately of a fine purple.

Remark. It may be proper to observe, that in these kinds of experiments, plates of tin are far more eligible than the solutions of tin usually employed: for the solutions fail of striking a purple colour with solution of pure gold, unless certain circumstances are observed, which are not easily hit upon; but tin in substance constantly succeeds, and requires no particular precaution.

Exper. 5. As gold is revived from its solutions by inflammable spirits, the metal gradually arising to the surface, in form of a bright yellow cuticle;

1. A solution of platina was mixed with a large proportion of highly-rectified spirit of wine, and exposed for many days to the sun, in a wide-mouthed glass, slightly covered with paper, so as to keep out dust. There was no appearance of any yellow skin; nor any other alteration, than that the platina had begun to crystallize from the evaporation of the fluid.

2. A drop or two of a solution of gold being added to a large quantity of a mixture of solution of platina and spirit of wine, and the whole exposed as above to the sun; a golden film was in a few days observed on the surface.

Remark. It follows from this experiment, and the foregoing one with tin, that platina contains no gold; and that it cannot, any more than the common metallic or other soluble substances, prevent a small proportion of gold mixed with it from being discoverable.

Exper. 6.—1. The spirits of sal ammoniac, prepared both by quicklime and by fixed alkaline salts, added to solutions of platina diluted with distilled water, precipitated a fine red sparkling powder; which, exsiccated, and exposed to the fire in an iron ladle, became blackish; without at all fulminating, which calces of gold, prepared in the same manner, do violently. On washing some of this precipitate on a filter, by repeated affusions of water, the greatest part of it dissolved; only a small quantity of a blackish matter remaining, and the liquor passing through of a deep, bright, golden colour. A very large quantity of the fluid was tinged of this colour by a small one of the powder.

2. Salt of wormwood, fixed nitre, the lixivium saponarium of the Lond. Pharmacopœia, precipitated a powder similar to the foregoing, except that its colour was less brilliant.

3. Sal ammoniac likewise, one of the ingredients, to which the menstruum owed its power of dissolving the platina at first, precipitated great part of it in form of a similar powder.

4. The liquors remaining after all these precipitations with saline substances, appeared of a yellow colour, almost as deep as before the precipitation. Fixed and volatile alkalies being added alternately, the liquor still continued yellow: but either of them, added after sal ammoniac had performed its office, threw down a fresh precipitate, which left the liquor colourless.

5. The addition of tin likewise, after either of the salts separately had thrown down all they were capable of doing, occasioned a fresh and complete precipitation; provided a little more of the menstruum was dropt in, to enable the liquor to act on the metal.

Exper. 7. As gold is totally precipitated by alkaline salts, but platina only in part; and as a minute portion of platina, remaining dissolved, tinges a surprisingly large quantity of the fluid of a yellow colour; it was presumed, that a small admixture of platina with gold might by this means be readily discoverable. A few drops of a solution of platina were therefore mixed with above 100 times the quantity of a solution of gold; the whole diluted with water; and a pure alkaline salt gradually added, as long as it occasioned any effervescence or precipitation. The remaining liquor was of so deep a yellow colour, that it was judged the platina would have discovered itself, though its proportion had been less than 1000th part of that of the gold.

Exper. 8.—1. Zinc, which totally precipitates all the other known metallic bodies, put into a diluted solution of platina, was very quickly acted on, and threw down a blackish calx. The liquor in good measure preserved its yellow colour; a n ark that part of the platina remained suspended.

2. Iron, which precipitates all the metals from their solutions, except zinc,

threw down a similar calx. It could not be judged by the eye, whether the precipitation was complete, the solutions of iron and platina nearly agreeing with each other in colour.

3. Copper, the precipitant of mercury and gold, readily threw down platina from its solution, in form of a greyish calx, which was found on trial to retain a notable quantity of the copper. The liquor remaining after the platina had fallen, was of a more dusky green than solutions of pure copper, probably from its retaining some of the platina.

4. Mercury, which precipitates gold alone from aqua regia, put into a diluted solution of platina, seemed in a little time to be divided, and did not run freely. Soon after, it appeared covered with a greyish matter, which at first was apprehended to be a precipitate, but was found afterwards to be a part of the mercury corroded. On applying a moderate heat, the whole of the quicksilver, the quantity of which was very considerable, was dissolved, without any precipitation.

The experiment was repeated with a larger quantity of mercury than the solution was capable of taking up. The platina now gradually fell down among the undissolved quicksilver, in form of a dark brownish powder; leaving the liquor nearly colourless.

5. A solution of gold mingled uniformly with a solution of platina, without occasioning any turbidness or precipitation. The mixture, diluted with water, and suffered to stand for some time, threw up a bright golden pellicle to the surface.

Exper. 9.—1. A solution of platina, super-impregnated with as much mercury as it was capable of taking up, on being evaporated a little, so as to dispose it to shoot, yielded crystals not at all like those of platina, but in form of spicula, externally of a yellowish hue. These, slightly washed with proof spirit, became colourless. Exposed to the fire, they emitted copious white fumes, with a hissing or crackling noise; and left a very small quantity of a reddish powder.

2. A mixture of solutions of gold and platina, being treated in the same manner, ruby-coloured crystals were obtained, which appeared to be chiefly gold, with very little of the platina.

Remark. It seems therefore, that mercury and gold crystallize from their solutions before platina, leaving the greatest part of that mineral dissolved. This affair, particularly with regard to gold, deserves further inquiry.

Exper. 10.—As the calces of metals, obtained by precipitation from acids, or by other means, vitrify along with frit or glass, and tinge them of various colours; and as this process is recommended by some for investigating the nature of unknown metallic bodies; the following trials were made with precipitates of platina.

1. Half an ounce of a precipitate thrown down from solution of platina by

plates of pure tin, was triturated in an iron mortar with 8 times its quantity of common white glass, the mixture put into a crucible, which was closely luted, and placed in a wind furnace. The fire was gradually raised, and kept up extremely strong for about 10 hours; when, the crucible being taken out and broken, the matter appeared of a dark blackish colour, untransparent, easily friable; interspersed with a bright whitish matter, apparently metallic.

Remark. It is probable, that this metallic matter was the platina; and that the glass owed its opacity and dark colour, not to this mineral, but to the tin in the precipitate, some particles of iron abraded from the mortar, or other accidental causes.

2. A quarter of an ounce of a precipitate of platina, made by alkaline salt, was ground in a glass mortar with 12 times its weight of white glass; and committed to the same fire as the foregoing. The result was a compact, cloudy glass, pretty transparent in thin pieces, covered in part with a thin whitish coat. Towards the upper part, and all round the sides, were observed several particles of metal; which appeared to the eye like bright platina, and proved hard to the point of a knife.

Remark. Nor does the glass here seem to have received any thing from the platina; the change being no other than what white glass is found to undergo from a slight impregnation with inflammable matter.

General Remarks. It appears from the experiments related in this paper, that platina, like gold, is not acted on by the simple acids,* which dissolve every known metallic body besides; that aquæ regiæ, the solvents of gold, prove likewise menstrua for platina; and that consequently the common methods of assaying or purifying gold by aquafortis, aqua regis, or the regal cement, can no longer be depended on; that it differs from gold, in giving no stain to the solid parts of animals, not striking a purple colour with tin, not being revived from its solutions by inflammable spirits, not being totally precipitable by alkaline salts; that in certain circumstances it throws out gold from its solutions; that these properties afford means of distinguishing a small proportion of gold mixed with a large one of platina, or a small proportion of platina with a large one of gold; and that platina contains no gold, excepting the few particles distinguished by the eye; that platina is precipitated from its solutions by the vitriolic acid, and by the metallic substances, which precipitated gold, though scarcely totally by any; and that its precipitates resist vitrification, and this perhaps in a more perfect manner than precipitates of gold itself.

PAPER III.

The two former papers have given an account of the habitus or relation

* As mentioned in a former note, it is soluble in the oxymuriatic acid.

of platina to the principal substances which act on metallic bodies; and shown that it is a simple metal, of a particular kind, essentially distinct from all those hitherto known, though possessed of some properties generally supposed peculiar to gold. Many of its distinguishing characters have been already pointed out; others will result from combining it with the several metals; with each of which, notwithstanding its resistance to the most intense fires by itself, or with unmetallic additions, it melts perfectly; occasioning remarkable alterations in their colour, texture, and hardness.

Art. 1.—*Platina with Tin.* 1. Equal parts of platina and pure tin were injected into a mixture of black flux and common salt in strong fusion; and urged with a quick fire, in a good blast furnace. After a few minutes the whole appeared perfectly melted; and on being instantly poured out, ran freely along a narrow mould, forming a smooth ingot, nearly of the same weight with the platina and tin employed. The compound proved extremely brittle, breaking easily from a fall; internally it appeared of a close and smooth, though uneven surface; and of a dark grey colour. By the file, or a knife, it was readily scraped into a blackish dust.

2. One part of platina and two of tin, covered with a black flux, borax, and common salt, were melted in a wind furnace: the platina appeared perfectly taken up by the tin, soon after the fire had been raised to a light white heat. The ingot was found deficient in weight about $\frac{1}{19}$. It greatly resembled the foregoing, being only a little less brittle, and of a somewhat lighter colour.

3. One ounce of platina and 4 of tin, covered with black flux and common salt, and urged with a quick fire, melted together without loss. This compound yielded a little to gentle strokes of a flat hammer, but was by no means tough. It broke in pieces from a rude blow, and was still readily scraped into dust by a knife. The surface of the fracture was rough and granulated.

4. One part of platina and 8 of tin, injected into a fluid mixture of black flux and common salt, united, without loss, into a pretty tough compound; which bore to be considerably flattened under the hammer without breaking, cut smooth with a thin chissel, and shaved with a knife. Broken, it appeared of a sparkling, dark coloured, coarse grained texture.

5. One part of platina and 12 of tin, treated in the same manner, formed a mixture tolerably ductile; but still of a dull, dark hue, and a rough coarse grain, though somewhat less so than the preceding.

6. A mixture of 1 part of platina and 24 of tin, proved not much stiffer than tin. The colour was whiter, and the grain finer and evener than those of the preceding compositions; though in both respects it fell considerably short of pure tin.

7. Several of these compositions, covered with black flux, which had been

previously melted, were exposed in crucibles closely luted, to a strong fire in a wind furnace, which was steadily kept up for 8 hours. When taken out, they were all found to have suffered some diminution, amounting to about $\frac{1}{40}$ of the tin. In appearance and quality, there was no sensible alteration, except that the mixture seemed more uniform, and the grain a little finer.

8. The remarkable gravity of platina induced us to examine the several mixtures hydrostatically. Here it was found, that the specific weight of the compound constantly turned out less than the medium of the gravities of the two ingredients; and generally the more so, as the proportion of the platina was the greater.

	Specific gravity. By experiment.	By calculation.	Difference.
Platina.	17.000		
Platina 1, tin 1	10.827	12.090	1.263
Platina 1, tin 2	8.972	10.354	1.481
Platina 1, tin 4	7.794	9.144	1.350
Platina 1, tin 8	7.705	8.271	0.566
Platina 1, tin 12	7.613	7.935	0.322
Platina 1, tin 24	7.471	7.573	0.102
Tin	7.180.		

Remarks. It appears from the foregoing experiments, that platina melts with at least equal its weight of tin; that it destroys the malleability of near 4 times its weight: that with larger proportions it forms compounds tolerably ductile, but renders the texture of the tin coarser, and debases its colour. The difference in colour of these compositions was much less conspicuous on the touchstone, than when the fractures of the ingots were examined; though, on close inspection, they appeared all sensibly duller and darker than pure tin, and the more so, in proportion as the platina prevailed. They all tarnished in the air; those least, which had a very small or a very large proportion of platina.

It is remarkable, that though tin is a metal very readily destructible by fire, yet in most of the preceding fusions, there was scarcely any sensible loss of weight. This is to be attributed not solely to the admixture of the platina, but also to the flux made use of, and more particularly to the celerity and short continuance of the heat. In N° 2 and 7, the only ones in which the loss was at all considerable, the fire was slowly raised, and long continued.

Art. 2.—Patina with Lead. 1. Equal parts of platina and lead were injected into a mixture of black flux and common salt, previously melted together; and the fire hastily raised by bellows. A much stronger heat was requisite than for the fusion of platina with an equal quantity of tin; and the loss was considerably greater, amounting to about $\frac{1}{64}$. The metal yielded difficultly to the file; broke, by a moderate blow, of a close texture, uneven surface, and rough jagged edges; the colour was very dark, with a faint purplish cast.

2. One part of platina and 2 of lead, covered with borax and black flux, and exposed to a gradual fire, in a wind furnace, did not come into fusion till the fire had been raised to a strong white heat: from the continuance of heat in this experiment the loss was great, being nearly $\frac{1}{24}$ of the mixture. The ingot proved hard and brittle, like the preceding, but broke with a striated surface.

3. One ounce of platina and 3 of lead, treated in the same manner, required still a very strong fire for their perfect fusion: and lost about $\frac{1}{26}$. The metal broke less easily than either of the preceding, and in some measure yielded to the hammer: the colour was somewhat darker, and inclined more to purplish.

4. One part of platina and 4 of lead, being covered with black flux and common salt, and committed to a wind furnace, the platina was not perfectly taken up, till the fire had been raised to a considerably strong white heat; the loss was $\frac{1}{40}$. The same proportions of the metals, injected into a fluid mixture of the flux and salt, previously brought to the above degree of heat, almost instantly melted, and lost only $\frac{1}{100}$. The ingot was much tougher than the foregoing, filed well, and cut tolerably smooth with a knife. On breaking, the upper part appeared composed of bright plates, the lower of dark purplish grains.

5. One part of platina and 8 of lead united easily in a quick fire, and lost little or nothing. The metal worked and looked like very bad lead; on breaking, the texture appeared partly composed of transverse fibres, and partly of grains; the colour dull and purplish.

6. One part of platina and 12 of lead united, without loss, into a compound very little different from the foregoing. On breaking, its texture was somewhat finer, and composed chiefly of fibres, with very few grains.

7. A mixture of 1 part of platina and 24 of lead proved not very much harder than lead of a middling quality. The colour was still somewhat purplish, and the texture fibrous; but the fibres were remarkably finer than where the platina was in larger proportion.

8. The foregoing compositions, when newly polished, appeared in general of a dark iron colour; which, on exposure to the air, quickly tarnished to a brownish yellow, a deep purplish, and at length a blackish. They all filed freely, without sticking in the teeth of the file, as lead does by itself.

9. On returning these compounds to the fire a second time, it was constantly observed, that after they had come into perfect fusion, if the heat was slackened a little, great part of the platina subsided; that nevertheless, the lead decanted off, even in a heat below ignition, retained so much of the platina, as rendered it of a fine fibrous texture, and purplish colour.

The several mixtures, covered with black flux, and kept in strong fusion, in crucibles closely luted, for 8 hours, suffered a diminution in weight, amounting to about $\frac{1}{30}$ of the lead. On breaking, those with a large proportion of platina appeared of a leafy, and those with a smaller, of a fine fibrous texture, which seemed in general to be characteristics of the perfect union of the platina and lead. They all looked whiter and brighter than at first, but tarnished sooner in the air. One mixture in particular, of 4 oz. of platina and 12 of lead, broke into large, white, bright, shining, talc-like flakes; which, on exposure to the air, changed in a little time to a reddish, a purple, and a deep blue; and at length turned slowly to a dark blackish colour.

10. On examining these compounds hydrostatically, their gravities turned out less than they ought to have been according to their calculation, but not so much less as those of the compositions of platina and tin.

	Specific gravity. By experiment.	By calculation.	Difference.
Platina	17.000		
Platina 1, lead 1	14.029	14.193	0.164
Platina 1, lead 2	12.925	13.257	0.332
Platina 1, lead 4	12.404	12.509	0.105
Platina 1, lead 8	11.947	12.009	0.062
Platina 1, lead 12	11.774	11.818	0.044
Platina 1, lead 24	11.575	11.610	0.035
Lead	11.386		

Remark. It appears, that a small proportion of platina is taken up and kept suspended by lead, in a very gentle heat; but that a large proportion is not taken up near so easily as by tin; and if united by a strong fire, subsides in part on its abatement. A little quantity stiffens and hardens lead more than it does tin; but a large one does not near so much diminish its malleability. A leafy or fibrous texture, a purplish colour, or disposition to acquire this colour in the air, are peculiar to the mixtures with lead.

Art. 3.—*Platina with Silver.* 1. Equal parts of platina and of pure silver revived from luna cornea, covered with a borax, and urged with a strong fire in a blast furnace, melted perfectly together, and without loss, but did not run freely along the mould. The ingot was hard to the file, and broke by a rude blow; though by gentle strokes it bore to be considerably flattened. Internally it appeared of a much duller and darker colour than silver, and of a coarser texture.

2. One part of platina and 2 of silver, covered with nitre and common salt, did not flow thin till the fire was raised to a very strong white heat. The compound proved less brittle than the foregoing, and not so hard to the file: the texture was composed of smaller grains, and the colour whiter.

3. One part of platina and 3 of silver still required a very strong fire for their perfect fusion; the metal was hard and brittle, though less so than the preceding: when well and repeatedly nealed, it bore to be hammered, or flattened between steel rollers, into thin plates.

4. One part of platina and 7 of silver melted together with ease. This compound hammered tolerably well, proved much harder than silver, and not so white, or of so fine a grain.

5. These compositions, weighed hydrostatically, turned out like the others, a little lighter than by calculation; but the difference, which before seemed to increase with the platina, was here greatest when the platina was in least proportion.

	Specific gravity. By experiment.	By calculation.	Difference.
Platina	17.000		
Platina 1, silver 1	13,535	13.990	0.455
Platina 1, silver 2	12.452	12.987	0.535
Platina 1, silver 3	11.790	12.485	0.695
Platina 1, silver 7	10.867	11.732	0.865
Silver	10.980.		

Remark. Platina appears to unite more difficultly with silver than with either of the foregoing metals. Even when the proportion of the platina is small, the greatest part of it subsides on an abatement of the heat, by which the union had been effected. This was prevented by pouring out the metal, when perfectly fluid, at one jet, into a broad mould: in which the compound began to congeal before the platina could separate.

Platina diminishes the malleability of silver far less than that of tin or lead; and does not, in whatever proportion employed, so much debase its colour, or dispose it to tarnish in the air.

Art. 4.—Platina with Gold. 1. Equal parts of platina and gold, exposed to an intense fire, melted perfectly together, and ran thin into a long mould, without loss. The metal was of a white colour, hard to the file, broke by a rude blow, but when well nealed, yielded considerably to the hammer.

2. One part of platina and 4 of gold came into fusion in a moderate fire, but still required a very strong one for their perfect union. This compound appeared but a little paler than standard gold with silver alloy; and proved so tough, as to be beaten, with proper care, into thin plates, without breaking or cracking about the edges. On melting it a second time with nitre and borax, it became very pale, and was not without great difficulty made to recover its colour.

Art. 5.—Platina with Copper. 1. Equal parts of platina and copper, exposed, without addition, to a strong fire hastily excited by bellows, soon became fluid but not thin; and lost about $\frac{1}{64}$. The metal proved extremely hard to the file; broke difficultly on the anvil; flew asunder on endeavouring to cut it with a chissel; and appeared internally of a coarse grained texture and white colour.

2. One ounce of platina and 2 of copper, urged with a quick fire in a blast furnace, without addition, flowed sufficiently thin, and scarcely suffered any sensible loss. The metal was still very hard, and yielded but little to the hammer. It looked darker coloured than the foregoing, with a slight reddish cast.

3. One ounce of platina and 4 of copper, treated in the same manner, united, without loss, into a pretty tough compound; which bore to be considerably flattened, cut with a chissel, and bent almost double before it cracked. Internally, it looked of a fine texture, and a very pale copper colour.

4. A mixture of 1 oz. of platina and 5 of copper, stretched somewhat more easily under the hammer than the preceding; and appeared of a redder colour.

5. On increasing the copper to 8 times the quantity of the platina, the compound proved sufficiently tough, broke difficultly, and hammered well. It was much harder than copper, and of a paler colour.

6. A mixture of 1 part of platina and 12 of copper was somewhat more easily extended under the hammer than the foregoing, and proved softer to the file. It stuck a little in the teeth of the file, which the compositions with a larger proportion of platina did not.

7. A mixture of 1 part of platina and 25 of copper was still a little paler coloured than pure copper, and considerably harder and stiffer, though very malleable. On increasing the copper a little further, the mixture retained a degree of hardness, and appeared of a fine rose colour.

8. On weighing the foregoing compositions hydrostatically, the diminution of gravity was found more regular than in the mixtures with other metals, being constantly greater in proportion as the quantity of platina was larger.

	Specific gravity. By experiment.	By calculation.	Difference.
Platina	17.000		
Platina 1, copper 1	11.400	12.915	1.515
Platina 1, copper 2	10.410	11.553	1.143
Platina 1, copper 4	9.908	10.464	0.556
Platina 1, copper 5	9,693	10.191	0.498
Platina 1, copper 8	9.300	9.738	0.438
Platina 1, copper 12	9.251	9.458	0.207
Platina 1, copper 25	8.970	9.144	0.174
Copper	8.830		

Remark. In the foregoing fusions, though in general no flux was made use of, there was scarcely any sensible loss of weight, unless in N° 1, where the large proportion of platina required the fire to be raised to a violent degree. This seems owing, in good measure, to the platina preventing the scorification of the copper; for on melting pure copper a great number of times, both with and without fluxes, there was constantly a little loss. A small proportion of platina appears to improve the hardness of copper, without injuring its colour, or, so far as could be judged, its malleability. The mixtures with a large proportion of platina are difficultly extended under the hammer when cold; and while red-hot, fly in pieces. They all bear a good polish, and do not tarnish in the air so much, or so soon, as pure copper.

Art. 6.—*Platina with Iron.* Iron, the last of the metals in point of fusibility, was several times attempted to be united with platina, in its perfect malleable state. But as the fluxes necessary for rendering forged iron fusible corroded the crucibles, before the metal flowed thin enough to dissolve the platina, pure cast iron was substituted.

1. Cast iron and platina, of each 3 oz. exposed without addition to a strong fire, united into a thick fluid; which, on adding an ounce more of iron, flowed thin, the compound suffered to cool in the crucible (which had become too soft from the heat to admit of its being poured out) was found, on breaking the vessel, in one lump, not convex, the form, which the iron usually assumes, but of a very concave surface; the weight about $\frac{1}{60}$ less than that of the metals employed. It proved excessively hard, so as not to be touched by the file; and so tough, as not to be broken by repeated blows of a sledge hammer, from which it received some impression. Heated red, it broke easily, and looked internally of a uniform texture, composed not of bright plates like the iron at first, but of very dark-coloured grains.

2. One ounce of platina being injected on 4 of cast iron beginning to melt, and the fire kept up strong, the whole came quickly into fusion, and on cooling, formed an equable compound, which like the former proved extremely hard, and seemed to stretch a little under the great hammer without breaking. The colour was still very dark, though less so than when the platina was in larger proportion.

3. One part of platina and 12 of iron melted without difficulty, and with little or no loss. This compound was still much harder than the iron at first, and had a very considerable degree of toughness. Like the others, it could not be broken while cold, without extreme violence; but proved very brittle when heated red.

4. The foregoing compositions, especially those in which the proportion of platina was large, received a fine polish; and did not rust or tarnish on being exposed to the air in a dry room for several months.

5. A composition of 1 part of platina and 4 of iron was treated with substances, which produce notable alterations in pure iron. Surrounded with

Reaumur's steel making mixture (composed of charcoal powder, soot, wood-ashes, and common salt) and cemented in a close luted crucible for 12 hours, it gained an increase of $\frac{1}{39}$ its weight, yielded to the file more easily than at first; seemed to receive no additional hardness on being ignited and quenched in water, and discovered none of the qualities of steel. A piece broken off from the same ingot, treated in the same manner, with the powder for softening cast iron (viz. bone-ash, with a small proportion of charcoal) was found increased in weight about $\frac{1}{34}$, proved less hard to the file than at first, but manifestly harder than the part cemented with the steel-making mixture.

	Specific gravity. By experiment.	By calculation.	Difference.
Platina	17.000		
Platina 3, iron 4	9.917	11.343	1.426
Platina 3, iron 12	8.700	9.080	0.380
Platina 3, iron 16	8.202	8.663	0.461
Platina 3, iron 36	7.800	7.862	0.062
Iron	7.100.		

General Remarks.—Platina melts with equal its weight of each of the metals; with one more readily than with another. With some it becomes fluid, if the proportion of the platina is not large, in a moderate fire; but a strong one is constantly requisite for its perfect solution. Compositions of silver, copper, lead, with about $\frac{1}{3}$ their weight of platina which had flowed thin enough to run freely into the mould, and appeared to the eye perfectly mixed, on being digested in aquafortis till the menstruum ceased to act, left several grains of platina in their original form. On viewing these with a microscope, some appeared to have suffered no alteration; others exhibited an infinite number of minute bright globular protuberances, as if they had just begun to melt.

Platina hardens and stiffens all the metals; one more than another, lead the most. In a moderate quantity it diminishes, and in a large one destroys, the toughness of all the malleable metals; but communicates some degree of this quality to cast iron. Tin bears much the least, and gold and silver the greatest quantity, without the loss of their malleability.

A very small proportion of platina scarcely injures the colour of copper and gold: a larger renders both pale; a far less quantity has this effect on copper than on gold. It debases and darkens, in proportion to its quantity, the colour of the white metals; that of silver much the least, and of lead the most. It in good measure preserves iron and copper from tarnishing in the air; scarcely alters gold or silver in this respect; makes tin tarnish soon, and lead exceeding quickly.

PAPER IV.—*Platina mixed with Semimetals.* 1. With mercury. 1. An ounce of platina and 6 oz. of pure quicksilver were rubbed together, with a little common salt and water, and a few drops of spirit of salt, in an iron mortar. After some hours trituration, the grains of platina became coated with the quicksilver, so as to cohere into an imperfect amalgam. A part of the fluid quicksilver poured off, and evaporated in an iron ladle, left a considerable quantity of a dark coloured powder, intermingled with bright shining moleculæ: a part strained through leather, left a smaller proportion of a similar powder. The platina, which had been thus attenuated by the mercury, so as to pass with it through the pores of leather, proved as refractory in the fire as at first. Exposed to a very vehement heat, by itself, with borax, with white glass, it neither melted, nor suffered any sensible alteration; nor did it communicate any colour to either of the fluxes.

2. One part of platina and about 4 of lead were melted perfectly together; and after the heat had somewhat abated, poured gently into 3 times the quantity of quicksilver, heated so as to fume. A blackish powder was immediately thrown to the surface: this appeared to be chiefly platina. On grinding them together, a fresh powder gradually separated; which, being occasionally washed off, in appearance greatly resembled the foregoing, but was found, on proper trials, to participate much more largely of the mercury and lead than of platina. The amalgam, which was of a very dull colour, on exposure to the fire swelled and leaped about, though the heat was scarcely sufficient to evaporate the quicksilver. After constant and rapid agitation with water, occasionally renewed, in an iron mill, for a week, it looked bright and uniform, and suffered the mercury to exhale freely. A dark coloured calx remained, which proved, on examination, to be platina, with a very little lead.

Remark. Mercury is supposed to have a greater affinity with lead than any other metallic body, gold and silver excepted. In this experiment, it had a greater affinity with platina than with lead, since it retained most of the platina, after the lead, which was in much larger proportion, had been almost entirely thrown out. The part of the platina, which the mercury rejected at first, and that which it retained to the last, did not appear dissimilar to each other, or different in quality from the platina employed.*

3. A mixture of 1 part of platina and 2 of gold, which proved very white and brittle, after being repeatedly nealed, was cautiously flattened into thin plates, and thrown red-hot into boiling quicksilver. On trituration and ablution with water, a powder separated, copiously at first, and by degrees more sparingly. After the process had been continued about 24 hours, there was no further separation, except of a very little blackish matter, into which a part of the mercury

* Concerning the action of mercury on platina [platinum] and the compound thence formed, see the interesting experiments of Mr. Chenevix in the Phil. Trans. for 1805.

is always changed in these kinds of operations. The amalgam, which looked very bright, left, on evaporation, a spongy mass, of a high colour, which being melted, and poured into an ingot, proved very soft, extremely malleable, and in all respects resembled the pure gold made use of, without the least appearance of platina.

Remark.—It is greatly to be wished that this method of purifying gold from platina may prove sufficiently accurate to exactly determine the quantity of each in the mixture. The experiments hitherto made do not sufficiently clear up this point; a great number are still necessary before it can be fully ascertained.

2. *With Bismuth.*—Equal parts of platina and bismuth, injected into a mixture of black flux and common salt, previously brought into fusion, and urged with a quick fire, strongly excited by bellows, melted perfectly in a few minutes, and suffered very little loss. Without these precautions, the bismuth could scarcely be made to take up above $\frac{1}{5}$ its weight; great part of which, on an abatement of the heat, subsided.

Mixtures of platina with different proportions of bismuth proved all, like the bismuth itself, extremely brittle: one was not remarkably more so than another. To the file, they were scarcely harder than pure bismuth. They broke of an irregular surface, composed chiefly of striæ, with some plates. When newly broken, they looked bright and sparkling; except the compositions with a large proportion of platina, which were of a dull greyish colour, without any brightness. They all tarnished slowly in the air, to a dark yellowish, purplish or bluish colour. Several acquired in part a fine deep blue, which has suffered no change in above a 12-month; some parts of the masses still remaining white as at first, and others inclining to purple.

3. *With Zinc.*—On 1 oz. of platina, covered with borax, and heated in a blast furnace to a strong white heat, was injected an equal quantity of zinc. A violent deflagration arose, and the platina was almost instantly dissolved: the matter, immediately poured out, was found to have lost near half an ounce.

On several times repeating this experiment with different proportions of the 2 metals, both in a quick fire, and in 1 more gradually raised in a wind furnace, the zinc was constantly found a powerful menstruum for platina, but suffered great loss from the heat requisite for rendering the mixture sufficiently fluid. When so much of the zinc had been dissipated, that the remainder amounted to no more than $\frac{1}{4}$ of the platina, the compound still continued fluid enough to run freely into a long mould.

Compositions of platina and zinc differed little in appearance from zinc itself; except that where the quantity of platina was large, they were of a closer texture, and a duller hue, with rather more of a bluish cast. They did not tarnish, or change their colour, on being exposed for several months to the air, in a dry room. They were harder to the file than the zinc at first, and fell in pieces under the hammer; without at all stretching; which pure zinc does in a considerable degree.

4. *With Regulus of Antimony.*—Regulus of antimony, the most difficultly fusible of the semimetals, dissolved, in a strong fire, equal its weight of platina. The compound looked of a much duller colour than the regulus at first; and broke of a close and uniform, though uneven, surface. It proved considerably harder to the file, but not remarkably more or less brittle.

On increasing the quantity of the regulus, the compound proved brighter, and of a leafy texture, little different from that of the pure regulus.

Platina mixed with Compound Metals.

With Brass.—1. Equal parts of platina and brass, covered with borax, and urged with a quick fire in a blast furnace, melted perfectly together, and scarcely suffered any loss. The mixture was of a greyish white colour, filed hard like bell-metal, broken from a blow of the hammer, without stretching or receiving any impression, and flew asunder on endeavouring to cut it with a chissel. Internally, it appeared of a uniform fine grain, a close texture, and a darker colour than on the outside. It bore a very fine polish, and did not tarnish on being exposed to the air in a dry room for many months.

2. One part of platina and 2 of brass, melted in a slow fire, lost about $\frac{1}{36}$. The ingot was of a duller colour than the foregoing, with a faint yellowish cast: it filed softer, broke less readily from the chissel, but cracked and fell in pieces under the hammer.

3. One part of platina, and 4 of brass, covered as before with borax, and exposed to a quick fire, melted without loss. This compound proved yellower than the preceding, filed softer, bore to be cut some depth with a chissel before it broke, and received some impression from the hammer, stretching a little, but soon cracking in various directions.

4. On increasing the brass to 6 times the weight of the platina, the compound appeared yellower, though still very pale. It proved softer to the file; and received a greater impression from the hammer, and a deeper one from the chissel, before it broke.

5. A mixture of 1 part of platina and 12 of brass was considerably paler, and much harder, than brass. It broke from the chissel; and cracked, before it had extended much, under the hammer. It bore a good polish, and was less apt to tarnish than brass; though in both respects it fell short of the compositions with larger proportions of platina.

With Copper and Tin.—1. One hundred parts of platina, 34 of copper, and 12 of tin, covered with borax, became fluid in a strong fire, and suffered no considerable loss. The ingot proved extremely hard, so as scarcely to be touched by the file; and very brittle, breaking from a moderate blow, of a rough surface, and dull bell-metal colour. It bore a good polish, and did not tarnish in the air.

2. Platina and copper, of each 1 oz., and 4 oz. of tin, melted perfectly together, and without loss. This compound filed freely and easily, bore to be cut with a knife, but broke readily on the anvil, of an irregular surface, and dull whitish colour. Polished, it looked like polished iron. The fracture soon tarnished to a yellow; the polished part grew dull, but retained its colour.

3. A mixture of platina and copper, of each 1 part, and 8 of tin, proved softer than the foregoing; and bore to be flattened a little under the hammer. It broke of a very irregular surface, composed of a great number of bright white plates. The fracture soon tarnished; the polished part retained its colour.

Remark. It is observable, that in the first of these experiments, platina was perfectly taken up by less than half its weight of a mixture of copper and tin; though it could scarcely be made to melt with less than its own weight of either of them separately, in a fire equally, or rather more, intense.

The specific gravity of these mixtures turned out, on experiment, a little less than by calculation; though the copper and tin, melted together without platina, formed a compound specifically heavier than even the copper by itself.

The several mixtures with zinc, bismuth, regulus of antimony and brass, were likewise weighed hydrostatically, and found all somewhat lighter than they ought to have been by calculation. As few hydrostatical experiments seem to have been made on zinc and bismuth, it may be proper to mention, that the gravity of pure zinc turned out 7.050, and that of bismuth 9.733.

Hitherto we have considered the miscibility of platina with metallic bodies, and the alterations which different proportions of it produce in their appearance and qualities: employing the necessary precautions for preventing the scorification and dissipation, which most of the metals suffer in the fire; and which some remarkably promote in those which by themselves are more difficultly, or not at all, destructible. We shall now examine the relation of platina, in this respect, to those metallic substances, which are the most destructive.

1. *Cupellation and Scorification of Lead with Platina.*

1. A mixture of platina and lead was cupelled, under a muffle, in an assay-furnace. For some time the process went on well; the lead gradually changing into scoriæ, which were thrown off to the sides, and absorbed by the cupel, or dissipated in fume. In proportion as the lead worked off, the matter required a stronger fire to keep it fluid; and at length, collecting into a dull flat lump, could no longer be made to flow in the greatest degree of heat which the furnace was capable of giving. The lump broke very easily, appeared of a dull grey

colour both internally and externally, and of a porous texture. It weighed about $\frac{1}{4}$ more than the quantity of platina employed.

2. This experiment was many times repeated and varied: the lead attempted to be worked off on bone-ash, pressed into the bottoms of crucibles, scorified in assay-crucibles, by intense fires, in a blast furnace, and blown off on tests before the nose of a bellows, with the same event; the platina not only perfectly resisting the power of lead, which by these operations destroys every other known metallic body, except gold and silver, but likewise retaining and preventing the scorification of a part of the lead itself.

3. In the history of the fusion of platina with lead, it has been observed, that this metal deposites in a gentle heat great part of the platina, which had been united with it by a strong one. As the part, which remained suspended, might be presumed to differ from that which subsided; a quantity of lead was decanted off from fresh parcels of platina, and both the decanted metal and the residuum submitted to the preceding operations separately. The event was still the same; the matter becoming consistent when the lead had been worked off to a certain point, and refusing further scorification.

4. A mixture of platina and lead, which had been cupelled in an assay-furnace as long as it could be kept fluid, was exposed in a crucible to a fire vehemently excited, by itself, with powdered charcoal, with black flux, borax, nitre, common salt. The matter neither melted nor suffered any considerable alteration, becoming only somewhat more porous; probably from a little of the lead having exsuded without the liquefaction of the mass. The immediate contact of burning fuel, agitated by bellows, made some of these mixtures flow, after they had refused to melt in vessels acted on by intense fires. Very little of the lead was dissipated by this means.

On examining the cupelled matters hydrostatically, those which appeared most spongy were found nearly as ponderous as the crude platina. Among the more compact, the gravity of one turned out 19.083; of another 19.136, and of a third 19.240.

Remark. It appears from these experiments, that platina, like gold and silver, is entirely indestructible by lead; that probably the purer grains, or fragments, have some heterogeneous admixtures, which are separated in these operations; and that, perfectly pure, it is more ponderous than gold, since, when mixed with a considerable proportion of a lighter metal, it fell very little short of the gravity of pure gold. There is no reason to suspect any increase of its specific gravity from the mixture; since in all the compositions with platina hitherto examined, there was constantly a diminution of the specific gravity; whether the proportion of the platina was large or small, the matter melted with a quick fire, or kept in fusion for many hours.

6. A mixture of 1 part of platina and 3 of gold was cupelled with lead, under a muffle. The matter drove well for a considerable time; at length it collected into a bright hemispherical lump, which by degrees became flatter, dull, and rough. The button, on being weighed, was found to retain a considerable portion of lead.

The experiment being repeated with a mixture of 1 part of platina and 6 of gold, some part of the lead was still retained. The bead proved rounder and brighter than the foregoing, and of a good golden colour on the outside: it broke easily under the hammer, and appeared internally greyish: some of the fragments hung together by the outer golden coat.

7. Mixtures of platina and silver, submitted to cupellation, retained likewise a considerable quantity of the lead. These, in becoming consistent, formed, not a hemispherical bead, but a flat mass, very rough, and brittle, and of a dull grey colour both internally and externally.

Cupellation and Scorification of Bismuth with Platina.

Mixtures of platina with bismuth, a metallic substance, in some respects more active than lead, were cupelled under a muffle, scorified in assay-crucibles, tested before the nose of a bellows. In numerous repetitions of these experiments, the event was the same as when lead was made use of. The mixtures, which at first flowed easily, became less and less fusible, in proportion as the bismuth was driven off; and at length could not be kept fluid in an intense white heat, though they appeared, on weighing, to retain a considerable proportion of the bismuth. Nor could this semimetal, any more than lead, be entirely separated, by cupellation, from mixtures of platina with either gold or silver.

Platina cupelled with bismuth, differed little in appearance from that which had been treated in the same manner with lead. The button was more spongy, and specifically lighter.

3. *Difflation of Regulus of Antimony with Platina.*

A mixture of platina and regulus of antimony was melted, by a strong fire, in a shallow wide crucible, and the nose of a bellows directed obliquely on the surface. The matter continued to flow, and fume copiously, for some hours; at length it became consistent in an intense white heat, and scarcely emitted any more fumes, though strongly blown on. The mass, when become cold, broke easily, appeared very porous, blebby, of a dull grey colour, and weighed considerably more than the quantity of platina employed.

Platina was likewise treated with crude antimony: and the regulus obtained from this mixture, difflated as the foregoing, with the same event; the platina not only resisting the antimonial semimetal, but likewise defending a part of it from the action of the fire and air, and refusing to melt, after a certain quantity had been dissipated.

4. *Deflagration of Zinc with Platina.*

A mixture of platina and zinc, exposed to a strong fire, deflagrated, and appeared in violent agitation. This continued but a little time; the matter quickly became solid, and could no longer be made to flow; or the zinc, of which a considerable proportion remained in it, to flame. The mass was very brittle, dull-coloured, spongy, and of no specific gravity.

General Remarks.

This extraordinary mineral, on which the most active fluxes, assisted by the most intense fires, have no effect, melts perfectly with all the known metallic bodies; unless arsenic, a substance impatient of a degree of heat sufficient to render itself fluid, is an exception.* All the metals take up equal their own weight; some metallic compositions more than twice their weight.

Platina appears in general to have no remarkable affinity with one metal more than with another. Lead and iron, which do not mingle together, and of which the former will take up some bodies from the latter, and the latter some from the former, seem both indifferent to platina; which, if combined with either, is not separated by the other.

Yet some substances have greater or less degrees of affinity with platina, than with other metallic bodies. Thus, from aqua regia, in certain circumstances, it throws out gold; and is itself precipitated by the other metals, which dissolve in that menstruum. From quicksilver it throws out lead; and is itself thrown out by gold.

The changes which platina occasions in the perfect metals, were examined in a former paper: its effects on the semi-metals are less remarkable. The principal are, that it increases the hardness of zinc, and the antimonial semimetal, but not of bismuth; and disposes this last to change its colour in the air, but not the others.

Its effects on the compound metals, are similar to those which it produces on the simple ones. Brass it renders white, hard, brittle, susceptible of a fine polish, and not liable to tarnish in the air, as it does the copper, and in some degree the zinc, of which this metal is composed. Mixtures of it with copper and tin are more apt to tarnish than with copper only, and less than with tin only.

All metallic substances, except gold, are exeded from platina by the simple acids: mercury is the only one separable by fire. The platina remaining after the separation of the metals, proves unfusible as at first.

Platina perfectly resists the destructive power of lead and bismuth, and the

* As mentioned in a former note, it may be brought into fusion when subjected to a sufficient degree of heat, with white arsenic.

rapacious antimonial semimetal; which last has been hitherto esteemed the severest test of gold, so as to have received the appellation of balneum solius solis, the bath which gold alone can sustain, and in which it is washed from all kinds of impurities.

Since therefore platina mixed with gold is not discoverable by any of the operations by which that metal is usually assayed or refined, nor by the hydrostatic balance; Mr. L. hoped that these papers, which contain part of the history of this extraordinary and till then unknown mineral, and the methods of distinguishing any sophistications of gold made by its means, which might otherwise have passed undiscovered, would be candidly received by the R. S. as a means of promoting that kind of knowledge, for which that illustrious body had been ever eminent.*

LXXXVII. An Explication of all the Inscriptions in the Palmyrene Language and Character hitherto published. By the Rev. John Swinton, M. A. of Christ-Church, Oxford, and F. R. S. p. 690.

Mr. S. states in this learned dissertation, that on examining the plates exhibited in the magnificent work entitled the Ruins of Palmyra, he had, by the help of the Greek inscriptions, corresponding with those in the Palmyrene character, been able to make out the Palmyrene alphabet, which he makes appear from the accompanying tables, to which is added the alphabet of the same language, as given by Spon and Gruter. For the Palmyrene inscriptions themselves, with the interpretations and the comments on them, the philological and antiquarian reader is referred to the original Transactions, as they would not admit of abridgment, and would have occupied too much space had they been retained entire. See these alphabets and numerals engraven in pl. 14.

LXXXVIII. Extract of a Letter from John Lining, M. D. of Charlestown, in South Carolina, to Charles Pinckney, Esq. in London: with his Answers to several Queries sent to him concerning his Experiment of Electricity with a Kite. Dated Charlestown, Jan. 14, 1754. p. 757.

Inclosed are answers to the queries sent me concerning the experiment with the kite. Since making that experiment last May, I have not had an opportunity of making any more, having been confined all the summer and autumn with the gout, which perhaps prevented my meeting with the same unhappy fate with Professor Richman of Petersburg. It appears that the professor had a wire, which came down from the iron rod, erected on his house, through the gallery-ceiling, to an iron bar, which stood in a glass vessel, filled with water and filings of brass; and that the professor stood so near that iron rod, that his face was within a foot of it. Now if there was no wire that went from that iron rod, or from any part of the wire above it, into the earth, it is no great wonder that the professor was killed. I should be extremely glad to be informed, whether the iron rod on his house, at the time the experiment was made, had any communication, by means of metal, with the earth. For if it had, there is then more danger attending these experiments than I imagined. It is likewise said in the account, that from the electrical needle, which he observed, there was no danger. I am at a loss to know what that electrical needle was, and should be glad to be informed. I know that a magnetic needle placed on a sharp point on the prime conductor, as soon as the conductor is sufficiently electrified, will move round with so great rapidity, that in the dark the electricity, thrown off from both poles of the needle, will appear like a circle of fire.

Answers of Dr. Lining to the Queries sent to him.

Query 1. In what manner, and of what materials, was your kite, and the string by which you flew it, made? and to what height did it rise above the earth?

Answer. The kite, which I used, was made in the common way; only, instead of paper, I covered it with a silk, called alamode. The line was a common small hempen one of 3 strands. A silk line, except it had been kept continually wet, would not conduct the electricity; and a wire, besides other inconveniencies, would have been too heavy. I had not any instrument, to take the height of the kite; but believe it was at least 250 feet high. It was flown in the day-time.

Query 2.—You say also, " *All* the electrical fluid, or lightning, was drawn from the cloud, and discharged in the air; and a greater degree of serenity succeeded, and no more of the awful noise of thunder, before expected, was heard." Now I should be glad you would inform us, whether the serenity in the air was such, as generally follows, after the clouds in the summer thunder-storms have discharged several loud thunder-claps; and whether any flashes of lightning appeared in the skies, after you had discharged the cloud of its lightning by the kite, as commonly do after a thunder-storm is over in a summer's night? for if there were no appearance of such flashes, then I think your assertion, that *all* the electric fluid or, lightning, was drawn from the cloud, stands fully proved; but if there were such flashes after, I conceive there must have been some of the electrical matter left behind.

Answer.—During the time of my drawing the lightning from the cloud, and for some little time afterwards, it rained; by which means, the body of the cloud being diminished, a greater degree of serenity necessarily succeeded; and the quantity of lightning extracted from the cloud, or rather its atmosphere, proved sufficient to prevent any thunder in town that afternoon; though there was a great appearance of thunder before the kite was raised. But whether the same serenity succeeded, as frequently happens after a thunder-storm, and whether there were any flashes of lightning seen in the evening, I cannot now recollect. If such flashes had afterwards been seen in the skies, as is common in a summer's evening, especially after a thunder-storm, those might proceed from other clouds, which had passed the town, at too great a distance to be acted on by the kite.

Electrified clouds have an electrical atmosphere, as well as the prime conductor, when it is electrified; and the diameter of that atmosphere, cæteris paribus, will bear some proportion to the size of the cloud. My smallest prime conductor is 2½ inches in diameter; and when it is fully charged, its atmosphere extends to the distance of about 3 feet from the surface of the conductor. How great then must the extent be of the atmosphere, which surrounds a large cloud fully electrified? It perhaps may extend to many hundreds of feet round the cloud, and may even reach so low as to touch the surface of the earth: and when that is the case, a man, or a rod of metal, placed on a cake of resin on the ground, may be electrified, and yield sparks of fire. When a sharp point is presented to that atmosphere, it cannot deprive the cloud of its whole quantity of electricity, except the sharp point be so near, that the cloud may explode upon it; and in that case the cloud must have a communication with the ground, by means of some non-electric body. Suppose an electrified cloud to have an atmosphere, which extends round it to the distance of 90 feet from its surface; and let that atmosphere be divided into 3 parts, A, B, and C, each 30 feet in diameter: now if a sharp metalline point erected on a kite, or otherwise, be placed either vertically or horizontally in the most interior part of the atmosphere C, that point will continue to act till a quantity of the lightning is drawn off, equal to the quantity contained in that atmosphere, and no longer. For then the semidiameter of the atmosphere being reduced to 60 feet, every part of it is above, and not in contact with, the sharp point, and consequently beyond its sphere of action. But let the sharp point be then advanced into the atmosphere B, and it will act as before, &c.

The truth of this, however contradictory it may be, to the general opinion of the action of sharp points, in drawing off the electricity or lightning,* may be illustrated by the following experiments on the prime conductor. Electrify the prime conductor in a dark room, and draw back the globe to a sufficient distance from the prime-conductor, to prevent its being supplied with any more electri-

* Mr. Franklin says, speaking of sharp points, "At whatever distance you see the light, you may draw off the electrical fire." page 2.—Orig.

city from the globe, while you are taking off the electrical atmosphere with a sharp point. Bring then a sharp point, either vertically or horizontally, or in any other direction, within 2 feet of the prime conductor; and the point, for some time, will appear luminous. After that light disappears, advance the point 3 or 4 inches nearer to the conductor, and you will observe the same phenomena as before; and by advancing the point gradually in this manner, as the light on it disappears, the point will be alternately luminous and dark, till you have taken off the whole atmosphere in different laminæ. As the point appears more and more luminous, the nearer that it approaches the prime-conductor, the electrical atmosphere may have different degrees of density, being perhaps denser near the prime-conductor, and rarer at a greater distance from it. If a phial be charged on the prime-conductor, when this experiment is made, the light on the sharp point will be much greater, and continue longer.

Query 3.—Did you make any trial, at what distance you could kill an animal with a discharge of the electrical fluid from the key or the bottle suspended to it?

Answer.—I have not hitherto had an opportunity of making any such experiment with the kite. But as to the first, I apprehend, that no animal could be killed by the discharge of any quantity of electricity accumulated on the key; as the key in that experiment answers the same end as the prime-conductor, and, like it, is capable of receiving *only* a certain charge of electricity, except the lightning flows down the line too fast, or the kite be so near the cloud that it may explode, when one standing on the ground approaches the key to draw sparks from it: but such an explosion would probably be fatal to the operator. When a phial is suspended to the key, after it has received its charge, if you let it continue hanging on the key, the surcharge will fly off from the hook of the phial, and the phial, when charged in that manner, will not give a greater shock than if it had been charged in the common way with the globe.

LXXXIX. *An Answer to Dr. Lining's Query relating to the Death of Professor Richman. By Mr. William Watson, F. R. S.* p. 765.

Dr. Lining's letter of the 14th of January 1754, being communicated to the Royal Society by Charles Pinckney, Esq. that learned body referred it to Mr. Watson, one of their members, in order that the best information, that could be procured on this subject, should be transmitted to Dr. Lining, for whose correspondence the Society had for many years had a very particular attention. Mr. Watson imagined, that it would be agreeable to Dr. Lining, as his abode is so remote from Petersburg, where the accident happened, to have transmitted to him not only the answer to what he more particularly requests, but also as general an account of every thing relating to so uncommon an accident, as could be procured.

The description of Professor Richman's apparatus, as sent by himself to Professor Heinsius of Leipsic, he called an electrical gnomon. To the construction of this gnomon were necessary a rod of metal, a glass jar, a linen thread of a foot and half in length, to one end of which was fastened half a grain of lead, and a quadrant. The rod of metal CD, fig. 5, pl. 11, was placed in the glass vessel E, which contained filings of metal. The linen thread CG was fastened to the rod at C, and, when the apparatus is not electrized, hangs perpendicularly down. The radius of the quadrant, which was divided into degrees, was 2 lines more than a foot and half in length. And here must be added an account of the other part of the apparatus, which was to communicate the electricity to the gnomon during a thunder storm. Through a glass bottle, the bottom of which was perforated, passed an iron rod, which was kept in its place by means of a cork fitted to the mouth of this bottle, through which cork likewise was inserted the iron rod. A tile was removed from the top of the house; and on this opening was placed the bottle, supported by the neighbouring tiles, in such manner that one end of the iron rod was not only 4 or 5 feet above the top of the house; but the other end, which came through the bottom of the bottle, did no where touch the tiles, or any other part of the house. To this end of the iron rod was fastened an iron chain, which was conducted into the chamber of Professor Richman, on electrics per se, so as no where to touch the building. The entrance to this chamber faced the north; and at the south end of it there was a window, near which stood a table 4 feet in height. On this the Professor placed his electrical gnomon, and connected it with the chain, which was brought under the ceiling of the room over this table, and communicated with the apparatus on the top of the house, by means of a wire BC, which hung from the chain, and was joined there to AB, by the little ring B, and communicated with the rod DC at C. When the iron rod at the top of the house was affected by the thunder, or otherwise suitable condition of the atmosphere, the thread before-mentioned deviated from the perpendicular; as it would also do, if artificially electrized. The Professor always observed a greater ascent of the thread from artificial electricity than by that from the atmosphere. By the former, he had seen it on the quadrant describe an angle of above 55°, but never above 30 by the latter. In the year 1752, Aug. 9, the apparatus acquired so great a degree of electricity from the atmosphere, that from the end of the rod the electrical flashes might be heard at several feet distance. Under these circumstances, if any one touched the apparatus, they felt a sharp stroke in their hand and arm.

Professor Richman sometimes added to this apparatus a glass bottle of water, after the manner of Professor Muschenbroek HI, adapted to a vessel of metal IK, placed on glass. The wire from the mouth of the bottle of water HL, during the time of the thunder, he caused to communicate with BC. From this addition he found the electricity from the atmosphere more vehement than it was without it. This he first observed on May 31, 1753, when the electrical fire exploded with such a force, that it might be heard at the distance of 3 rooms from the apparatus. On the left hand of the bottle was placed a 2d electrical gnomon. When this was made use of, the wire of metal BC, and the wire HL, were connected with MBL, a prime conductor from an apparatus for artificial electricity, viz. a glass globe, &c. At the same time also, from the chain AB was fastened a piece of wire BK, in contact with the vessel IK. By these means, when the electrical machine was put in motion, both the electrical gnomons were electrized: but this went off in a great measure, as soon as the motion of the machine ceased. By this whole apparatus taken together, Professor Richman observed a kind of reciprocation in the effects of electricity; for at first, when the electrical machine was put in motion, both the linen threads CG and CG arose with the degrees of their respective quadrants. If then the wire BC of the right gnomon was touched, the thread CG collapsed to the rod CD; but the thread on the left side continued diverging as before the touch. Also, if the wire BC of the left gnomon was touched, then in its turn the thread CG at the rod CD of the right gnomon collapsed, and the thread of the right gnomon ascended again. This reciprocation of the ascending and descending of the thread, might be repeated 3 or 4 times without exciting the machine anew.

The ingenious and industrious Professor Richman lost his life on the 6th of August 1753, as he was observing, with Mr. Sokolow, engraver to the Royal Academy at Petersburg, the effects of electricity on his gnomon, during a thunder storm. As soon as his death was publicly known, it was imagined that the lightning was more particularly directed into his room by the means of his before-mentioned apparatus. And when this affair was more inquired into, this opinion appeared to be not ill-founded; for Mr. Sokolow saw that a globe of blue fire, as large as his fist, jumped from the rod of the right gnomon CD, towards the forehead of Professor Richman, who at that instant was at about a foot distance from the rod, observing the electrical index. This globe of fire, which struck Professor Richman, was attended with a report as loud as that of a pistol. The metal wire BC was broken in pieces, and its fragments thrown on Mr. Sokolow's clothes, from their heat burnt marks of their dimensions on them. Half of the glass vessel E was broken off, and the filings of metal in it were thrown about the room. Hence it is plain, that the force of the lightning was collected on the right rod CD, which touched the filings of metal in the glass vessel E. On examining the effects of lightning in the Professor's chamber, they found the door-case split half through, and the door torn off, and thrown into the chamber. The lightning therefore seems to have continued its course along the chain, conducted under the ceiling of the room; but that it came from the apparatus at the top of the house to the door, and then into the chamber, does not, as far as can be collected, appear.

If indeed it could be ascertained, that the lightning, which was the death of Professor Richman, was collected on the apparatus, for this reason, because these bodies, at the instant of the lightning, were capable of attracting and retaining the electricity, it would then be in our power sometimes to divert the effects of lightning. But of this fact, more time and longer experience must acquaint us with the truth.

Hence Mr. Pinckney may acquaint Dr. Lining, that in Mr. Watson's opinion, at the time Professor Richman was killed, his apparatus was perfectly insulated, and had no communication with the earth, by the means of metallic or other substances, readily conducting electricity, and that the great quantity of electricity, with which, from the vastness of the cause, the apparatus was replete, discharged itself through the Professor's body, being the nearest non-electric substance in contact with the floor, and was unfortunately the cause of his death. This, it is presumed, would not have happened, had the chain, or any other part of the apparatus, touched the floor, by which the electricity would have been readily communicated to the earth.

Since the reading of the above to the Royal Society, a treatise in Latin, intitled, Oratio de Meteoris vi Electrica Ortis, by Mr. Lomonosow, of the Royal Academy of Sciences at Petersburg, has been transmitted to the Society. By this, among many other curious facts, we have been informed of certain particulars in regard to the death of Professor Richman; of which the following may not be improper to be inserted here.

Mr. Lomonosow observes, that with regard to the sudden death of the gentleman before-mentioned, the accounts, communicated to the public, contained some circumstances not fairly stated, and others of some importance were entirely omitted. With regard to the first, it is incontestably true, that the window, in the room where Professor Richman was, (a) fig. 6, had continued shut, that the wind might have no effect on his electrometer; but that the window in the next room (efdg) was open, and the door (d), between these two rooms, was half open; so that the draught of air might justly be suspected to have followed the direction of the iron conductor of the Professor's apparatus; that this conductor came from the top of the house at (i), and was continued to (h) and (b). 2dly. That this conductor was not placed far from that door-case, part of which was torn off. 3dly. That at this time no use was made of the Leyden bottle, mentioned in the preceding account; but the iron was inserted into a glass stand, to prevent the dissipation of the electrical power, and that the gnomon should show its real strength.

With regard to the 2d, there has as yet been no mention, that Professor

Richman, at the time of his death, had 70 rubles (a silver coin) in his left coat-pocket, which by this accident were not in the least altered. 2dly. That his clock, which stood at (f), in the corner of the next room, between the open window and the door, was stopped; and that the ashes from the hearth (g) were thrown about the room. 3dly. That many persons without doors declared their having actually seen the lightning shoot from the cloud to the Professor's apparatus at the top of his house. A view is likewise added of the chamber, (fig. 7) where the Professor was struck by the lightning: who stood at (h), with his head projecting towards (g) his electrometer; at (m) stood Mr. Sokolow the engraver; from the door (c) a piece was torn off, and carried to (d); (ab) part of the door-case rent.

In this treatise Mr. Lomonosow, among other phenomena of electricity, takes notice, that he once saw, in a storm of thunder and lightning, brushes of electrical fire with a hissing noise, communicate between the iron rod of his apparatus and the side of his window; and that these were 3 feet in length, and a foot in breadth. Effects like these no one but himself has had the opportunity of observing.

XC. Extract of a Letter from John Henry Winkler, Professor of Natural Philosophy at Leipsic, and F. R. S. relating to two Electrical Experiments. Dated Leipsic, May 22, 1754. p. 772.

On January 8, he sprinkled a plate of metal with the seeds of club-moss.* To this plate he connected a chain, which communicated with the coating of the Leyden bottle of water. He afterwards sufficiently electrized this water, to make the artificial thunder, of which he gave an account in his treatise De Avertendi Fulminis Artificio, p. 10 and 11. Having drawn these seeds together on a heap on the plate, he brought over them the sphere of metal, the size of which is arbitrary, impregnated with this electricity. On bringing this sphere near the plate, the electricity exploded, by which the seeds were set all on fire. These seeds were dry, and had no inflammable spirit mixed with them. The flame which arose from these seeds was true fire, as it lighted some flax, which lay on the seeds, and extended itself beyond the metal.

Jan. 13 he put some aurum fulminans on a circular piece of parchment: this parchment he cemented to a plate of metal, and caused the bottle replete with electricity to be discharged on it. Immediately the aurum fulminans exploded with a very loud report, and the circle of parchment was torn all to pieces.

* Lycopodium, club-moss, wolf's-claw.—Orig.

XCI. Of a Fire-ball, seen at Hornsey. By William Hirst, F. R. S. p. 773.

This phenomenon Mr. H. saw on Feb. 26, 1754, about 5 minutes before 11 at night. He was then going down the hill adjoining to the south side of Hornsey church, and was not a little surprised to find himself suddenly surrounded with a light equal to that of the full moon, though the moon, which was then 4 days old, had been set about 50 minutes. He had a distinct, though short, view of a ball of fire, which at first appeared to be about 15° high, w. by N. Its descent was not exactly perpendicular, but made an angle of about 80° with the s. s. w. part of the horizon, moving from left to right, so that when it went below the horizon, its bearing from him was w. N. w. It moved with great velocity, not continuing visible much longer than 2 seconds; though he did not lose sight of it till it descended below the horizon. But short as this duration was, its shape might be well discerned. The diameter of the nucleus, or head of the meteor, appeared to be equal to the semidiameter of the meridional full moon, and the tail, which terminated in a point, seemed not longer than twice the diameter of the nucleus. And its track in descending seemed to be but about 10° from the vertical; as the position in fig. 8, pl. 11. This meteor was not attended with any noise, nor left any luminous stream after its descent below the horizon. The appearance of such meteors at that cold season of the year is the more extraordinary, as their generation is attributed to exhalations caused by heat, or the action of the sun; for which reason they are generally seen after hot sultry weather.

By the distinctness and red fiery colour of this phenomenon, he imagined that it was not very high in the atmosphere; but should be induced to think otherwise, if credit is given to an account from Dublin, which states that a like meteor was seen there between 10 and 11 that night, which illuminated the whole hemisphere, and continued about 4 seconds. The near agreement in these two accounts, as to the situation and time (allowing for the difference between the meridians of Dublin and Hornsey), it being nearly half an hour past 10 at Dublin when 11 here, makes it very probable that it was one and the same meteor; which, if so, is a proof that its height in the atmosphere must be very considerable.

XCII. A Comparison between the Notions of M. de Courtivron and Mr. Melvil, concerning the Difference of Refrangibility of the Rays of Light. By Mons. Clairaut, Memb. of the R. Acad. of Sciences at Paris, and F.R.S. p. 776.

M. Clairaut observes that both the above-named gentlemen, Mr. Melvil and Mr. de Courtivron (the former in a paper lately printed in these Transactions, and the latter in a book published by him in 1752) had thought of accounting for the difference of refrangibility, by the difference of velocity in the rays of light; which, if it really agreed with the observations, would give a great simplicity to the theory of refraction, as reducing it under the same laws as the theory of gravity; whereas on the hypothesis, in which the particles of light are endowed with tendencies different from each other, we are obliged to multiply the properties of matter.

Messieurs de Courtivron and Melvil went so far the same ways, as to examine, whether the immersions and emersions of Jupiter's satellites could not afford the means of distinguishing the difference of velocities between the rays of several colours. In fact, if, according to that hypothesis, the red rays were swifter than the others, it possibly might happen that the satellite would appear of a reddish colour in the beginning of the emersion; viz. before the full time required for the whole transmission of light from the satellite to us. As to the examination of the number of seconds between the propagation of the red and violet rays, the two authors differ widely; and M. Clairaut thinks, that Mr. de Courtivron's calculations are more surely grounded than the others.

Mr. Melvil supposes, that the difference of velocity between two sorts of rays must be very nearly as the difference of their sines of refraction, when their sines of incidence are the same. Whence he concludes that, as the sine of refraction of the red rays is about $\frac{1}{77}$ greater than the sine of refraction of the violet ones, the velocity of the first rays must also exceed the velocity of the second by about $\frac{1}{77}$. He indeed gives those proportions as only being nearly the same; for, says he further, to know exactly the ratio of the velocities from the sines of refraction, the following problem should be resolved, which he proposes to the learned: "If two bodies fall, in equal angles of incidence, on a space terminated by parallel planes, in which any power acts perpendicularly to the planes (according to the hypothesis in prop. 94, lib. 1, of the Principia), the ratio of the sines of the emergence to the common sine of incidence, and consequently to each other, being given, to determine the proportion of their velocities at the time of their incidence on the first plane."

But as the investigation of the curve described by the rays of light, in any hypothesis of attractive power, has been published long ago (at least by M. Claraut in 1738), and by such a method as leads to the solution of Mr. Melvil's problem, he doubts not but if he had seen that method, he would have resolved the problem which he proposes, and perceived what a considerable difference there is between the proportion of the velocities, and that of the sines of refraction.

M. de Courtivron, who has made use of M. Claraut's solution, arrived at the following result: If p denote the ratio of the sines of incidence to the sine of refraction for one of the colours, and q the same ratio for any other, then $\sqrt{1 - qq}$ to $\sqrt{1 - pp}$ will express the ratio which the velocity of the first rays bears to the velocity of the others. Now, to make use of such a theorem, if p and q be made equal to $\frac{77}{50}$ and $\frac{78}{50}$, which are the proportions between the sines of incidence and refraction for the red and violet rays, the ratio of the velocities sought will come out in even numbers, that of 45 to 44, which differs entirely from Mr. Melvil's.

Thus, if Mr. Short's observations have led him to conclude, from Mr. Melvil's principles, that the difference of refrangibility cannot be caused by the difference of velocities, when the motion of light is performed in the manner of a projectile, how surer may not his assertion be according to M. de Courtivron's calculation, since they give a difference of time considerably greater?

XCIII. On some New Electrical Experiments. By John Canton, M. A., F.R.S. p. 780.

The resinous and vitreous electricity of Mr. Du Fay, which arose from his observing bodies of the one class to attract, what those of the other would repel, when each were excited by attrition, received no light till the publication of the second part of Mr. Franklin's experiments; where it appears, that the one kind of bodies electrify positively, and the other negatively; that excited glass throws out the electric fire, and excited sulphur drinks it in. But no reason has yet been assigned, why vitreous bodies should receive, and resinous bodies part with this fire, by rubbing them. Some persons indeed, of considerable knowledge in these matters, have supposed the expansion of glass, when heated by friction, to be the cause of its receiving more of the electric fluid than its natural share; but this supposition cannot be made with regard to bodies of the other sort, such as sulphur, sealing-wax, &c. which part with it when treated in the same manner. The following experiments, first made at the latter end of December 1753, and often repeated since, may perhaps cast new light on this difficult subject.

Having rubbed a glass tube with a piece of thin sheet-lead and flower of emery mixed with water, till its transparency was entirely destroyed; after making it perfectly clean and dry, Mr. C. excited it with new flannel, and found it act in all respects like excited sulphur or sealing-wax. The electric fire seems to issue from the knuckle, or end of the finger, and to spread itself on the surface of this tube, in the beautiful manner represented at A and B in fig. 1, pl. 14.

If this rough or unpolished tube, be excited by a piece of dry oiled silk, especially when rubbed over with a little chalk or whiting, it will act like a glass tube with its natural polish. And in this case, the fire appears only at the knuckle, or end of the finger; where it is very much condensed before it enters; as at A and B in fig. 2.

But if the rough tube be greased all over with tallow from a candle, and as much as possible of it wiped off with a napkin, then the oiled silk will receive a kind of polish by rubbing it, and after a few strokes, will make the tube act in the same manner as when excited at first by flannel.

The oiled silk, when covered with chalk or whiting, will make the greased rough tube act again like a polished one: but if the friction be continued till the rubber is become very smooth, the electric power will be changed to that of sulphur, sealing-wax, &c.

Thus may the positive and negative powers of electricity be produced at pleasure, by altering the surfaces of the tube and rubber; according as the one or the other is most affected by the friction between them; for if the polish be taken off one half of the tube, the different powers may be excited with the same rubber at a single stroke. And the rubber is found to move much easier over the rough, than over the polished part of it.

That polished glass electrizes positively, and rough glass rubbed with flannel negatively, seems plain, from the appearance of the light between the knuckle, or end of the finger, and the respective tubes; but yet may be further confirmed by observing that a polished glass tube, when excited by smooth oiled silk, if the hand be kept at least 3 inches from the top of the rubber, will at every stroke appear to throw out a great number of diverging pencils of electric fire, as in fig. 3; but not one was ever seen to accompany the rubbing of sulphur, sealing-wax, &c. nor was Mr. C. ever able to make any sensible alteration in the air of a room, merely by the friction of those bodies; whereas the glass tube, when excited so as to emit pencils, will, in a few minutes, electrify the air to such a degree that, after the tube is carried away, a pair of balls, about the size of the smallest peas, turned out of cork, or the pith of elder, and hung to a wire by linen threads of 6 inches long, will repel each other to the distance of $1\frac{1}{8}$ inch, when held at arm's length in the middle of the room. But their repulsion will decrease as they are moved toward the floor, wainscot, or any of the furniture; and they will touch each other when brought within a small distance of any conductor. Some degree of this electric power sometimes continues in the air above an hour after the rubbing of the tube, when the weather is very dry.

The electricity from the clouds, in the open air, may be discovered in the same manner, if the balls be held at a sufficient distance from buildings, trees, &c. as he had several times experienced, by a pair which he carried in a small narrow box with a sliding cover, fig. 4, so contrived as to keep their threads straight, and that they may be properly suspended, when let fall out of it; and these balls will easily determine whether the electricity of the clouds or air be positive, by the decrease, or negative, by the increase of their repulsion, at the approach of excited amber or sealing-wax.

To electrify the air, or moisture contained in it, negatively; he supported by silk, between two chairs placed back to back, at the distance of about 3 feet, a tin tube with a fine sewing needle at one end of it; and rubbed sulphur, sealing-wax, or the rough glass tube, as near as can be to the other end, for 3 or 4 minutes. Then will the air be found to be negatively electrical; and will continue so a considerable time after the apparatus is removed into another room.

The air without-doors is sometimes found to be electrical in clear weather; but never at night, except when there has appeared an aurora borealis, and then but to a small degree. How far positive and negative electricity in the air, with a proper quantity of moisture between, to serve as a conductor, will account for this, and other meteors sometimes seen in a serene sky, he leaves to the curious in this part of natural philosophy to determine. That dry air at a great distance from the earth, if in an electric state, will continue so till it meets with such a conductor, seems probable from this experiment: an excited glass tube with its natural polish, being placed upright in the middle of a room, by putting one end of it in a hole made for that purpose in a block of wood, will generally lose its electricity in less than 5 minutes, by attracting to it a sufficient quantity of moisture, to conduct the electric fluid from all parts of its surface to the floor. But if, immediately after it is excited, it be placed in the same manner before a good fire, at the distance of about 2 feet, where no moisture will adhere to its surface, it will continue electrical a whole day, and how much longer he knows not. It may not be improper to mention here, that if a solid cylinder of glass be set before the fire till quite dry, it may as easily be excited as a glass tube, and will act like one in every respect; the first stroke will make it strongly electrical.

XCIV. On the Effects of Electricity in the County Hospital at Shrewsbury. By Cheney Hart, M.D. p. 786.

They tried the effects of electricity in many different cases, though with little success, except in the case of one woman, whose left arm had been paralytic some years, and remained so, notwithstanding all the endeavours used to remedy it, so that it was absolutely motionless, and senseless of heat, cold, or pain. This patient had her arm electrized frequently, and the sparks were drawn from it, and the greatest blows given to it, for many days successively, by which in about 8 or 9 days time her arm grew sensible of pain and warmth, &c. and she had some little motion of her fingers, being able to grasp any thing with her arm down, or before her; but she could not lift it up to her head any better. This encouraged them to continue the electrizing 3 or 4 weeks longer; in which time she had got some little strength in her arm, could open and shut her fingers, and lift it half way to her head: but the pain she had from the electrizing, and the fear that increased continually of new shocks, made her obstinately resist using it any longer; and she chose, she said, rather to remain paralytic, than undergo such operations any more: so that she was discharged out of the Infirmary, with such little relief as above mentioned, and Dr. H. never has heard more of her. He wished she had tried it a while longer, as it bid so fair to do her service; and this was the only case, which gave any reasonable hopes from its use.

Another young girl, about 16, whose right arm was paralytic, on being electrized the 2d time, became universally paralytic, and remained so about a fortnight; when the increased palsy was removed indeed by the medicines which her case indicated; but the first diseased arm remained as before. However, notwithstanding the former bad accident, he had a mind to try the electricity on her again, which he renewed, and after about 3 or 4 days use, she became the 2d time universally paralytic, and even lost her voice and tongue, and with difficulty could swallow: this confirmed him in opinion, that the electric shocks had occasioned these symptoms. He therefore omitted it, and the girl, though she got better of her additional palsy, remained as bad as before of her first; and after about 4 months repeated course of medicines of different kinds on her, she was discharged incurable.

These were the only 2 cases worth noticing, that had occurred, in which it could be said to have produced any remarkable effects at all: for on numbers of others, that had experienced it, Dr. H. observed nothing happen, except that when the affected palsied limb was touched with the electrical conductor, a convulsive motion was produced immediately; but this was over very soon, and they had all remained as motionless and bad as before.

XCV. On the Number of Inhabitants within the London Bills of Mortality. By the Rev. Wm. Brakenridge, D. D., F. R. S. p. 788.

Dr. B. consulted the yearly bills of mortality for the last 50 years, viz. from 1704 to 1753, which he imagines sufficient for the purpose; and from them he extracted all the numbers of the baptisms and burials, both within the walls of London, and at large within the bills of mortality. And because it may be surer to compute from a number of years taken at an average, than from the numbers in any one year as they stand in the bills; he took the sums of the numbers, for each 5 years of the 50, and then the 5th part of each of these sums: which will at a medium be the number for any particular year. And in like manner, he took the sums of the numbers for each 10 years, and the 10th part of each of the sums will be the number for any year, at an average. And the numbers so found appeared thus:

Years.	In the City only. Baptisms.	Burials.	In the whole Bills of Mortality. Baptisms.	Burials.	
1704 to 8	1870	2553	15867	22103	Average of 5 Years.
1709—13	1805	2551	15288	21701	
1714—18	1890	2706	17586	24641	
1719—23	1871	2719	18360	26978	
1724—28	1829	2727	18442	27670	
1729—33	1578	2532	17452	26267	
1734—38	1406	2242	16762	26165	
1739—43	1221	2397	15034	28219	
1744—48	1062	1989	14402	23884	
1749—53	1087	1790	14850	22006	
1703—13	1837	2552	15577	21602	Average of 10 Years.
1714—23	1880	2712	18073	25809	
1724—33	1703	2647	17920	27168	
1734—43	1313	2320	15898	27192	
1744—53	1074	1890	14626	22945	

Where the numbers are ranged in 5 columns. The first denotes the years, the 2d and 3d the baptisms and burials within the city walls, and the 4th and 5th show the baptisms and burials at large within the bills. Thus, for instance, 22945 is the number of burials, at a medium, for any of the 10 years within the bills from 1744 to 1753 inclusive. And, in like manner, 1221 is the number of baptisms for any year, at an average for 5 years, from 1739 to 1743 inclusive, and so of others. The numbers above the line are computed for 5 years, and those below are for 10.

In the burials, it is always to be considered, that there are perhaps 2000 more than what the bills represent them. For there are burying-grounds belonging to the Protestant Dissenters, the Quakers, and the Jews, that are very considerable, of which there is no account taken. In the first of which, in Bunyan-fields, he was informed there are about 400 burials in the year, and in the others together there may be about 400 more, which sum of 800 we may suppose comes from all parts within the bills. But Dr. B. thinks the one-half, viz. 400, must at least come from within the city, where there are most Protestant Dissenters and Jews. So that 400 may always be added to the burials, within the city. Also both from within and without the city, a great many burials go out into the country, of which no notice is taken. But if we were to suppose that there are 1200 in the whole, carried out into the country, over and above the 800 mentioned above, in the burying-grounds, he thinks that to be the outmost. And therefore in the calculations he supposes 2000 burials yearly, more than in the bills at large.

It is next to be observed, that in the bills the baptisms are always about two-fifth parts, at least, less than the burials, with the numbers added to them above-mentioned; and that this difference within the city seems continually to increase, so that it is much greater now, than it was some years ago; which appears plainly to arise from two causes; the number of Dissenters of various denominations, and the multitude of people that live unmarried. But he thinks it is rather owing to the last: for, in London and Westminster, the one-half of the people at least live single, that are above 21 years of age; which must prevent almost as many more births, that might be reasonably expected. And this is not mere conjecture; for he had some proof from a particular detail of one parish within the city; where the greater part of those that are above that age are single. In the natural state of mankind, it seems plain that the number of births should be greater than the burials, and he believes that in many parishes in the country they are nearly double. He found it so in the Isle of Wight, where he lived some time, and had an opportunity to see their registers; for there the births were generally nearly double. And even in London, before the great fire in 1666, it appears, from some parish registers, that the baptisms were nearly about equal to the burials, but never afterwards: the reason of which he does not understand, unless it be that more people were then married, and that from that time there was a greater confluence of strangers: for there certainly were more dissenters at that time than ever after.

It is further to be observed, that in the bills from the year 1704 to the year 1728, without the city, both the numbers of christenings and burials continually increased; and that from that time to 1743, they continued nearly the same; but that after 1743 they gradually decreased till this time; which plainly shows, that the inhabitants were increasing till about the year 1728; and that from thence to 1743, they remained in the same state nearly; but that afterwards, during the last 10 years, till 1753, they were constantly diminishing. For it is evident, that the number of inhabitants must always be in proportion, to the number of births, and burials considered together. And hence it appears, that the cities of London and Westminster were in the most flourishing state, with regard to numbers, from 1728 to 1743, and that they are now past their height, and in the same state they were in the year 1708; and the first decrease seems to have been at the beginning of the last French war, which was in 1744. Within the city walls the number of the inhabitants do not seem to fluctuate, in the same periods of time, as without; for the most numerous state of the city, appears to have been from the year 1718, to the year 1728, and then after that they have been continually decreasing: so that when they were most numerous within the walls, they were not then arrived at the height without; and when they were in the highest state without, they were diminishing in the city. Perhaps the vast number of new buildings, within the liberties of Westminster, may have in part caused this diminution. And as from the year 1718, within the city, the christenings have been so remarkably decreasing, that they are now but three-fifths of what they were at that time, and the burials are likewise diminished above one-fourth in the last 5 years; this seems to show, that the inhabitants within the city walls must be nearly one-fourth fewer, than they were in the year 1718.

Now, in order to calculate the number of inhabitants, it will be necessary to observe, that in a year in London there generally dies one person in 30. This Sir Wm. Petty has long ago observed; and Dr. B. found it to be near the truth, on consulting his parish register. For in the parish of Bassishaw, London, there are not above 800 people, as appears from an account lately given him: and the burials for the last 10 years in the whole amount to 262; which at a medium gives 26 for one year, which is the 30th part of 800 nearly. In some parishes in London there die more than in this proportion, as in St. Giles's Cripplegate; and in others in the out parts of the town there die fewer; but in general it will hold true, in and about the city. In the town of Breslaw in Germany, from which Dr. Halley formed his table for the probabilities of life, there die about 2 in 69, that is less than 1 in 34; as is plain from an easy computation. But there certainly die more than in that proportion, within the London bills; for it appears that one-third at least of the children die under 2 years of age; whereas at Breslaw there die under that age, only one-fifth; and therefore the difference being two-fifteenths, or four-thirtieths, there die 4 in 30 more at London than at Breslaw, under 2 years of age.

In the country the case is very different; for there does not die above 1 in 50, in healthy places. Sir Wm. Petty has also observed this. For in the parish of Newchurch in the Isle of Wight, where Dr. B. resided some time, there are about 900 people, and there does not die, at a medium, above 18 yearly; which is one in 50 exactly. And he believes this will be found to be nearly the same in most of the counties in Britain, where the people do not live in great towns; which shows the great difference between the effects of the air in London and the country.

If then it be allowed, that in London and Westminster there dies one in 30, it will be very easy to make a calculation of the whole number of the people nearly, that are within the bills. For if we take the number of burials at an average for some years, and multiply that by 30, the product must be the number of the people. Thus if we take the number of the burials, at large within the bills, for any one of the last 10 years, at a medium, from 1744 to 1753 inclusive, to be 22945, and add to this 2000, for those burials omitted in the bills, as is supposed above, the total will be 24945, all the burials within the limits of the bills, for one year at 1753; and then multiply this by 30, the product 748350 will be the whole number of the people nearly, at present. But if we take 27192, the number of the burials, at a medium, for any one of the ten years preceding 1743 inclusive, and add to this 2000, as above; the whole of the burials at that time within the bills will be 29192, which being multiplied by 30 gives 875760, for the number of the people at the year 1743. And therefore the inhabitants are fewer now than they were in 1743, by 127000.

If we were to try the same calculation, by taking the burials, at a medium, only for 5 years to 1753, and also for 5 years to 1743 inclusive, the difference will be greater. For the numbers at these two times will be 720180, and 906570, of which the difference is 186390; so that the people would appear fewer at 1753 than they were in 1743, by 186000. But this is not so much to be depended on as the numbers above; because there were two extraordinary bills at 1740 and 1741. Or if we should imagine that there might not more die at London than at Breslaw, that is 1 in 34, still the difference would be greater than we found at first. For taking the burials at an average for ten years, at 1753 and 1743, as above, the numbers would at these two times be 848130 and 992528, of which the difference is 144398; so that it seems plain, if the bills are to be depended on, that there is a decrease of the people since 1743 of above an hundred thousand, and that at present the number is about 740000. And this decrease has been annually continued: for if we try the thing further, at the distance of 5 years, and take at a medium for 5 years, the burials for 1753 and 1748, the numbers will come out 720180 and 776520; of which the difference is 56340, the number decreased for the last 5 years.

There is another way of computing, from the number of houses; but he thinks this not so certain as the other. For here are two difficulties; to ascertain the number of houses, and to fix on the number of persons for each house. As to the last, Sir Wm. Petty thought we might allow 8 persons to a house: which Dr. B. found to be a mistake. He made an experiment of it, and got an exact account of the numbers in each house in a certain parish in London; and he found that they exactly come to 6 in a house, empty and full together, for there is seldom above one in 20 empty. And as in that parish the people are in a middle condition, and some of them have a number of servants; it may be presumed they are in a middle state with regard to numbers, between the very great families, and those in the lowest rank. This is also confirmed, if we allow, as above, 1 in 30 to die yearly in London. For within the city walls there were 11857 houses in the 97 parishes, as appears from Mr. Smart's account, which was supposed to be very accurate at that time: but since he published that in 1741, there are not so many houses within the city walls; for in many parishes there are houses greatly enlarged, some rebuilt instead of 2 or 3; and warehouses made of others. In some parishes there is 1 in 20 fewer than in his time. In others perhaps there is no alteration. But he thinks they must, at an average, be diminished 3 in 100 at least; and consequently there are about 354 fewer, and the number of houses within the city walls is about 11503; which being multiplied by 6, gives 69018, for the number of inhabitants; which is nearly equal to the burials 2290 multiplied by 30, or 68700; taking the burials at a medium for 10 years, and adding 400 as above.

The number of houses within the bills may then be nearly come at, from the number of burials. For if we take the number of burials for the last 10 years, at an average within the city, to be 1890, and add 400, which makes 2290, we may say, if 2290 comes from 11503 houses, then the whole number 24945 of burials within the bills, having allowed 2000 as above, must come from 125302 houses. And there cannot be fewer; for there are more burials within the city, in proportion to the baptisms, than in the out-parishes; and therefore more burials in proportion to the number of houses; which shows that the number of houses cannot be less than 125302; which being multiplied by 6, will give 751812, for the number of people for this present time; and it is nearly equal to the number 748350 found above. So that the numbers produced from these 2 methods being almost equal, this is some further proof that our supposition, of 6 persons to a house, empty and full, is near the truth. But if we suppose, that the number of houses within the walls is now the same, as in Mr. Smart's time, 11857; then all the houses within the bills will be 129158, and the number of people 774948, greater than 748350, found above, by 26598; which is not much in such calculations.

Sir Wm. Petty also says, that he was informed there were 84000 houses tenanted within the bills, in the year 1682, in which he wrote, and if so, the number of houses seem to be increased near one-third since that time. And, according to our way of computing, to suppose 6 to a house, empty and full, there could not be more than 504000 people at that time; which is less than the number we found above, for the present time, 748350, by 244350. But now, instead of increasing, we are decreasing; for since the year 1743 the inhabitants have been annually diminished; by which it appears that this great city is past its height, and is rather on the decline with regard to numbers. And hence we see how far Sir William was mistaken, who imagined that it might increase continually till the year 1800; when the number of people would be 5 millions, that is near 7 times as much as they are at present.

Now, to account for this decrease, there may be various conjectures: Dr. B. thinks 3 causes may be assigned, that may all operate jointly. One may be the vicious custom that has prevailed of late years, among the lower people, of drinking spirituous liquors; another the fashionable humour of living single that daily increases; and a third may be the great increase of trade in the northern parts of Britain, that keeps the people there employed at home, that they have no occasion, as formerly, to come hither for business; and it were to be wished that this cause was the most prevailing. But whatever be the cause, it seems plain, that it could not be the late French war, as some imagine. For, by what was shown above, there has been a decrease of 56000 since the year 1749, after the peace; but if the war had been the cause, there ought rather to have been an increase after it. And as in the whole, we could not have lost more than 150000 in the war, by land and sea, of which there was not one-fifth, or 30000, taken from about the city; this can never account for 64000, the decrease before the year 1748. In the former war, between 1702 and 1711, the city never decreased, but continually increased: from which one would imagine that the last war could not diminish its numbers.

Nor can this decrease in the bills be accounted for, from a greater number than formerly leaving the town in summer; because it does not appear that there is a greater number of such, than was 10 years ago. And if it could be allowed that the number was greater, it can never be thought that it can amount to 120000 more than in the year 1743.

It is true, this decrease may appear surprizing to some, when they see the number of new buildings in Westminster, continually increasing; but then, on the other hand, it is likewise to be considered, that there are a great number of houses enlarged, or rebuilt, instead of 2 or 3 others, as mentioned above; and others falling in and empty, about the eastern parts of the city: so that for the last 20 years the inhabitants seem only to be moving, from the eastern to the western parts of the town, and not increasing.

XCVI. On a large Calculus found in a Mare. By Mr. Wm. Watson, F.R.S. p. 800.

This stone was composed of different laminæ, and its figure is that of an oblate spheroid, whose greatest diameter is 8½ inches; its lesser 8 inches. Its surface is extremely regular, but it appears in several of its parts, as though it had been corroded by some acrid menstruum; and in a place or two, where the external lamina is quite worn away, and the lamina immediately underneath it polished during its continuance in the mare, the calculus has great resemblance in colour to occidental bezoar.

It weighed in air 15 lb. 12 oz. Avoirdupois; in water 6 lb.: so that its specific gravity to that of water was nearly as 8 to 5. So that it is not only considerably lighter than any fossil petrifaction, but much more so than many animal; some human calculi, when fresh extracted, being to water as 2 to 1.

With regard to its bulk, it is the largest he remembered to have been observed, except one presented to the R.S. in the year 1737, which was taken out of the stomach of a dray-horse, belonging to Sir Henry Hickes, Knt. at Deptford, and which weighed 19lb. Avoirdupois, exclusive of the outward shell or crust, which was broken off in several pieces. Both these stones were in appearance like a pebble, and formed of different laminæ. The greatest circumference of the stone taken from this mare was somewhat more than 26 inches; that of Sir Henry Hickes's 28.

Sir Henry Hickes's horse was 22 years old; and, for 11 or 12 years before he died, frequently was observed to be in violent pain; but the mare, the subject of the present letter, though 16 years old, gave no signs of being in pain till about 3 months before her death, when she would frequently lie down, and roll about. And it is the more extraordinary that, large as the stone was, it did not disable the mare from doing her usual work for a more considerable time before her death; which did not seem to be occasioned by the stone, she dying near her foaling time; nor so far disturb her economy, as to prevent her propagating her species.

In 1746, the Duke of Richmond presented to the Society a stone found in the colon of a horse, the circumference of which was 16 inches. His Grace at the same time presented some other stones, found in the intestines of a mare, which were polished like bezoar. It was very remarkable, that 2 of these stones, when sawed asunder, were found to have been formed each on an iron nail, as a nucleus.

XCVII. On the Belemnites. By Mr. Gustavus Brander, F.R.S.* p. 803.

The belemnites is a fossil, that has hitherto perplexed the naturalists of all countries: it has been treated of by various authors, and differently ascribed to all the 3 kingdoms of nature; but Mr. B. delivers it as his opinion, that it belongs to the testaceous part of the animal kingdom, and to the family of the nautili. The nautilus, or sailor, is a concamerated shell, with a syphunculus running through every cell, see pl. 14, fig. 5. The syphunculus, and the concamerations, are the generical character of this tribe, and are supposed to serve the animal to buoy up its shell, by which means it can swim or sink at pleasure.

Those that are curved are very common, both in the recent and fossil state; the straight ones have hitherto only been met with fossil, and are common in Sweden, Livonia, and several parts of Germany; and have, by naturalists, been called orthoceratitæ; and Mr. B. had seen some in Dr. Mason's private collection at Cambridge, which he said were found at Whitby in England; the cha-

* The belemnites is in all probability a species of nautilus, and its inhabitant may be allied to that of other nautili; and consequently resemble in some degree a sepia or cuttle, which by the older writers was often called a polypus. Linneus imagined it a petrifaction of the Alcyonium Lyncurium. Mr. Brander's notion of the testaceous tribe in general having proceeded from polypes, can only have arisen from his want of zoological information.

racter of which being exactly the same with the nautili, Mr. B. makes no scruple to class them together.

Whoever will examine nicely bodies of any genera, will have a difficulty to say, where they begin, and where they end; the gradation is so insensible that they must be bewildered. From the orthoceratitæ, which is doubtless a species of nautilus, we gradually proceed to the belemnites. The orthoceratites is a straight concamerated shell, ending in a point; some of which are seen in stone 18 inches long. See fig. 6, 7, 8, 9, and 16. The nucleus, or alveolus of the belemnites, is likewise a straight concamerated shell or body, exactly resembling the other in shape and structure, but of a smaller species, fig. 10 and 14; and, from the very great analogy, may reasonably be deemed to be of the same family. In the conic cavity of the belemnites, fig. 11, that contains the nucleus, it is very common to observe visible marks of a shelly substance, as a further confirmation that the nucleus was a testaceous body.

And now a word as to the belemnites itself, the counter part to the other. It has indeed been truly matter of speculation, how that huge solid substance called the belemnites, exclusive of the nucleus, could be formed; and how it happens that some should have the nucleus within them, others not; the cavity to contain the same in some very small, in others scarcely or not at all visible.

These are the main difficulties, all which Mr. B. endeavours to elucidate; but first acknowledges his obligation to Mons. de Peysonnel, and particularly to Mr. John Ellis, F.R.S. for their curious observations on the nature of coral, on which this latter part of Mr. B.'s hypothesis is founded. They have plainly demonstrated, that many bodies which we always took to be vegetable from their appearance, are really animal, and constructed by the polype; and that several coralline substances, hitherto reputed marine plants, are thick beset with a prodigious quantity of seedling-shells, too small for the naked eye to see, close by each other, as diamonds in a bodkin, ready to come forth in due time out of their several nests or cellules; see Phil. Trans. vol. xlvii, p. 445, and vol. xlviii. p. 115. Hence he submits, if it is not highly probable that the testaceous tribe in general are generated like butterflies, and flies of all kinds, the one from a maggot, the other from a polype? Nay, it appears presumptive, that it must be so with a great many. On which circumstance he proceeds, that as corals in general, from late observation, seem to be constructed by polypi, what inconsistency then to believe them to be the primary state of all or most of the testaceous tribe? If so, it is almost beyond a conjecture, that the body called a belemnites (which, on being put into acids, is found to ferment in like manner as coral, and other cretaceous bodies), is formed likewise by a polype, from which the nucleus seems to be the ultimate state.

And he further submits, whether this concamerated shell, or body, of which the belemnites is only the habitation, does not appear a strong voucher for this new hypothesis, by more immediately leading us into the connexion and manner of generation (perhaps particular to the testaceous tribe) by remaining within its nidus all its life; whereas they generally quit them as soon as they are able to shift for themselves.

The polype is an animal of the vermicular kind; the bodies of some are long and slender, like a fine sinew or fibre, extremely tender; and from the head proceeds a variety of claws, or arms, with which it catches its food, and prepares its habitation or chrysalis. They are without doubt of various shapes and textures, according, as he supposes, to the species of the animal that is hereafter to proceed from them; and very wonderful it is, how so small, so delicate an animal, should be capable of forming so large a body as the belemnites! but is not every particular performance of nature equally the same to a diligent inquirer? Some animals in the terrestrial part of the creation, naturally associate and herd together. Others again seek solitude. The same dispositions we find impressed on those of the aquatic system: then why not among the polypi? as is evidently seen by the prodigious variety of coral bodies, where it seems in some as if thousands acted in concert together; in others, where each acts for itself; of which latter is the belemnites.

The shape of the belemnites is generally more or less conical, terminating in a point, and of various colours, according to the juices of the stratum in which it lay: it has usually a seam or fissure, running down the whole length of it, sometimes filled with a cretaceous substance. In some it is in the middle or axis of the body; in others on one side. Its interior constitution seems composed of several conoid cortices, or crusts, which, when broken transversely, appear to proceed in striæ or rays from the seam or centre; which seam he takes to have been the habitation or cell of the animal in its polype state, and in which the body was affixed; or perhaps serving as a syphunculus, in which was a ligament that proceeded from the nucleus in its perfect state.

The crusts it is composed of probably denote certain periods in the age of the animal; as the annual rings in a piece of timber, its age: but what those periods are, we are not acquainted with; see fig. 11, 12, 13, 15. The animals of the testaceous tribe in general, as they increase in age, increase their shell in thickness till they have lived their stated time, or attained to good old age; and that is done by adding a new crust or lamina to it, as several, if not all the tubuli, the oysters, and the nautili, witness; after which they become inactive and dull, the effect of extreme old age, suffering other marine bodies, as worms, oysters, &c. to penetrate and affix themselves to their outer coat. The like appearances we frequently meet with on belemnitæ, when the animal within was perhaps either waxed old, or was dead; and is an additional proof that the body is of marine origin.

Every one, who has made this part of natural history his study, must have observed, that the minutiæ, or exceedingly small fossil shells, very frequently occur, and in the greatest abundance, especially in fine loam or clay proper to preserve them: which shows that it was spring or spawning-time when the deluge overwhelmed the country they were natives of. And that diminishes one of the difficulties concerning the belemnites, why some have the cavity, others not, or but very small: for may we not attribute these several appearances to the different ages of the animal; as in the spring or spawning-time, and some time after, a thousand small fish appear in the water to one grown to maturity, or seedlings on the shores of shell-fish, to one at full growth? and, from a parity of reasoning, is there not great likelihood to believe that every belemnites would have had a nucleus, if it had lived; and to suppose that deficiency to be caused by the deluge coming on, in the early part of the season, in that spot where they are natives, before they had attained perfection?

The country of the belemnitæ is unknown to us; but there is great probability it is the same with that of the conchæ anomiæ in general, and ammonitæ; since they are usually found together, and are well supposed to be the inhabitants of deep or unknown seas, beyond human reach.

Having had so frequent occasion to mention the orthoceratitæ in the course of this subject, they being here rare and uncommon fossils, Mr. B. has given the figures of some few species of them, which perhaps may not be unacceptable, N° 6, 7, 8, 9, 16. If it should be asked, whether they proceed likewise from a belemnites? he answers, that he supposes them to proceed from a polype like the rest, but whether their parent polype formed itself a belemnites-like chrysalis or habitation, being a stouter animal, is more than he can affirm, though very probable, as the terminating point in them is as sharp and fine as the nucleus of the belemnites; and it is observed, that all the turbinated shells increase their circumvolutions from the point or apex; but that is not the immediate business of the present purpose, as nature has many ways to compass her ends. His design will be answered, if it shall only be thought, that he has evinced the belemnites to be an animal production, formed by a polype, as other coralline bodies; and its nucleus to be a concamerated testaceous body, of the nautili genus, proceeding from it.

Description of the Plate 14.

Fig. 5. A section of a common nautilus. 6, 7, 9, 16, Sections of orthoceratitæ. 8. An orthoceratites entire. 10. A section of a belemnites, with the nucleus. 11. Ditto, without the nucleus. 12. An oblique section of a belem-

nites, to show the inner structure. 13. A belemnites without a cavity, only a small perforation. 14. A nucleus of a belemnites. 15. A belemnites, with a very small cavity.

XCVIII. On the Agaric, applied after Amputations, with Regard to determining its Species. By Mr. William Watson, F. R. S. p. 811.

The agaric applied as a styptic after amputations, and which had been brought from France, Mr. W. did not believe to be the common agaric of the oak, as had been imagined by the French surgeons. What is called the common agaric is a parasitical plant, found growing on the oak, and on several other trees; and is denominated by Caspar Bauhin, fungus in caudicibus nascens, unguis equini figurâ; of which touchwood or spunk, and the amadoue ordinaire of the French, is usually prepared. He thought it impossible, by any process, to prepare from the common agaric a substance perfectly similar to the French agaric, which should exactly answer the description, which Breynius (Ephem. Nat. Curios. Ann. 45, obs. 150) gives of the fungus coriaceus quercinus hæmatodes.

Mr. Ray, in his Synopsis Stirpium Britann. on the authority of the late Dr. Sherard, says, that this fungus coriaceus quercinus hæmatodes is found on putrid oaks in Ireland, where it is called oak-leather; and that the country people there collect and preserve it to dress ulcers with. Dr. S. makes no doubt but that it may be found in England; and Mr. Ray had some sent him from the late Dr. Eales in Hertfordshire. Dr. Richardson, in Yorkshire, found it growing on the ash; and Dr. Dillenius observes, that in Virginia it is used as leather, to spread plasters on; and that, besides its being a soft substance, sitting easy on the afflicted part, it has a healing property.

XCIX. Two Letters concerning the Use of Agaric, as a Styptic. Letter 1.—*From Mr. J. Warner, F. R. S. and Surgeon to Guy's Hospital.* p. 813.

In the first of these letters Mr. W. states that he had received from Mr. Fellowes, an account of the styptic effects of the agaric, in a case under the care of Mr. Gooch, and that he (Mr. Warner) had continued to use the same application with the greatest success.

Letter 2.—*Addressed to Mr. Warner by Mr. B. Gooch.*—After returning thanks for the agaric of the oak, which he had received by Mr. Fellowes, the writer of this letter proceeds to give an account of the effects he had observed from it. Two or 3 days after Mr. G. received it, he was desired by an ingenious surgeon to be with him on business. He carried some of the agaric with him, and he was pleased with the opportunity of trying it in an amputation below the knee of a boy of about 10 years old. They applied it according to the directions given by Mr. Warner in his book, and the hæmorrhage was entirely stopped in 6 minutes. He informed him that, on the 5th day inclusive, the dressings and agaric came all off without force, and left the stump in a good digested state, without the least appearance of blood; and that the pain, in consequence of the operation, did not require an anodyne. He cut a boy for the stone the same day, and a vessel bleeding rather more than is thought allowable, he applied a very small piece of the agaric, and a soft dossil of lint over it, which, with gentle pressure of the finger, restrained the bleeding in less than a minute. His own patient, aged near 70, on whom he made trial of it, in amputating his leg below the knee, appeared as proper a subject to establish the credit of this new styptic as could be produced, if it failed not in its efficacy; there being in him a great depravation of the fluids, and a general relaxation of the solids; and he had an ulcer on his leg, of the phagedænic kind, of many years standing, attended with carious bones. Under these discouraging circumstances he applied to Mr. G. about a month before, and begged of him to take off his leg; the pain, he said, being so violent and continual, that he knew not how to live with it; and though he thought him a very bad subject for the operation, yet he did not care to deny his most earnest request, seeing no other possible means left of affording relief in his miserable condition. Considering the rigidity of the fibres in an old person, and that their natural contractile power, evident in the division of an artery, must be greatly weakened in this case, Mr. G. was afraid, that the agaric, if it should answer, would not act so expeditiously as it did in the other; and that probably they might meet with much more difficulty in restraining the hæmorrhage. Therefore, to assist it all he could, he tacked it to thick compresses of lint with pieces of card in the middle, thinking by that means he could apply it more readily, and keep it in stronger and closer contact with the mouths of the vessels, if he should find it necessary; for he was very solicitous for the support of its credit and reputation, his own being connected in some measure with it, and the patient's welfare also depending on it. He applied most of the pieces without being under a necessity of having the tourniquet-ligature slackened, to show the mouths of the vessels; then covered the stump thick with lint, applied a pledget of tow spread with common digestive over it, and over that a circular piece of stiff paper, to make the pressure of the palm of the hand more equal. This done, after 3 or 4 minutes he desired his assistant to slacken the tourniquet-ligature; on which it bled at a great rate, and made some of his brethren soon imagine, and declare, they thought it would not do in this case. Mr. G. was not without the same fears; but went on with resolution, and every thing was conducted without hurry or confusion. He desired to have the tourniquet-ligature let quite loose, in order to remove, as much as possible, all impediment to the reflux of the blood, and made a strong compression at the end of the stump, on which the bleeding almost intantly abated, which was totally stopped in about half an hour after; and, in the whole, he believed he did not lose more than 12 oz. of blood. Now, apprehending that the circular structure of the common bandage, as usually applied, might produce the same inconvenience, which he observed arose from the tourniquet-ligature before it was quite loose, if no other attended it, having strong suspicions of tight bandage doing much mischief; he therefore only put several strips of common plaster, about an inch broad, over the piece of stiff paper at the end of the stump, to meet and lap over at the top of the knee when bent, and then slipped on a barber's woollen cap; which method seemed to answer the purpose very well in this, as he had found it do in some other amputations. A physician, and 4 surgeons of eminence, who were desirous of seeing the effect of the agaric, were present at the operation. Four days after he opened the stump, but took away no more of the dressings than what were loose, among which were 2 or 3 pieces of the agaric, without any signs of fresh bleeding, or visible pulsation at the ends of the arteries. Two days after he dressed it again; the stump then appeared well digested all over, and had a much better aspect than could reasonably be expected in such an unpromising subject; and appearances were so favourable as to give hopes of his recovery, though he was not without distant fears, which he was guarding against as much as possible.

C. Extract of a Letter from Mons. Bonnet, F. R. S. of Geneva, to John Clephane, M. D., F. R. S. Translated from the French. Dated Geneva, June 3, 1754. p. 818.

The inoculation of the small-pox continues to be attended with the greatest success in our city. Of 70, who were inoculated, there was not one in any danger. Lausanne has been as it were forced to imitate us; and we hope, that this excellent method, which we received from England, will spread itself from one place to another, for the good of mankind. Mons. de la Condamine has read to the Royal Academy of Sciences at Paris a dissertation on this subject, which was greatly applauded. But I presume, that the French will be a long time in adopting the practice of inoculation. The clergy there throw a terrible obstacle in its way.

CI. Extract of a Letter from Constantinople, of the 16th September, 1754, *from Murd. Mackenzie, M. D. concerning the late Earthquake there.* p. 819.

On the 2d instant we had a terrible shock of an earthquake, about $\frac{3}{4}$ after 9 at night, which moved from east to west, and has done a great deal of mischief here, and in the neighbourhood. I shall only mention what I have seen.

Four of the 7 towers are much hurt; one of them, which is an octagon, has

2 of its sides thrown down from top to bottom. It is said several of the Janisaries, who were on guard there, are killed. The 3 other of the 4 are much shattered, and part of the walls fallen down. All the turrets, on the city wall, from the 7 towers to the Adrianople-gate, are much shattered, though none fallen; all the cupolas of the portico of Sultan Mahomet the IId's mosque are thrown down; the Sickergee Han, a strong stone building near the above mosque, is quite destroyed: some part of the wall of the Cara Han is thrown down; one bagnio is quite fallen, and many people said to be destroyed in it. The Cautirligee Han is quite down; and the Vizir Han much shattered; 7 minarets (columns from whence the people are called to prayers) of small mosques are thrown down; the mosque called Little Santa Sophia is much damaged, and the prison of Galata is quite down, and all the prisoners buried in its ruins. There has been much damage done at Balat, a large suburb, Scutari, and on the canal; and there are bad accounts from Nicomedia, but none well avouched. There have been several small shocks felt since, but none have done any harm. Some say there were 2000 people destroyed by this calamity, in the town and suburbs; some 900; and others reduce them to about 60, who, by what I have seen, are nearer the truth.

The shock at Smyrna, in the year 1739, which I also felt, was much stronger.

On the 6th, about 9 at night, there appeared a cloud due west, when it began to lighten and thunder, and the thunder continued, without any interval, till half an hour past 10, moving gradually to north-east, where it ceased, and the night was very serene and calm after it. About 10, when the thunder was north of us, it rained for a quarter of an hour very heavily, then became clear, and all the stars appeared. Such a peal of thunder I never heard in any country; for I can aver, that it did not stop a minute in an hour and a half's time.

Another letter, dated Oct. 1, says, that a Tartar was arrived express from Armenia, in 20 days, with advice, that the city of Sivas, one of the Sebastias of the ancients, was quite destroyed by an earthquake, on the same night, in which that was felt at Constantinople; and that a lake of fresh water is risen where the town sunk. The earthquake was felt at Angora and Smyrna, but there was no notice that they had felt any thing of it at Aleppo, though there were letters from it about that time.

CII. Extract of a Letter from Camillo Paderni, Keeper of the Herculaneum Museum, to Thomas Hollis, Esq. relating to the late Discoveries at Herculaneum. Dated Naples, Oct. 18, 1754. p. 821.

The first thing here discovered was a garden, in which were found several marble statues of excellent Greek artists. This route led towards a palace, which lay near the garden. But before arriving at the palace, they came to a long sqnare, which formed a kind of forum, and was adorned throughout with columns of stucco; in the middle of which was a bath. At the several angles of the square was a terminus of marble, and on every one of those stood a bust of bronze, of Greek workmanship, one of which had on it the name of the artist, ΑΠΟΛΛΩΝΙΟΣ ΑΡΧΙΟΥ ΑΘΗΝΑΙΟΣ. A small fountain was placed before each terminus, which was constructed in the following manner: level with the pavement was a vase to receive the water, which fell from above; in the middle of this vase was a stand of balustrade work, to support another marble vase. This 2d vase was square on the outside, and circular within, where it had the appearance of a scallop-shell; in its centre was the spout, which threw up the water, that was supplied by leaden pipes inclosed within the balustrades. Among the columns, which adorned the bath, were alternately placed a statue of bronze, and a bust of the same metal, at the equal distance of a certain number of palms. Seven statues were taken out from April 15 to September 30, near the height of 6 Neapolitan palms, except one of them, which is much larger, and of an excellent expression. This represents a fawn lying down, which appears to be drunk, resting on the goat-skin, in which they anciently put wine. Two other of these statues are of young men, and 3 of nymphs; all of middling workmanship. September 27, I went myself to take out a head in bronze, which proved to be that of Seneca, and the finest that has hitherto appeared, being as excellent a performance as can well be conceived. Our greatest hopes are from the palace itself, which is of a very large extent. As yet we have only entered into one room, the floor of which is formed of mosaic work, not inelegant. It appears to have been a library, adorned with presses, inlaid with different sorts of wood, disposed in rows; at the top of which were cornices, as in our own times. I was buried in this spot more than 12 days, to carry off the volumes found there; many of which were so perished, that it was impossible to remove them. Those, which I took away, amounted to the number of 337, all of them at present incapable of being opened. These are all written in Greek characters. While I was busy in this work, I observed a large bundle, which, from the size, I imagined must contain more than a single volume. I tried with the utmost care to get it out, but could not, from the damp and weight of it. However I perceived, that it consisted of about 18 volumes, each of which was in length a palm and 3 Neapolitan inches; being the longest hitherto discovered. They were wrapped about with the bark of a tree, and covered at each end with a piece of wood. All these were written in Latin, as appears by a few words, which broke off from them. I was in hopes to have got something out of them, but they are in a worse condition than the Greek. From the latter the public will see some entire columns, having myself had the good fortune to extract 2, and many other fine fragments. Of all these an account is drawing up, which will be published together with the other Greek characters, now engraving on copper-plates, and afterwards make a separate work by themselves. At present the monk, who was sent for from Rome, to try to open the former manuscripts, has begun to give us some hopes in respect to one of them. Those which I have opened, are philosophical tracts, the subject of which are known to me; but I am not at liberty to be more explicit.

CIII. Extract of a Letter from Sir James Gray, Bart. his Majesty's Envoy to the King of Naples, relating to the same Discoveries at Herculaneum. Dated Naples, Oct. 29, 1754. p. 825.

Several curious and valuable things are daily found in the mine of antiquities at Portici. They have lately met with more rolls of papyri of different lengths and sizes, some with the umbilicus remaining in them; the greater part are Greek in small capitals. The Canonico Mazocchi, who is much esteemed for his learning and knowledge of antiquity, is employed in copying and explaining 5 entire columns, that have been lately unrolled off one of the papyri, which gives some hopes of further discoveries. This manuscript treats of music and poetry. The Epicurean philosophy is the subject of another fragment, a small bust of Epicurus, with his name in Greek characters, was found in the same room, and was possibly the ornament of that part of the library, where the writings in favour of his principles were kept; and it may also be supposed, that some other heads of philosophers, found in the same room, were placed with the same taste and propriety.

Last week were found 2 fine bronze heads, of excellent workmanship, one of Seneca, and another of a captive king. The king spares no expence in recovering and preserving these valuable remains. In order to satisfy the curiosity of the public, he has ordered a catalogue to be printed, with some designs of the principal statues and paintings, which will be published soon. A more exact account of these discoveries will some time or other be given by Monsignor Baiardi, who, in 3 large 4to volumes already printed, has not finished his introduction.

CIV. Of some Trials to keep Water and Fish sweet, with Lime-water. By Stephen Hales, D. D., F. R. S. p. 826.

Dr. Alston, of Edinburgh, had found, that the small proportion of a pound of slacked lime to a hogshead of water, stirring it, effectually preserved the water sweet, not only in a glass or earthen vessels, but also in a new oaken vessel.

April 9, Dr. Hales put into a 7 gallon cask of water, in the proportion of a pound to a hogshead, some white marble lime; which was what they call sweated, that is wrapped in dung, without which sweating, it is said, that it will not be reduced to lime.—April 26. It had some taste of the wood, and a small degree of ill smell, which being something more so on July 27, it was then poured away.

July 15, He put into an 18 gallon cask 18 ounces of unslacked stone-lime, made of very hard stone of the Clee-hills in Shropshire; that is, in the proportion of $4\frac{1}{2}$ lb. of lime to a hogshead of 72 gallons.—June 25, the water was sweet, but had a disagreeable taste of the cask, and continued the same Aug. 24; but Oct. 17 the taste was something worse. And, Nov. 12, there seemed to be a very small degree of a putrid smell and taste. But the prevailing disagreeable taste was from the wood of the cask, which discoloured the water in some degree.

He put also into a 9 gallon cask 2 ounces of the same unslacked stone-lime to a gallon; which was in the proportion of 9 pounds to the hogshead, and found it much the same all along as the former.

With chalk-lime, in the proportion of 2 lb. to a hogshead, it soon stunk much, and continued so to do for 4 months. This was Thames water, taken up below London-bridge, which is well known to grow sweet again, after having stunk for some time. So that chalk-lime (almost the only sort in use here), will not preserve water from putrefaction; though stone-lime, as Dr. Alston has happily discovered, does preserve water in a great measure from the great degrees of putrefaction it is subject to, and therefore may be very serviceable at sea.

Being informed, by one who had been in the East Indies, that native mineral sulphur had been found to keep water sweet there in earthen jars, at land, and also at sea. April 2, he put into a kilderkin, or 18 gallons of pure pond water, a pound of native mineral sulphur, in 7 lumps. April 26, sweet. May 3, began manifestly to stink. May 7, stunk much, and was poured away.

May 8, the kilderkin being scalded, and made sweet, it was filled again with the same pond water, and 6 lb. of native mineral sulphur put into it. July 27, it was sweet. Oct. 17, it was discoloured, and somewhat in a small degree fetid. Nov. 12, the same. Hence native mineral sulphur may be of service to preserve water from great degrees of putrefaction at sea.

Dr. Alston having written that he found fish would continue sweet in lime-water for 7 weeks and more. April 19, Dr. H. put 4 gudgeons into white marble lime-water. May 10, they were sweet; but on boiling one of them, the flesh, though sweet, was reduced to be soft pap.

And Mons. Clairaut, who was at Lapland, to measure a degree of the earth, told Dr. H. on this occasion, that the fish, which they there kept long dried, were thus pappy when boiled, but not unwholsome. May 22, they smelled sweet, and were firm to the feeling; but on boiling one of them, it dissolved away like anchovy. June 12, another of the gudgeons, though sweet and firm to the touch, being put into new-made stone lime-water, which was only milk-warm, dissolved also, and the bones of the head were rotten and brittle.

June 18, two small eels, skinned, were put into stone lime-water. June 22, one of them, which was firm to handle, when boiled was soft and pappy. June 25, the other eel was the same when boiled.

In order to try whether the lime, which adhered to, or had soaked into, the flesh of the fish, which had lain in lime-water, had the quality of thus dissolving the texture of the flesh in boiling, Dr. H. boiled a small eel, and a morsel of mutton, for 10 minutes, in stone lime-water, when they were boiled enough, and were of a due degree of firmness, and not pappy. A like eel, boiled in well-water, was boiled enough in 5 minutes. Hence it appears, that the lime does not, in boiling so short a time, dissolve the texture of the flesh into a pap, which must therefore be the effect of unfetid putrefaction.

But lime-water made of chalk lime has very little of an antiseptic quality. For last year, in the month of May, he put some gudgeons, and an eel, into our common lime-water, and in 7 days boiled one of the gudgeons, but found it too putrid to eat. After 28 days he boiled another, and it dissolved almost into insensible parts; which shows, that it was much putrefied.

Dr. Alston likewise informed him, that he put a piece of veal in pounded or slacked stone-lime, which in a week became tough and dry. Dr. H. put a piece of veal, from half to three-quarters of an inch thick, into chalk-lime, on May the 10th, and on the 31st of the same month it had a putrid smell, and was in the middle red and raw, with a thin hard outside.

Having communicated these experiments to Dr. Pringle (whose trials having been made with chalk lime-water, which is in common use here, agreed with the last of mine), he observed, that the difference between stone lime-water and chalk lime-water, might probably consist in this: the chalk, before calcination, being a highly septic substance,* if some of its particles were not fully calcined, these, by mixing with the water, would impart to it some degree of a putrefying quality, contrary to that virtue the water receives from such parts as are sufficiently burnt. That the same would be the case of shells, also septics; and therefore that the lime-water, made either of chalk or shells, would prove more or less antiseptic, or even continue septic, according to the degree of calcination. He added, that as all his experiments, relating to the antiseptic quality of lime-water, were made in a furnace heated to the degree of human blood, a circumstance which he had marked in his Observations,† the uncalcined parts of the

* Observ. on the Diseases of the Army, 1st ed. p. 390.—Orig.

† To one of the experiments preceding that on the lime-water, the author subjoins the following note: "All the following experiments, whether made in the lamp furnace, or by the fire, were in a degree of heat equal to that of the human blood, viz. 100° of Fahrenheit's scale." p. 383.—Orig.

lime would in that state become more active in promoting putrefaction, than when the trials were made in cold water.

And indeed it must be owned, that when any experiments are made on medicinal substances out of the body, the nearer we can make them to the heat of the blood, and to other circumstances those substances must undergo in the first passages, the more just the inferences will be, that are drawn from those experiments.

In regard to that quality of lime-water, in preserving fish longer sweet than flesh, Dr. Pringle took notice, that he doubted it was a common mistake to account fish a more corruptible substance than the flesh of land animals. For though fish might become sooner stale for eating than most flesh meats, yet that fish did not so soon rise to a rank degree of putrefaction as flesh; and therefore that the former would be kept longer tolerably sweet than the latter by any kind of antiseptic.

CV. Medical and Chemical Observations on Antimony. By John Huxham, M. D., F. R. S. p. 832.

In the present advanced state of pharmaceutical chemistry, it is deemed unnecessary to reprint this long paper on the different preparations of antimony. Dr. H. particularly recommends his so called essence of antimony, or vinum antimoniale, prepared by infusing either the glass of antimony or regulus of antimony in white wine. This he preferred to every other antimonial medicine.

CVI. Of Mr. Samuel Tull's Method of Castrating Fish. Communicated by W. Watson, F.R.S. p. 870.

Several years since, Mr. Tull of Edmonton performed the operation of castrating fishes, before Sir Hans Sloane, Bart. and several members of the Royal Society, who met at Sir Hans house for that purpose. About 5 or 6 years ago he performed the same operation in presence of our late president Mr. Folkes, and others.

In England, where in many parts sea-fish are in great plenty, the fish of rivers and ponds are less esteemed; and improvements, either with regard to their bulk or increase, are less attended to; but in Germany, remote from the sea, where pond-fish are a great article of traffic, Mr. Tull's method may be of great use.

Mr. Tull says that he castrates both the male and female fish; and that, although almost any time is proper for the operation, the least so is just after they have spawned, as the fish then are too weak and languid to bear, with success, so severe an operation. The most eligible time however is when the ovaries of the female have their ova in them, and when the vessels of the male, analogous to these, have their seminal matter in them; inasmuch as at this time these vessels are more easily distinguished from the ureters, which convey the urine from the kidneys into the bladder, and are situated near the seminal vessels on each side of the spine. These may, without sufficient attention, be taken for the ovaries; and the more so, when these last are empty. When fishes have spawned a few weeks, they are fit for the operation; for, like hens, they have small eggs in their ovaries as soon as they have laid their former clutch of eggs.

When a fish is intended to be castrated, it must be held in a wet cloth, with its belly upwards; then with a sharp pen-knife, with its point bent backwards, or other well-adapted instrument, the operator cuts through the integuments of the rim of the belly, and in doing this he carefully avoids wounding any of the intestines. As soon as a small aperture is made, he carefully inserts a hooked pen-knife, and with this he dilates this aperture from between the two fore-fins, almost to the anus. From the back of this instrument, being blunt, the danger of wounding the intestines is avoided. He then, with two small blunt silver hooks, of 5 or 6 inches long, by the help of an assistant, holds open the belly of the fish; and, with a spoon or spatula, removes carefully the intestines from one side. When these are removed, you see the ureter, a small vessel, nearly in the direction of the spine; and at the same time the ovary, a larger vessel, lying before it, that is, nearer the integuments of the belly. This last vessel you take up with a hook of the same kind with those before mentioned, and detaching it from the side far enough for the purpose, divide * it transversely with a pair of sharp scissars; remembering always, that great care is taken in not wounding, or otherwise injuring, the intestines. After one of the ovaries has been divided, proceed in the like manner to divide the other; and then sew up the divided integuments of the belly with silk, inserting the stitches at a very small distance one from the other.

Mr. Tull first put this method into practice, in order to prevent the excessive increase of fish in some of his ponds, where the numbers did not permit any of them to grow to an advantageous size. But from castration the increase was not only prevented, but the castrated fish, as Mr. Tull asserts, grew much larger than their usual size, were more fat, and, which is no trifling consideration, were always in season.

He observes further, that the spawning time is very various: that trouts, for instance, are full about Christmas; perch in February; pikes in March; and carp and tench in May. You must always, however, make some allowance for climate and situation, with regard to the spawning of fish. And, from a very

* Mr. Tull has frequently, to prevent the re-union of the divided ovaries, by which the effect of the operation might be defeated, taken out part of them, and yet the fish have survived.—Orig.

diligent attention, he assures that he has been able to settle a point much controverted by naturalists, in regard to the copulation of fishes. The most generally received opinion has been, that they did not copulate; but that the female did cast her spawn in the water, and that then it was fecundated by the spermatic matter of the male. Mr. Tull, in contradiction to this hypothesis, asserts, that he has frequently seen fishes in actual copulation; and that this is generally done before the ova arrive at maturity.

After Mr. Tull has castrated his fish, they are put into the water where they are intended to continue. He makes no particular appropriation, neither with regard to the ponds into which they are put, nor does he give them any particular nourishment; but they take their chance in common with other fish, as though they were not castrated. And he remarks further, that if tolerable care is taken, very few fish die of the operation, when performed in the manner here described; though heretofore, when, instead of the belly, he made the opening in the sides of the fish, numbers died, from his wounding the intestines, and frequently dividing the ureters.

CVII. An Attempt to point out, in a concise Manner, the Advantages which will accrue from a Periodic Review of the Variation of the Magnetic Needle, throughout the known World; addressed to the Royal Society, by Wm. Mountaine and James Dodson, Fellows, and requesting their Contribution thereto, by Communicating such Observations concerning it, as they have lately made, or can procure from their Correspondents in Foreign Parts. p. 875.

About the year 1700, Dr. Halley having collected together many observations on the variation of the needle, in several parts of the world, drew (on a mercator chart) certain lines, showing the quantity of that variation, in those parts of the world, over the representation of which those lines were drawn; but as the quantity of this variation is in a perpetual state of fluctuation, in perhaps every part of the world, it had been so much changed in the space of about 40 years, that when the writers of this paper endeavoured, about the year 1744, to draw on it other lines to answer the purposes above mentioned, they found that those laid down by Dr. Halley were grown entirely useless; and that a system of such lines, or something analogous, should be performed once in every 10 or 12 years at least, in order to answer the purposes intended by that gentleman.

In the reconstruction of them, the writers received the assistance of the commissioners of the navy, and of the directors of the East-India and African companies, having leave to peruse the journals of those mariners which were under the direction of each respective body; from these, and a few private communications, they were enabled to draw the proper lines over the most frequented seas. and to make some attempts toward doing the same in those least so; a copy of the chart, thus again rendered useful, they presented to the R. S. with an account of the methods used in performing the same.

Though the most beneficial use of these lines belongs to the sea, yet if they could be extended over the land likewise, the advantages arising would more than compensate the trouble, as will appear by taking a short view of each.

And first, the use of these lines at sea may be considered either as common to the art of navigating in all large bodies of water; or as particular in some such: the general use being that of steering the true course designed, and finding the ship's true place, as near as may be, by what the mariners call the dead reckoning.

The particular uses will be best explained by examples: for instance, in the southern parts of the great Atlantic ocean, beginning with the coast of Brazil and Patagonia, and proceeding to the south of the Cape of Good Hope into the Indian ocean, as far as the common tracks of our East-India ships extend, the variation lines have appeared to be, for the most part, directed northward and southward: whence, in most places of that great body of waters, if the latitude and variation be found by celestial observations, the longitude will be obtained by the lines on the chart; the great usefulness of which has been attested to the writers, by many persons who have, successfully to themselves, practically applied the last constructed chart, to correct their dead-reckoning on that long passage.

Indeed, where the variation lines run nearly eastward and westward, as has appeared in the Atlantic ocean, from the west coast of Europe to the east coast of North America, no assistance toward obtaining the longitude can be derived from them; but as it frequently happens, within those limits, that meridian observations, for determining the latitude, cannot be obtained, especially about Newfoundland; then, if a good observation of the variation can be taken, at any time of the day, the latitude may be nearly ascertained by the lines on the chart.

Secondly, the advantages that will arise by extending the variation lines over the land, as well as sea, will be the confirmation of those drawn over the waters; the continuation of which, from sea to sea, will be thus conspicuous, and we hall be enabled to judge better of their nature, properties and causes; and if the same can be extended over all the parts of the known world, the eye will be presented, at one view, with the different degrees of attraction, with which all the parts of this great magnet are endued, at the time when such lines are drawn; this the writers would have attempted to do, in the year 1744, if they could have procured a sufficient number of observations for that purpose; but though they frequently advertised their request, in the public papers, no assistance was obtained.

As the writers have by experience found, that the proper period for re-examining the state of the variation, is now at hand, without which the above-mentioned valuable advantages of the chart will be lost to the mariner; they have determined to collect and compare all the observations that can be procured by them, in the space of a year from this time, or so long after as the return of the East-India ships then next following; if such delay should become necessary, by the arising of any doubt in consequence of such comparison; and then to publish the result of their process, in such a manner as shall seem most convenient.

Several of the learned and ingenious have endeavoured to account for this phenomenon of the variation of the magnetic needle, and its continual mutation; whence different methods of computation have been proposed, by which they have endeavoured to determine what the quantity of the variation, according to their several hypotheses, will be at any given place and time: the above proposition therefore will, if carried into execution, bring these severally to the test, and enable the judicious either to approve or reject them; the writers being determined to publish nothing which shall not be warranted by the real observations, which shall come into their hands, and shall leave the application to others: if any of them should be so far confirmed, by this examination and comparison, as to give just ground for a calculation, their labour will be at an end; but if not, they humbly recommend the continuance of such a periodic operation, as they now propose to undertake, being the only means of attaining such a desirable event, and of supplying the defect till it can be obtained.

END OF THE FORTY-EIGHTH VOLUME OF THE ORIGINAL.

Art. I. On the Pressure of Weights on Moving Machines. By Christian Hée, Professor of Mathematics and Experimental Philosophy in the Marine Institution of Copenhagen. From the Latin. Vol. XLIX. p. 1.

Let fig. 9, pl. 11, represent an axe-in-peritrochio. Let A be the moving power; its distance from the centre of motion a; also B the weight, b its distance from the centre; and c the radius of the axis where the friction is. Further, let M denote the weight of the machine, and d the distance of the centre of the forces from the centre of gravity. It is required to find the pressure on the axis, when the descending power A actuates the machine.

If now the pressure arising from the descending power A, or that by which the thread is stretched at the side α, be called π; then, by the equality of action and re-action, the pressure or tension at the other side β will be $= \frac{\pi a}{b}$; hence the whole pressure, exclusive of the weight of the machine and cord, will be $= \pi +$

$\frac{\pi a}{b} = (1 + \frac{a}{b})\,\pi$. Let now the constant ratio of the pressure to the friction be that of 1 to μ; then the friction will be $(1 + \frac{a}{b})\,\pi\mu$; and the momentum of this friction $= (1 + \frac{a}{b})\,c\pi\mu$: but the momentum of the friction from the weight of the machine is $= \text{M}c\mu$; which added to the former momentum, gives $((1 + \frac{a}{b})\,\pi + \text{M})\,c\mu$: hence the momentum of the moving power is $= \text{A}a - \text{B}b - ((1 + \frac{a}{b})\,\pi + \text{M})\,c\mu$. But since the momentum of inertia is $\text{A}a^2 + \text{B}b^2 + \text{M}d^2$; therefore the accelerating force will be $= \frac{\text{A}a - \text{B}b - ((1 + \frac{a}{b})\,\pi + \text{M})\,c\mu}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2}$; and for the acceleration of the point α, or of the moving power A, it will be, by the principles of mechanics, $\frac{\text{A}a^2 - \text{B}ab - ((1 + \frac{a}{b})\,\pi + \text{M})\,ac\mu}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2} \times \dot{t} = \dot{c}$; where $\dot{t}$ denotes the element of the time, and $\dot{c}$ the element of the velocity. But if A should fall freely, it would be $\frac{\text{A}}{\text{A}}\,\dot{t} = \ddot{t}$. And since the increments or decrements of velocities, generated in the same particles of time in the same body, are as the generating forces, therefore $\ddot{t} : \ddot{t} - \dot{c} :: \text{A}$: the force generating the decrement of celerity $\ddot{t} - \dot{c}$, which is the same force that retards the fall of the body, stretches the string, and presses on the side α; hence, substituting for the values, we have the following analogy $1 : 1 - \frac{\text{A}a^2 - \text{B}ab - ((1 + \frac{a}{b})\,\pi + \text{M})\,ac\mu}{\text{A}^2 + \text{B}b^2 + \text{M}d^2} :: \text{A} : \pi$; therefore $\pi = \frac{\text{AB}b^2 + \text{AM}d^2 + \text{AB}ab + ((1 + \frac{a}{b})\,\pi + \text{M})\,\text{A}ac\mu}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2}$; from which equation is found

$$\pi = \frac{\text{AB}b^2 + \text{AM}d^2 + \text{AB}ab + \text{AM}ac\mu}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2 - (1 + \frac{a}{b})\text{A}ac\mu}, \text{ and}$$

$$\frac{\pi a}{b} = \frac{\text{AB}ab + \text{AM}d^2\frac{a}{b} + \text{AB}a^2 + \text{AM}\frac{a^2c\mu}{b}}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2 - (1 + \frac{a}{b})\,\text{A}ac\mu}, \text{ and the whole pressure}$$

$$\pi + \frac{\pi a}{b} = \frac{\text{AB}\,(a + b)^2 + \text{AM}\,(d^2 + ac\mu)\,.\,(1 + \frac{a}{b})}{\text{A}a^2 + \text{B}b^2 + \text{M}d^2 - (1 + \frac{a}{b})\,\text{A}ac\mu}.$$

If now we should exclude the friction and the weight of the machine, we should have the whole pressure $= \frac{\text{AB}(a + b)^2}{\text{A}a^2 + \text{B}b^2}$. And if, as is the case in the pulley, we suppose $a = b$, the whole pressure will be $= \frac{\text{AB}\,(a + b)^2}{(\text{A} + \text{B})a^2} = \frac{4\text{AB}}{\text{A} + \text{B}}$.

II. Investigation of a General Rule for the Resolution of Isoperimetrical Problems of all Orders. By Mr. Thomas Simpson, F.R.S. p. 4.

The different species of problems comprehended under the name of Isoperimetrical ones, are of much greater extent than the name imports; since, not only the determination of the greatest areas and solids, under equal perimeters or bounds, whence the name is derived, but whatever relates to the maxima and minima of quantities depending on a line, space, or body, of which the figure is unknown, is by mathematicians included under that denomination. But notwithstanding the usefulness and great extent of this subject, nothing had been done in it further than the resolution of certain particular cases (such as finding the line of the swiftest descent, and the solid of the least resistance), till the celebrated mathematician Mac Laurin, in his treatise of fluxions, gave the investigation of an elegant and very easy method, by which the principal problems belonging to the first order may be resolved.

The paper Mr. S. now lays before the Society contains further improvements on this subject; as it is by far more general than any thing yet offered, and is drawn up with a view to obviate the difficulties attending the resolution of a very intricate kind of problems, and thus to open an easy way to some very interesting inquiries in natural philosophy. But instead of reprinting the calculations, it will be better to refer to p. 98, &c. of the author's Miscellaneous Tracts in 4to, published 1757, where the same paper is given in a more extended and improved state.

III. On the Effects of Lightning at Plymouth. By John Huxham, M.D., F. R. S. p. 16.

Sunday, December 15, 1754, 25 minutes after one P.M. a vast body of lightning fell on the great hulk at Plymouth-dock, which serves to hoist in and fix the masts of the men of war. It burst out about a mile or two to the westward of the hulk, and rushed with incredible velocity towards it. The piece of the Derrick cut out was at least 18 inches diameter, and about 15 or 16 feet long: this particular piece was in 3 or 4 places begirt with iron hoops about 2 inches broad, and half an inch thick, which were completely cut in two by the lightning, as if done by the nicest hand and instrument, The lightning was immediately succeeded by a dreadful peal of thunder, and that by the most violent snower of hail, which fell only in and about this town, for a mile or two: there was very little of it at the dock, though only 2 miles distant. The hail-stones were as large as small nutmegs, all very nearly of the same size and shape. They measured, immediately after they fell, near 2 inches round.

IV. A Remarkable Case of a Morbid Eye. By Mr. Edward Spry, Surgeon, at Plymouth, in Devonshire. p. 18.

The wife of Thos. Smaldridge, a mariner of Plymouth, complained to Mr. S. of a violent pain in her left eye, and sometimes of very acute pains in the temple of the same side, with some defect in her sight. She also imagined that her eye was larger than ordinary; but on inspection it did not appear so. The cornea however became less transparent, and the pupil greatly dilated: but though the pain of her eye was so great, yet the blood-vessels of the conjunctiva were no way enlarged, nor in the least redder than that and the sclerotica were before; and, from its whiteness, it appeared no more morbid than the other.

Having resorted to bleeding, blistering, mercurial purges, &c. without any good effect, Dr. Martyne of Plymouth was consulted, but with no better success, her pain increasing, rather than diminishing. And as the pupil became enlarged, and the cornea more opaque, with great inflammation of the conjunctiva and sclerotica, and an apparent prominence of the whole eye, when every thing hitherto failed, Mr. S. tried 2 or 3 drastic purges; but these disagreeing very much, he was forced to return to his former method. He then cut a seton in her neck, which run very much; but all to no purpose, and she became still more miserable. The conjunctiva became greatly inflamed, with an eversion of the upper lid, attended with great pain. He often made incisions with his lancet on this coat, which bled plentifully, and gave her ease for a day or two, and even took 8 oz. of blood from the temporal artery. But the eye being greatly enlarged, and of so terrible an appearance, after all endeavours for 8 or 10 months, he judged her disease to be a carcinoma, and therefore proposed cutting out the whole eye as the only remedy.

Several skilful surgeons were consulted, and it was agreed to defer the operation, and trust to nature; though she was in that miserable condition; but at length her eye becoming much greater, and her pain being increased, he resolved on the operation, lest the bones of the orbit might become carious.

Thus, having called in Dr. Huxham, with some of the most skilful surgeons of Plymouth, he performed the operation in the following manner; viz. the tumor was so very large, and the upper lid so distended, that he was obliged first to divide the orbicular muscle at the inner canthus; and there began the incision round the upper part of the tumor, for the more convenient use of the knife. He had not cut deep when a great quantity of pus, like lymph, flowed out with great force, like a fountain, and the tumor subsided a good deal: but pursuing the operation, he found a large cist, which filled the whole orbit behind the eye; and so part of the cist was left to slough off with the dressings. The whole eye being cut out, he filled the wound with lint, &c. and in 3 days removed the dressings, with a great quantity of sanies, which were daily renewed, and the part of the cist, which was left behind, sloughed off the 2d day. The cure went on with success, and, in a month from the operation, was completed; and she remained free from pain from that time to the above date.

Having examined the diseased eye after its excision, they found the humours very much confused: the aqueous humour was not so clear as usual, the crystalline less solid and transparent, and the vitreous almost reduced to a liquid state. The cist was very strong and elastic, and had a cavity large enough to contain a hen's egg.

V. A Supplement to the Account of a Distempered Skin, published in the 424th N° of the Phil. Trans. By Mr. Henry Baker, F.R.S.* p. 21.

In 1731, a lad, 14 years of age, was brought by his father from Euston-Hall, in Suffolk, and shown to the R.S. on account of his having a cuticular disorder, of a different kind from any noticed in the histories of diseases, as mentioned in the aforesaid N° of the Phil. Trans.

More than 24 years from the date of that account, he was living, and shown at London by the name of the porcupine-man. His name was Edward Lambert. He was then 40 years of age, a good-looking, well-shaped man, of a florid countenance; and when his body and hands were covered, seemed nothing different from other people. But except his head and face, the palms of his hands, and bottoms of his feet, his skin was all over covered in the same manner as in 1731. This covering seemed to Mr. B. most nearly to resemble an innumerable company of warts, of a dark-brown colour, and a cylindric figure, rising to a like height, and growing as close as possible to each other; but so stiff and elastic, then when the hand was drawn over them, they made a rustling noise.

When he saw this man, in September 1755, they were shedding off in several places, and young ones, of a paler brown, succeeding in their stead, which, he said, happened annually in some of the autumn or winter months: and then he commonly was let blood, to prevent some little sickness, which he else was subject to while they were falling off. At other times he was incommoded by them no otherwise, than by the fretting out his linen, which, he said, they did very quickly: and when they came to their full growth, being then in many places near an inch in height, the pressure of his clothes was troublesome.

He had had the small-pox, and been twice salivated, in hopes of getting rid of this disagreeable covering: during which disorders the warting came off, and his

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

skin appeared white and smooth, like that of other people; but on his recovery, it soon became as it was before. His health at other times had been very good during his whole life.

But the most extraordinary circumstance of this man's story was, that he had had 6 children, all with the same rugged covering as himself: the first appearance in them, as well as in him, came on in about 9 weeks after the birth. Only one of them was then living, a very pretty boy, 8 years of age, whom Mr. B. saw, and examined, with his father, and who was exactly in the same condition, which he thought needless to repeat. He also had had the small-pox, and during that time was free from this disorder.

VI. On the late Eruption of Mount Vesuvius. By Isaac Jamineau, Esq. his Majesty's Consul at Naples. p. 24.

In April 1754, the fire issued from one end of a hillock, in the shape of a crescent, within the crater. On his 2d visit, in September, the crescent was turned to a cone, but much higher than before, being increased in proportion to the fire, that now discharged, by frequent explosions, thousands of stones on fire. On a 3d visit, in the middle of October, the cone seemed lower, which was owing to the rising of the bottom of the cup, whose depth from 80 feet was decreased to 50. The lava was actually running in many places; and where it was not, the fire was universally visible within a foot or two of the surface.

The running of the lava within the crater increased daily, so that in a month's time the cup was filled within 25 feet. On Tuesday, December 3, at night, after a little shaking, which was not felt above 2 or 3 miles off, an opening burst on the eastern side of the mountain.

Notwithstanding its slowness, it drives the strongest stone fences before it, and from lighting the trees, like torches, affords a most extraordinary, though dismal and pitiful spectacle.

But the lesser stream which I saw before, is a small trout-stream compared to this, which sets off in a cascade of a mile's length, and, though rather with a less declivity, is equally rapid, from the greater quantity of matter rushing down it. The breadth about 60 feet at the top; but by having melted down an island, that divided its stream about 200 yards in the fall, the breadth in that place must be above 100 yards.

VII. An Account of the Species of Plant, from which the Agaric, used as a Styptic, is prepared. By Mr. Wm. Watson, F. R. S. p. 28.

Mr. W. having written to M. Clairaut of Paris, requesting him to put some questions to Messrs. de Jussieu and Morand, concerning the species of agaric used as a styptic, he received for answer that it is the

"Agaricus pedis equini figurâ. Inst. Rei Herbar.
"Fungus in caudicibus nascens, pedis equini figurâ. C. B. Pin.
"Fungus durus arborum, sive igniarius. Park. Theat.
"Fungi arborei ad ellychnia. I. B.
"Fungi igniarii, Cæsalpini et Tragi.
"Boletus acaulis pulvinatus lævis, poris tenuissimis. Linn. Flor. Suec.

It is the agaric employed for the amadoue; and Mr. Brossart, who first brought this preparation into practice, conceives that that which grows on old oaks, which have been lopped, is the most valuable; that it should be gathered in August or September, and be kept in a dry room.

The way of preparing it, is to take off with a knife the white and hard part, till you find a substance so soft, as to yield under the finger, like shammoy leather. This is to be divided into pieces of different sizes and thickness: beat these with a hammer, to give them a still greater degree of softness, so that they may be easily torn with the finger.

Mr. Morand thinks, that the agaric, which when growing is of a greyish colour on the outside, is better than that which is white.*

VIII. Of a Mountain of Iron Ore, at Taberg in Sweden. By Peter Ascanius, M. D. Translated from the Latin by Mr. Emanuel Mendes de Costa, F. R. S. p. 30.

The mines of Sweden are justly esteemed superior to the mines of most other countries; and those of iron are the most famed. Among the most curious of the latter, is that of Taberg, if, with propriety, it can be called a mine. The Swedish iron is, and has always been, carried to most parts of Europe, and is preferred to all other iron.

This mountain is situated in a sandy tract of land, of which the sand is extremely fine. The whole mountain is one mass of rich iron ore, and even in some parts is mixed with particles of native iron. About 200 years ago (for so long have they worked on this mountain) they blew up the masses of ore; yet the mountain appears very little diminished, except in the laves or hollow places, which are at the foot of the mountain, opposite to the valley. In the interior fissures of the mountain, bones of animals, as of stags and other kinds, are frequently found imbedded in the sand. No ore is found beyond the foot of the mountain, nor on the neighbouring plain; so that it appears, as if the mountain had been artificially laid on the sand, for it has no roots, or, like other mountains, its substance does not penetrate the ground. The ore breaks easily, and what is broken from the sides of the mountain readily falls to the foot of it; while

* The plant here mentioned is the boletus igniarius, Linn.

in other mines the ore, with great trouble and cost, is dug from the bowels of the earth. The only inconveniency which happens here is, that the sand, which is digged in very great quantities in the fissures, when the ore is blown up, falls with it to the foot of the mountain, and buries or covers it, which they are forced to dig away again; on which account they always blow up the ore from the bottom of the mountain upwards, for the greater ease of the miners, and to hinder the heaping of the sand at the bottom. They then carry the ore to the neighbouring furnaces, where being roasted, and broken small, they mix it with lime-stone and powdered coal, and smelt it into iron.

IX. An Extraordinary Case of a Child. By Mr. Richard Guy, Surgeon. p. 34.

A child near 7 years of age, having languished, for near 12 months past, of a supposed dropsy, and undergone the most skilful treatment of several eminent physicians unsuccessfully, died in an emaciated state. By desire of the parent, Mr. G. opened the body, expecting to find water, but to his great surprize, there appeared as follows: a large round solid substance, shaped in the form of an egg, weighing 14 lb. $2\frac{1}{4}$ oz. of the adipose cellular consistence; some parts of it being more brawny than others. On dividing it through the centre, were found several little cists, containing a meliceratous fluid; the whole seemed enveloped in a membrane, which he apprehended to be the omentum, but the extension, from so large a body contained in it, had made it almost lose its reticular appearance. It was surrounded with many small blood-vessels, but no considerable ones. It adhered to the peritoneum, the back-bone, and almost all the internal cavity of the abdomen, resting the large end in the pelvis, and greatly compressing the bladder and ureters. The intestines were all crouded together on the right side, in as small a compass as could possibly contain them. The intestine colon passed round the lower part, in the form of an *S*, which adhered likewise: it also inveloped the right kidney, which appeared something larger than the other; and on dividing it, he found small stones, not exceeding the size of a large pin's head. The other kidney did not adhere to this substance. The small end pressed upwards against the diaphragm, so hard, as to force the heart close under the left clavicula: the lungs were so confined, as to render only one lobe capable of respiration; the others appeared as in a still-born child. The liver, gall-bladder, and spleen, were as in health; the intestines the same; the mesentery was much extended with blood; the matrix and ovaria as in their natural state; and no other parts, that he could discover, affected. He could not discover, on dissection, any nuclei, that might particularly supply, or give rise to, this enormous substance.

X. Extracts of two Letters from Mr. James Latterman, Student in Physic and Surgery, to Dr. Schlosser, concerning the Effects of the Agaric of the Oak. To which are added some remarkable Experiments made on the Arteries of Horses, with the Powder of the Lycoperdon, or Lupi Crepitus. By M. La Fosse, Farrier to the King of France. Communicated by Mr. Joseph Warner, F. R. S. Surgeon of Guy's Hospital. p. 36.

[In these letters some additional cases are given of the successful application of agaric as a styptic after surgical operations. Also, of the application of the lycoperdon, with similar success. After the several cases of this kind, related in the preceding numbers of these Transactions, it is deemed superfluous to reprint the detail of these additional experiments, especially as the surgical practitioners of the present day are agreed, that for stopping hæmorrhages from the larger arteries, the ligature alone is to be relied on.]

Respecting the manner in which these vegetable styptics act on the mouths of the bleeding vessels, Mr. La Fosse observes First, that when applied to the mouths of the divided arteries, the bleeding has ceased in a few minutes, and that the mouths of the divided arteries have healed up without any further discharge. Secondly, that in 24 hours after the application of this powder, a thin pellicle or skin is formed on the mouths of the divided arteries, and that within the vessels is found a small plug of congealed blood. Thirdly, that the pulsation of the artery is to be seen in a very distinct manner at the extremities of the vessels. Fourthly, that the coagulated blood is of a conical figure, whose basis is at the mouth of the vessel, and its apex in an opposite direction.

XI. On the Use of Lycoperdon, in Stopping Blood after Amputations. By James Parsons, M. D., F. R. S.* p. 38.

XII. The State of the Thermometer, Feb. 8 and 9, 1755. By Henry Miles, D. D., F. R. S. p. 43.

The cold on the 8th, especially at midnight, was extraordinary, if it be considered in how short a space of time it increased to that degree. The lowest state of the thermometer, was on the 8th day, at 7^h a. m. when it had fallen to $17\frac{1}{2}$.

XIII. Some Cases of Dropsies, cured by Sweet Oil. By Wm. Oliver, M. D., F. R. S. p. 46.

Mr. Pierce took from Miss —— 11 pints of water. As soon as the bandage could be loosened, Dr. Hartley and Dr. O. examined the state of her belly. The

* Not reprinted for the reason assigned in the remark affixed to the preceding paper.

epigastric region was quite emptied; but they found a great fullness, which extended itself on each side the inguen, towards the back. They put her on a very spare dry diet, and allowed her but a quarter of a pint of liquids in the 24 hours. But though her urine much exceeded in quantity what she drank, the swelling increased, and they feared the belly would soon fill again. A lady, who was with her, said that, just before she left London, she had heard that two persons had been cured of confirmed dropsies by being anointed, morning and evening, with common sallad oil, which was rubbed into the whole abdomen, for an hour at a time, with a warm hand. They could not refuse the trial of so innocent a method. The iâtraleiptæ began their operation. About the 3d day of anointing, the urine was considerably increased, and continued to be so. The fullness gradually decreased, and in a fortnight's time was quite gone. Her appetite, digestion, and sleep, grew natural, and she recovered flesh, strength, and spirits. About 6 weeks after her first anointing, her menses appeared, and at the end of the next month she had a regular return of good colour, and in sufficient quantity. Dr. O. saw her at the public room a week before, in as good health as she ever enjoyed.

This recovery was much talked of, and set all the hydropics on rubbing. A man, aged 55, from hard drinking, and many wrong methods of cure, had been cachectic 15 years, and had often the symptoms of jaundice and dropsy. Half a year before, his belly, legs, and thighs, swelled to an enormous size. He was with difficulty moved from his bed to his chair, and was given over, as a person in an incurable dropsy. About 3 weeks before, he began to anoint. After 3 or 4 days rubbing, his urine was greatly increased; and in a fortnight, his belly, thighs, and legs, were wonderfully decreased; and Dr. O. saw him afterwards walking about the town, though before he could not move a joint.

A woman 70 years of age, of a thin habit, who got a livelihood by carrying cakes about the town, fell into an ascites. Her belly was so greatly distended, that she was obliged to quit her business, to confine herself to her house, and for the most part to her bed. She anointed. Her urine soon increased in quantity, and continued to do so.

XIV. Observations of the Eclipses of Jupiter's Satellites at Lisbon. By John Chevalier, F. R. S. p. 48.

Jan. 11, 1754, he observed the immersion of the 2d satellite at $9^h\ 4^m\ 3^s$.—Jan. 15, the 1st satellite immerged at $11^h\ 23^m\ 58^s$.—Jan. 18, the 2d satellite immerged at $11^h\ 35^m\ 30^s$.

XV. Of those Malignant Fevers, that raged at Rouen, at the End of the Year 1753, *and the Beginning of* 1754. *By Mons. Le Cat, M. D.* p. 49.

About the end of Nov. 1753, a malignant distemper broke out in Rouen;

the ravages of which continued during the subsequent months of Dec. Jan. and part of Feb. But before he enters on the history of this epidemic, he gives an account of the diseases which prevailed during some of the preceding years.

The medical gentlemen, who had practised in Rouen from the beginning of the 18th century, state, that, for the last 30 years, that country had been more subject to malignant fevers than it had ever been before; and that the greatest part of them had been accompanied with miliary eruptions. M. le Cat fixes this epocha in 1723 and 1724, because the first of these years was excessively dry, the rain at Paris amounting to no more than 7 inches 8 lines, while the mean year comes to 19, and the year 1724 had only 12; while the year 1725 produced more than 17½ inches, which should cause a temperature nearly approaching to the mean quantity, which may be considered as the most healthy.

He observed in 1736 and 1737 certain gangrenous sore throats, which chiefly attacked children; they appeared again in 1748, in young persons of the first distinction, not only at Rouen, but also at St. Cyr, near Versailles, and at Paris. Persons of a certain age were also seized with it, not only in town, but in the country; and in some the tongue alone was the seat of the gangrenous eschar. In the same years 1737 and 1738, there was a great number of malignant peripneumonies, of that kind called pituitous. The lungs of these subjects, many of which he opened, were become schirrous; and the patients perished for want of being able to admit air into them, as if they had been strangled. Some of them most earnestly begged him to open their breasts, imagining that a new vent would give them breath.

In 1739 they had, at the Hôtel Dieu, continual fevers, with frequent faintings: and the patients, without any violent symptom, died in 6 or 7 days. He found small abscesses in the substance of their hearts, near the auricles. Nothing remarkable happened from 1739 to 1743, but that the finest, longest, and driest summer he ever knew in Normandy, produced epidemical bloody-fluxes, which grievously afflicted both Rouen and the whole country round about. These fluxes were preceded by great lowness of spirits, attended with violent colics, and a sharp fever: the pulse small, the mouth and tongue foul, a nasty taste in the mouth, and frequent nausea; and whenever a hiccup came on, death was not far off.

The principal seat of this distemper was in the large intestines; though sometimes the small guts and stomach had their share. In one, who voided pure blood a little before his death, he found a great portion of the intestinal canal full of blood, the villous coat being much swelled, and greatly inflamed; and, putting it in water, one might easily discern, with a magnifying glass, a great number of red points, which appeared to be the mouths of the vessels, which poured out the blood found in the intestines. Another had blood discharged even up to the stomach; and the inner membrane of this organ, towards the pylorus, was in the same condition with that of the great intestines of the foregoing patient. The duodenum, jejunum, and the beginning of the ileum, were sound; the end of the ileum was inflamed, and the large intestines were gangrened. In another, the same intestines were all mortified; the cæcum, and half the colon, were as large as a stomach distended with wind. Their canals were full of a bloody matter, and their inner membrane separated very easily. The gangrene seemed particularly to affect this coat. The stomach and small guts were sound; yet his death was preceded by the hiccough. In some others, the gangrene had seized all the coats of the intestines; and sometimes these canals were so far pierced by the eschars, as to let the fæces pass through into the cavity of the belly. And in some the bladder itself partook of the disorders observed in the great intestines.

A few bleedings at first, cooling liquors, as whey, chicken-water made into an emulsion, emollient clysters often repeated, and paregorics given properly, and in small quantities, were the most sovereign remedies for this disease. Purgatives were generally hurtful. Ipecacuanha succeeded with some; and an English pupil, Mr. George Ross, made very successful trials with boluses of vitrum antimonii ceratum. Whenever blood was taken away in an over great quantity, the patient in 3 or 4 days fell into the agonies of death. Anodyne drops given too freely, instead of quieting, occasioned restlessness, and increased the fever and inflammation.

M. le Cat was himself struck with this disease, as if with lightning, and passed, in a few hours, from a good state of health into a sinking and insensibility, which indicated a gangrene coming on, and the utmost danger. Two bleedings, close on each other, brought him to himself; but his insensibility was succeeded by the usual colic and flux, which was the principal distemper: then $1\frac{1}{2}$ oz. of diacodium freed him from this painful and dangerous condition, as speedily as the infected air had thrown it on him.

In the following season, and even in the year 1744, when this distemper prevailed no longer epidemically, there happened some very extraordinary circumstances. A woman, the 30th of November 1743, being of a robust habit of body, and in perfect health, was suddenly seized with a violent colic in her stomach, and died in 3 hours, He found 3 gangrenous places at the upper orifice of the stomach. He doubted whether ever any distemper could have deserved the name of a plague more than this, if it had been epidemical.

In the course of the year 1744, they had a great number of gouty rheumatisms, with fevers. The patients were deprived of the use of their limbs; the miliary eruption often came on, and seemed to relieve them by restoring their limbs. In some, their pains went off by forming phlegmons and erysipelases on the extremities; some of which seized the arm and fore-arm, and were considerable enough to bring on the death of the patients; others were attended with large gangrenous eschars, which also frequently proved fatal. Of all the remedies, that did service in these disorders, decoctions of the bark, and the sudorific woods, as also that of scorsonera, were most effectual. But if a plentiful miliary eruption came on, notwithstanding the relief it seemed at first to procure, the event seldom turned out well.

The years 1745, 46, and 47, proved tolerably healthy; some disorders of the throat, becoming more common about the end of the last of these 3 years, were the fore-runners of the gangrenous sore throats of 1748. In these cruel distempers, the throat was in the same state with that of the larger intestines in 1743. Great and frequent bleedings made the patients go off the sooner.

There were also this year malignant fevers, that began with rigors, fixed pain in the head, pain about the heart, the fever in appearance very small, yet attended with delirium, and often with a miliary eruption. Those who died had the villous coat of the stomach spread over with inflammatory spots, which swelled its substance, and gave it a brownish purple-colour. These spots were in greatest number about the upper orifice of the stomach. The small guts had also some of these spots. Sometimes the glands of the mesentery were found obstructed, where the larger intestines, and other viscera, were in a sound state.

He cured, or rather stopped the progress of, these distempers, by giving, on the first coming on of the rigors, a cordial and febrifuge electuary.* When the distemper did not yield to this remedy, he had recourse to small bleedings, and gentle physic. Such as were seized naturally with a slight flux, got well with the help of diluting liquors, made a little detersive, such as lemonade; but some of them lasted 40 days, and more. The years 1749, 50, and 51, had the like malignant fevers, some of which were accompanied with violent colics in the beginning, followed with fluxes, which it was found necessary to moderate. He succeeded with 1 or 2 bleedings, after which he gave the decoctum album.†

Some of these diseases had the appearance at first, of a slight peripneumony, or cold, with perpetual faint sweats: then followed a drowsiness and stupor, a rambling for some moments at night, the belly puffed up, and uneasy, little or no urine, then a miliary eruption and delirium; and the patient was carried off in a few days. The stomach in these subjects was inflamed, as also the small guts, by patches. In some there were ulcers, which almost penetrated the sub-

* Kinkina, 1 oz.; Venice treacle and rhubarb, of each ½ oz.; salt of centaury and wormwood, of each 1 dr.; syr. of mercurialis, q. s.—Orig.

† Crumb of bread, 2 oz.; hartshorn-shavings, ½ oz.; root of the greater comfrey, cut in slices, 1 oz.; to be boiled in a quart of water for a ¼ of an hour; strain, and add 1 oz. of diacodium. —Orig.

stance of the intestines. Their lungs were full of blood, and in the back part adhering to the pleura. Those, who had a slight looseness only in the morning, which did not check the sweats, recovered. Some of the malignant fevers, which were at the Hôtel Dieu in 1750, were reported to be caused by infection conveyed in bales of horse-hair, to which was left some of the animals' flesh, that was become putrefied; and yet these fevers did not differ from others which we have already described.

A girl about 20 years of age, who died of this fever, had the mesentery filled with obstructed glands, and the intestines mortified in different places. A man had, besides these symptoms, almost the whole mesentery mortified, and an anthrax or carbuncle at the upper and fore-part of the arm-pit, and the whole body of a livid colour. This carbuncle proves, that these malignant fevers were something pestilential.

M. le Cat makes no mention of the small-pox, which hardly ever leaves this climate in any season of the year, but which is more common towards the end of summer, and in autumn, and for the most part is accompanied with the miliary eruptions, which he had already observed to be joined to all these diseases, and which seldom failed to render them mortal. He opened several of these variolous bodies, and in the greater number found superficial ulcers on the nervous coat of the stomach, towards its upper orifice, with livid and inflammatory spots on the other parts of the same, as also on the intestines (though in a small number) and the glands of the mesentery enlarged, and hardened.

In the year 1752, and beginning of 53, these malignant fevers, that put on the appearance of peripneumonies, became mortal in 7 days, and they discovered, that they were occasioned by a suppurative inflammation of the pericardium. Laxative medicines, quickened by an emetic, were most successful against these inflammations.

About the end of the year 1753, and beginning of 54, these malignant fevers, which had their seat in the stomach, small guts, and partly in the lungs, appeared again, and seized a great number of persons of distinction. This circumstance made them be considered as a new distemper by those who did not attend to it sooner; and the havock they had usually made, being rendered more remarkable by the quality of those who were the unhappy victims, gave the suspicion throughout Europe of having the plague. These reasons redoubled the diligence of the gentlemen of the faculty. The physicians met together, at their college, several times, to communicate their observations on these diseases. M. le Cat thinks they may be divided into 3 degrees.

The patients of the first degree felt, at the beginning, a lassitude, and pain in the joints, attended with some fever, the fits of which went off by sweats.

They perfectly resembled those malignant, wandering, gouty rheumatisms of 1744. But these symptoms were of no long duration; they left the patients long intervals, in which they were able to rise out of bed. There was no great danger attending; and all that was terrible in them was this, that they were of long continuance. The disease of the 2d degree had, besides the foregoing symptoms, a continual fever, with exacerbations, and a pain in the head, that increased as the fever increased. That of the 3d degree began with the symptoms of the first, for 4, 5, and sometimes 8 days; after which it passed to those of the 2d, and was besides accompanied, in the exacerbations, with a cough, sore throat, nausea, a dry, black, and foul tongue; a delirium, or a tendency to it, in the height of the fits, followed by sweats; a remarkable stupidity in the remissions; in some a small oppression of the breast, with spitting of blood; in others a swelled belly, which was slow in every evacuation, especially that of urine. Afterwards there often appeared the miliary eruption; some had a small flux, and blood was perceived in the stools. A great number were affected with a dejection of spirits, and were struck with a sort of terror, as made them tremble at the sound of a common voice.

These diseases ran through a course of 30 or 40 days, which he thinks may be divided into 4 periods. The first, or first 7 days, were passed with the symptoms of the first degree: the next 7 days with those of the 2d degree. In the 3d period, which consisted of about the same number of days, the patient laboured under all the symptoms of the complete disease. Towards the 21st the miliary eruption came on, which led the patient either to death on the 25th, or to recovery about the 30th or 40th day. Some patients, who were attacked with more violence, ran through all the stages in 7 days, as was remarked in 1752; and this short space brought some persons of the most vigorous constitutions to their graves. Many of their bodies were opened, on which they made the following observations:

In some, part of the villous coat of the stomach, and of the small guts, was inflamed; and the rest of these organs were filled with an eruption of the miliary crystalline kind, except that it was larger; and there was likewise an obstruction in the glands of the mesentery. In others, a strong inflammation had seized the whole stomach, and a small portion of the œsophagus; but the intestines were free. These were filled with wind in those subjects whose bellies had been swelled. In those cases, where the delirium had continued long and violent, they found either ulceration on the stomach, or its villous coat separated, with a great inflammation, and even some gangrenous spots on the other coats of that organ. Nothing extraordinary was ever found in the brain.

The most successful method of treating these disorders, was as follows: A bleeding or two, at first, was directly followed by a * vomit. M. le Cat had seen this remedy produce a small flux of 5 or 6 stools a day, which, with the addition of lemonade, was generally sufficient to effect a cure. But when this success did not follow, the patient was bled first in the arm, then in the foot, and every 2 or 3 days there was given some cassia, quickened by an emetic, and dissolved in a decoction of tamarinds. They prescribed ptisans of strawberry-leaves, adding some nitre; lemonades, clarified whey, pure water by itself, a good many simple clysters; draughts of the distilled water of borage and bugloss, sweetened with syrup of lemons and water-lily. Many did well with a simple julep of sugar and water, and a little wine. There were some, who, when they were just sinking, were raised again by cordials of the warmest kind, such as Venice treacle, given in large doses, and the preparation, called vinegar of the 4 thieves,† by spoonfuls, in broth. These medicines brought out a most plentiful miliary eruption, by which they were cured.

The manner of recovery from this disease deserves a place in the history of it. There were but few, who recovered of it in the usual way, that is to say, who only wanted the restoration of their strength, exhausted as well by sickness as the medicines. Almost all of them, even those who had it in the first and second degree, still felt some remains of the symptoms of the disease. Such patients, as had any critical abscesses, were saved by this tribute only; but others, who escaped the mortality of this dangerous poison, carried about with them for several months, and still feel, its terrible effects; for to the usual weakness of convalescents were joined palpitations of the heart; a little of the painful lassitude in the joints, which was a sign of the first attacks of the disease; a slight pain in the head, but almost constant; an uncertain pulse; and, on the lessening or cessation of these complaints, they were replaced by wandering pains in the hypochondria, swimmings in the head, melancholy, and a remarkable disposition to fear, being the remains of what constituted one of the characteristics of the disease.

* It is called in the original, l'emetique en lavage, which signifies an emetic well diluted with water; the formula of which is, 4 gr. of emetic tartar, dissolved in a quart of water; the 4th part of which is given at a time. After this has worked either by vomit or stool, another 4th is taken, and so on, till the patient is supposed to have vomited or purged enough.—Orig.

† This is an infusion of several aromatic plants in vinegar. The reason of its being called vinaigre des quatre voleurs, is this:

When the plague raged at Marseilles, 4 rogues broke into the houses of the sick, and carried off what they pleased, retiring to a secret place with their booty, and returned to the same business at different times, till they had amassed great riches; but were at last apprehended, and hanged. Being asked, how they durst venture into the pestilential houses? they said, they preserved themselves by drinking a glass of their vinegar twice or thrice a day, sprinkling their handkerchiefs and clothes with the same, and were not afraid. The French retain this name for it, though it is not in their dispensatories, and use it as a high cordial.—Orig.

XVI. An Account of the Death of Mr. George William Richman, Professor of Experimental Philosophy, a Member of the Imperial Academy of Sciences at Petersburg. Translated from the High Dutch. p. 61.

In order to demonstrate what Mr. R. might advance in an intended discourse with the greater certainty, he neglected no opportunity on the appearance of a thunder-cloud, diligently to discover its strength. Bars were standing for this purpose always on the roof of the house. These received the electrical power of the clouds, and imparted it to certain chains fastened to them; by which it was conducted into one of his rooms, where his apparatus was. He was attending the usual meeting of the Academy the 26th of July 1753, a little before noon, when it thundered at a pretty distance, the sky being clear, and the sun shining. On this he hastened home, in hopes of confirming his former observations, or possibly enabling himself to make new ones. The engraver Sokolow, who had the care of his future treatise, accompanied him, to make himself the better acquainted with the chief circumstances of the electrical experiment, in order to be enabled to represent it more justly on a copper-plate. Mr. Richman carried the engraver immediately to his apparatus, taking notice of the degree of electricity on his bar, which was then only 4; and by which it appeared, that his bar had received very little from the thunder. He described to Mr. Sokolow the dangerous consequences which would attend the electrical power being increased to the 45th, or more degrees of his expositor. In the mean time the misfortune happened, about half an hour after noon, which cost Professor Richman his life. A thick cloud, that came from the north-east, and seemed to float very low in the air, was taken notice of by people walking in the street; and these affirm, that they could plainly see, on the subsequent flash of lightning, and peal of thunder, a quantity of vaporous matter issue from it, which diffused itself in the circumjacent space. It was such a thunder-clap as has hardly been remembered at Petersburg. The serene weather continued afterwards just as before. An English captain observed, that as the wind had been till then easterly, not long before the thunder it veered about to westward, but immediately after the stroke it returned to its former point, east. By this it appears in what manner the inflammation of the electrical particles followed so quickly, the wind driving it against another cloud, not so pregnant with that combustible matter. The neighbours declare, that they saw through their windows a vapour, in different rays, dart along the whole extent of the street; and that wherever it touched the ground, it emitted every where sparks; which is not incredible; for there were people who, walking along between these rays of vapour, were quite stunned, and some beaten to the ground, though they speedily recovered again.

A centinel in the Great Perspective*, not far from Mr. Richman's house, which stands at the corner of the said Perspective, was thrown some paces from his centry-box, but without receiving any injury. It is not therefore to be doubted but that this very thunder-cloud, or its electrical discharge, must have struck the iron bars, which were on Mr. Richman's house-top; by which a great part of the electric force was conducted, by means of the chains, to his electrical expositor; and thus it could not fail of having the melancholy effect, the parallel of which has not been known. According to the account of the engraver Sokolow, Mr. Richman inclined his head towards the expositor, to observe what degree of force it would have; and while he stood in that bent posture, a great white and bluish fire appeared between the electrical expositor and Mr. Richman's head. At the same time arose a sort of stream, or vapour, which entirely numbed the engraver, and made him sink down on the ground; so that he cannot remember to have heard the loud thunder-clap. The iron ruler belonging to the expositor, which hung perpendicular, as it received all the force from the bars and chains, cast from it a thread, which was fixed to its top, and drove it upward towards the expositor. That this ruler might point out the degrees of strength, that for its more powerful operation, it stood with its lower end in a glass vessel, filled with brass filings. This ruler hanging right, a globular flame has been always produced, as well by artificial electricity as that of the clouds, which may be denominated natural electricity. This being now stopped, by the filings and glass vessel, from taking its direction downwards, seems to have expanded itself round about the ruler, and by those bodies, incapable of electricity, to have been carried on towards Mr. Richman. And this is further confirmed, because they afterwards found the vessel broken in pieces, and the filings scattered about. The particulars, which happened to Mr. Richman, Mr. Sokolow is ignorant of. As soon as he had recovered his senses, he got up, and ran out of the house, acquainting every one whom he met in the street, that the thunder had struck into Mr. Richman's house. On the other side, as soon as Mrs. Richman heard the very loud stroke of thunder, she came hastening into the chamber, in which she conjectured she should see the bad consequences. She found her husband past sensation, sitting upon a chest, which happened to be placed behind him, and leaning against the wall; which situation must have been occasioned by his falling back on receiving the electrical blow. He was no sooner struck than killed. There was not the least appearance of life. A sulphureous smell, not unlike that which is caused by the explosion of gun-powder, diffused itself through the whole house. Some servants, who were hard by in the kitchen, felt its effects, being quite stupified. The electrical expositor stood on

* Probably a street so called.

a low beaufet, upon which was likewise placed a China bowl that was cracked; and there was such a shaking in the house, that the shock even stopped the movement of an English clock, or pendulum, which was in an adjoining room. There was no other inflammation happened in the house. But we have found another effect of the force of electricity, or of thunder-bolts, discoverable by the door-posts of the house; for they were rent asunder length-wise, and the door, with that part of the posts, so torn away, twirled into the porch. The reason of which appears to be, because one of the above-mentioned chains, that were carried from the bars at the house-top to the expositor, passed very near them: and the kitchen door, being at a little distance off, had a splinter torn out, and dashed against a stair-case, that went towards the top of the house; so that part of the elecrical matter seems to have taken its course this way, but without doing any more damage. They opened a vein of the breathless body twice, but no blood followed. They endeavoured to recover sensation by violent chafing, but in vain. On turning the corpse topsy-turvy, during the rubbing, an inconsiderable quantity of blood fell out of the mouth. There appeared a red spot on the forehead, from which spirted some drops of blood through the pores, without wounding the skin. The shoe belonging to the left foot was burst open. Uncovering the foot at that place, they found a blue mark, by which it is concluded, that the electrical force of the thunder having forced into the head, made its way out again at the foot. On the body, particularly on the left side, were several red and blue spots, resembling leather shrunk by being burnt. Many more blue spots were afterwards visible over the whole body, and in particular on the back. That on the forehead changed to a brownish red. The hair of the head was not singed, though the spot touched some of it. In the place where the shoe was unripped, the stocking was entire; as was his coat every where, the waistcoat being only singed on the fore-flap, where it joined the hinder. But there appeared on the back of the engraver's coat long narrow streaks, as if red-hot wires had burnt off the nap.

When the body was opened the next day, 24 hours afterwards, the cranium was very entire, having no fissure or cross-opening; the brain as sound as possibly it could; the transparent pellicles of the wind-pipe were excessively tender, gave way, and rent easily. There was some extravasated blood in it, and in the cavities below the lungs; those by the breast being quite sound, and not damaged, but those towards the back of a brownish black colour, and filled with more of the above blood; otherwise none of the entrails were touched; the throat, glands, and the thin intestines, were all inflamed. The singed leather-coloured spots penetrated the skin only. In short, though one could trace out all the consequences of an instantaneous stroke throughout the whole body, yet many of them have not appeared to happen to others struck by thunder, when they have been examined. Should not one therefore be led to conclude, that the electrical force, that occasioned Mr. Richman's death, must have been of a different substance from the common thunder-bolt? That it was much more subtile, is obvious, because it left so few visible traces in the body, which it penetrated. Twice 24 hours being elapsed, the body was so far corrupted, that it was with difficulty they got it into a coffin.

Mr. R. was born the 11th of July, 1711, at Pernau, after the decease of his father, Mr. Wm. Richman, treasurer of the king of Sweden, who was carried off by the plague, at the close of the year 1710. Having laid the foundation of his learning at the Gymnasium at Revel, he prosecuted his studies at the universities of Halle and Iena, where he always made the mathematics and philosophy his principal objects. He was made a member of the Imperial Academy in the year 1735; extraordinary professor in 1741; and at last, in 1745, ordinary professor of experimental philosophy.

XVII. Of a Roman Inscription found at Malton in Yorkshire, in the Year 1753. By John Ward, LL.D. Rhet. Prof. Gresh. and V.P.R.S. p. 69.

This inscription was dug up in the Pye Pits, opposite the lodge at Malton, a town situated on the river Derwent, in the North Riding of Yorkshire. And the inscription, in words at length, may be read in the following manner:

Diis Manibus. Aurelius Macrinus, ex equitibus singularibus Augusti.

The peculiarity of this inscription, and what renders it remarkable, is the character of the person, to whose memory it was erected. These equites singulares are often mentioned in Gruter, Fabretti, and other collectors of ancient monuments; but this is the first instance of them, which has ever occurred in any of our British inscriptions. Modern writers have differed very much in their sentiments, concerning the particular office and duty of this part of the Roman cavalry; but Dr. W. thinks it most probable that these equites singulares made part of the emperor's body guards. Reinesius was of opinion, that they not only attended the emperors themselves, but also the governors of the Roman provinces, in the like station; though Fabretti, who has given a large collection of these inscriptions, declares that he had met with no sufficient evidence of this, either from ancient writers or inscriptions. Schelius, in his notes on this passage of Hyginus, thinks that they were first instituted by Augustus. And there is an inscription in Gruter, which mentions one of these equites singulares as having served under Augustus in several of his wars, and been rewarded by him.

This account of the origin and station of that body of Roman horse may afford some light in settling the time, when this funeral monument of Aurelius Macrinus was erected. For if they always attended on the emperor himself, some one of the Roman emperors must then have been resident in Britain. And as Severus resided here for about 3 of his last years, and died at York; it seems most probable that this monument was set up within that time. And to this both the form of the letters on the inscription very well agree, and the ligature of the two letters G and A at the end of it. Fabretti observes, that these equites singulares had a burying-place allotted them at Rome, in the Via Labicana, not far from the sepulchre of the empress Helena. Several of their monuments have been found in that cemetery, adorned at the top with a human figure, lying on a couch; and below the inscription, a horse with trappings, and a boy holding a whip. And if any such are met with elsewhere, they have, as he supposes, been removed from thence. Montfaucon has given a draught of one of those monuments, which contains the inscription recited above, and answers to this description of Fabretti, both as to the human figure, and that of the horse; the former of which has a patera in the left hand, and a mask is suspended at each end of the couch; and the boy, who is there wanting, he found on another. Those ornaments might very probably be omitted on such monuments, when erected in the provinces; and it is plain there could not be room for the human figure above the inscription in this of Malton. At which place, as Mr. Borwick says in his letter, many urns, coins, and other remains of antiquity, have been found, in and about the Pye Pits; whence he supposes it to have been a cemetery for some Roman garrison.

In one inscription the emperor Commodus is himself called eques singularis, for the explication of which character recourse must be had to the accounts given by historians of his life and actions. And among other instances of his base and infamous conduct, he is said to have demeaned himself to that degree, as to act a part in most of the public games that were celebrated at Rome. Thus, one of his diversions was to attack wild animals in the amphitheatre; at which exercise he was so expert, as never to miss his aim in killing them, either with a javelin or an arrow. He would often combat with the gladiators, and was so fond of that character, that he assumed the name of one of them, who had been very famous. At other times he would act as a charioteer in the Circus. He joined also in the athletic exercises, and was at last strangled by a champion, with whom he had formerly engaged. Dr. W. does not find indeed, that he is ever mentioned by historians as a racer on a single horse, which is the character given him in the inscription; as appears from Isidore, who calls them equites singulares, as distinguished from the desultores. But that horse-racing was also one of his recreations, we learn from a passage in Dion Cassius; who says that Commodus came once to Rome on a sudden, when he was not expected, and exhibited a race of 30 horses in the space of 2 hours. It is not improbable therefore, that he might sometimes take a part in that exercise, as well as in those above mentioned. And as he affected to have all his actions, however shameful or ridiculous, publicly recorded, this inscription might have been erected in compliment to him under that character.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the Company of Apothecaries for the Year 1745, *Pursuant to the Direction of Sir Hans Sloane, Bart. &c.* p. 78.

[This is the 33d presentation of this kind, completing to the number of 1650 different plants.]

XIX. On the Advantage of taking the Mean of a Number of Observations, in Practical Astronomy. By T. Simpson, F. R. S. p. 82.

It is well known that the method practised by astronomers, to diminish the errors arising from the imperfections of instruments, and of the organs of sense, by taking the mean of several observations, has not been so generally received, but that some persons of note have publicly maintained, that one single observation, taken with due care, was as much to be relied on, as the mean of a great number. As this appeared to be a matter of much importance, Mr. S. was inclined to try whether, by the application of mathematical principles, it might not receive some new light; whence the utility and advantage of the method in practice might appear with a greater degree of evidence.

But the rest of this paper will be better consulted in Mr. Simpson's Miscellaneous Tracts, published in 1757, where the paper is much improved. From a particular example which Mr. S. calculates, he infers that the chance, for an error exceeding 2 seconds, is not $\frac{1}{10}$ part so great from the mean of 6, as from one single observation. And it will be found, in the same manner, that the chance for an error exceeding 3 seconds, will not be $\frac{1}{1000}$ part so great from the mean of 6, as from one single observation. On the whole of which it appears, that the taking of the mean of a number of observations, greatly diminishes the chances for all the smaller errors, and cuts off almost all possibility of any great ones: which last consideration alone seems sufficient to recommend the use of the method, not only to astronomers, but to all others concerned in making experiments of any kind, to which the above reasoning is equally applicable. And the more observations or experiments there are made, the less will the conclusion be liable to err, provided they admit of being repeated under the same circumstances.

XX. Of the Success of Agaric, and the Fungus Vinosus, in Amputations. By Mr. James Ford, Surgeon, of Bristol. p. 93.

Mr. F. here gives an account of 2 cases of amputation, in which the agaric was successfully employed as a styptic.

XXI. Queries sent to a Friend in Constantinople. By Dr. Maty, F.R.S. and answered by James Porter, Esq. F.R.S. Ambassador there. p. 96.

1. Whether we may know with any certainty, how many people are generally carried off by the plague at Constantinople? 2. Whether the number of inhabitants in that capital may be ascertained? 3. Whether what has been advanced by some travellers, and from them assumed by writers on politics, be true, that there are more women than men born in the east? 4. Whether plurality of wives is, in fact, as it was confidently affirmed to be, in the order of nature, favourable to the increase of mankind? 5. What is the actual state of inoculation in the East? 6. What is become of the printing-house at Constantinople? and are there any original maps of the Turkish dominions, drawn from actual surveys? 7. What sort of learning is cultivated among the Greeks, and among the Turks?

To these 7 queries Mr. Porter made the following answer:

1. The only plague, which he observed at Constantinople, in the course of 7 years, was that of the year 1751: there are almost annually dispersed accidents, some perhaps real, some suggested by trick and design, to serve sinister purposes.

2. The Turks have no register, no bills of mortality: they are prohibited, by their law, from enumerating the people. He applied to the Reis Effendi, and other ministers of the Porte, to know what probable calculation they could make concerning the number of dead; but they all concurred in one general answer, that they had no other but what was founded on the decrease of the consumption of the quantity of corn, or bread; and in general talked of about 150000. Corn is delivered out by an officer of consideration, and an exact register kept. Before the commencement of the plague, in March and April 1751, the consumption of corn was 19000 measures, called khilos. On its continuance and decrease it diminished to 17000, and on its total cessation, it was found not to amount to above 14000. A khilo weighs 22 okes. It is ground to 18 okes of flour. The bakers have generally the secret to make out of this last quantity 27 okes of bread. They add to an oke of flour one of water, besides some salt; and as their bread is almost dough, few of the watery particles are exhaled; and it is esteemed good if it is not doubled in quantity, when taken out of the oven.

The people live principally on bread; the poorer with onion, garlick, fruits, or pulse, according to the seasons; the others with very small portions of flesh, or fish. The more laborious professions, as labouring men, stone-cutters, carpenters, &c. eat from 2 to 2½ okes a day; the other, according to the common run of families, composed of men, women, and children, half an oke each; so that the lowest calculation, on a medium, may be about an oke and a quarter daily, eaten by each person at Constantinople. But should it be thought too much, an oke, which is $2\frac{3}{4}$ lb. English, he supposes nearer truth: the following conclusions then will result: That therefore on the highest number of 19000 × 27, we have 513000, the quantity of okes of bread consumed, and consequently the number of souls at Constantinople. That on the decrease of the plague to 17000, 54000 persons were either dead or missing. That when the quantity was reduced to 14000 on the cessation, those either fled or dead amounted to 135000.

It is said by some, that Constantinople contains near 3 millions of inhabitants; but on whatever supposition we take the consumption of the quantity of bread, that quantity will be found erroneous. On a gross calculation made by some of the principal men, and particularly the Chiorbachees, or colonels of Janizaries, who had their stations at the most noted and only places where the funerals pass, they reckoned for 6 weeks, while the plague was at its height, and in its crisis, from 900 to 1000 per diem; and that the whole amount of the dead in that time might be about 40000: and from the time it was in its increase and decline, they added 15 to 20000 more. If therefore we admit 60000 in the whole, it will be as that sum to 513000, or as 1 to $8\frac{11}{20}$.

There is a remarkable coincidence between this proportion, and the number of dead which was carried out of the Adrianople-gate, during 12 days, the same season of the year 1752; and of the like number of days in 1751.

Hence the number of dead, at least through that gate, in time of common health, was to those in that of sickness, as 59 to 489, or as 1 to $8\frac{1}{3}$, nearly. The Adrianople gate is reckoned the greatest passage for the dead, on account of its vicinity to the most extensive burial-places. A great deduction must be made for the vast decrease of the consumption of wheat towards the cessation of the plague, from the considerable numbers, who fled into Asia, the islands of the Archipelago, and Romelia.

It is extremely difficult, if not impossible, to come at any other computus of the number of inhabitants, much more so of houses, at Constantinople. The city is divided somewhat in the manner it was under the Grecian empire, that is, into different quarters, called Mahales, and each under the special direction of an Imaum. As far as it extends to their immediate advantage, they are informed of the number of families in their district; but whoever would dare to collect from them, might not only risk the censure of the government, but his head. Besides, if the inquiry is general concerning houses, it is impossible to fix a determined idea; they confound palace, seraglio, shop, room, and call them indiscriminately houses. The Jews say, that they have 10000 houses at Constantinople: but in what we call a house, there are perhaps 10 families, and the distinct number of the latter they dare not mention. Mr. P. endeavoured with persuasion, and all his weight, to induce the Greek and Armenian patriarchs, to obtain for him a register of the births and burials of their respective communities; but at length they acknowledged it impossible. Their parishes are farmed to curates, by the diocesan bishops; the income arises from births and burials; so that to conceal the former, they must likewise the latter; and they never exhibit a faithful register.

3. That there are more women than men born in the east, seems a figment of travellers, rather than founded in truth; it is scarcely to be known where polygamy is lawful. The apparent conclusion may seem natural, because many of the harems of the opulent, especially in the great cities, are numerous: but these are not composed of the natives of those cities, but are brought from countries where the Christian rites are observed; in time of peace, from Georgia, and in war from Hungary and Russia, &c. so that if more women are found in such families than men, they must be considered as an extraneous production annually, or daily imported.

4. Mr. P. affirms it as a truth, that in general, Mahometans, notwithstanding their law, procreate less than Christians. The rich, who are the only persons that can maintain concubines, have seldom 4 or 5 children. Few exceed 2 or 3; many of the former, and most of the middling and poorer sort, have generally but one wife. The latter indeed exchange them with facility; but yet we do not perceive they have a numerous progeny. He thinks this arises from a cause different from that which is commonly assigned, not from their being enervated by variety, but rather from their law. The frequent ablutions, required by the doctrine of purity and impurity, perhaps may check the natural passion; or when it is at its height, they find themselves prohibited enjoyment.

5. Inoculation is practised at present among the Greeks, and, notwithstanding religious scruples, among the Romanists: with the few he had known, it generally succeeded; but the numbers will not admit of comparison. There are not perhaps 20 in a year inoculated. The Timoni family pretend, that a daughter had been inoculated at 6 months old, but afterwards acquired the small-pox in the natural way, and died at 23 years. The evidence is doubtful. Timoni's account is incorrect; his facts are not to be depended on. Pylarini's is more exact. It was neither Circassians, Georgians, nor Asiatics, who introduced the practice. The first woman was of the Morea; her successor was a Bosniac; they brought it from Thessaly, or the Peloponnesus, now Morea. They properly scarified the patient, commonly on many parts, sometimes on the forehead, under the hair, sometimes on the cheeks, and on the radius of the arm. A father told Mr. P., that the old woman not being able, through age, to make the incision on his daughter, with the razor, he performed that operation. The needle has also been used. The Turks never inoculate: they trust to their fatum. Whence the method had its origin seems here unknown. A Capuchin

friar was on a mission in Georgia for above 16 years; he has returned about 2 years; he is a grave sober man, who gives an historical account of the virtues and vices, good and evil, of that country, with plainness and candour. The usual introduction and security of these missionaries is the pretence to the practice of physic, that in destroying bodies they may save souls: so that this honest man, who is extremely ignorant, was in high reputation both as physician and confessor. It was therefore impossible, as he himself observes, that either the public or private practice of inoculation could be concealed from him; but he has most solemnly declared to Mr. P. repeatedly, that he never heard one word about it at Akalsike, Imirette, or Tifflis; he is persuaded, that it has never been known among them. He has often and frequently attended the small-pox, which is almost certain death there; and generally, if not always, of the confluent kind.

6. Printing was introduced by an Hungarian renegado, who called himself Ibrahim Effendi: it had no long continuance. The copies are not many, and are now very dear and scarce; few even to be bought. The maps did not exceed 3 or 4; one of Persia, one of the Bosphorus, and one of the Euxinus, or Black-sea; they are not to be found but in private hands. All our maps of these countries are extremely imperfect and incorrect. The jealousy and superstition of the people, though the government should permit Christians to raise any printing-house, would be an irresistible impediment; and they are too ignorant themselves to be ever capable of doing it. The adoptive son of this Ibrahim Effendi, who bears the same name, is secretary under the interpreter of the Porte; he has all the materials for printing, but never could find, since his father's death, and during Sultan Mahmud's reign, money to carry it on. The question is now, whether Sultan Osman is not too strict a mussulman to continue the permission.

7. The progress of arts and sciences, and literature, seems travelling on, gradatim, to the westward, from Egypt to Greece, from Greece to Rome, thence to the west of Europe, and he supposes at last to America. We find few traces in the east: the Greeks, who should be the depositaries of them, are the same Greeks they ever were, Homines contentionis cupidiores quam veritatis. They have retained all the vices, imperfections, ill habitudes, of their ancestors; but have lost all their public spirit, and public virtues. The clergy, who should support the whole machine of learning, are themselves the source of ignorance; all their talents and acquisitions consist in bribing among the Turks, and sollicit-ing to destroy one patriarch, in order to make another; to raise from a curacy to a bishoprick, and to exchange from an indifferent one to a better. They endeavour to cultivate literal Greek, and some study it, but advance no further. There are neither grammarians, critics, historians, nor philosophers, among

them; nor have they proper preceptors or masters to instruct. They have formed a sort of an academy at Mount Athos, for their youth, which will scarcely survive the person who has undertaken it: he has himself but the mere elements of science. However, his desire of knowing may improve him; and he may perhaps lay the foundations in some youth with success.

The Turks have many books among them, though exceedingly dear; folios he had seen cost 100 to 2 and 300 dollars each; i. e. from 15l. to 45l. The few printed folios, some of which he picked up some years before, cost 5l. to 6l. sterling. Their scribes spend many years about a few copies. Their learning consists principally in abstruse metaphysics: some few touch the surface of science. He had looked out with great industry for old Arabian manuscripts in the mathematical way: what they brought him were translations of some propositions of Euclid, Theodosius, Archimedes, and Apollonius. They have some parts of Aristotle; but their favourite philosophy is the atomical or Epicurean, which with them is called the Democritical, from Democritus. Many of their speculative men have adopted that system, and conform to it in their secret practice. The institutes and practice of physic are taken from Galen. Eben Zyna, or Avicena, is a principal guide: Mathiolus is known. But with all this, as the sole drift and end of their study is gain, there does not seem the least emulation towards true knowledge: so that the state of letters may be said to remain deplorable, without the least glimmering or remote prospect of a recovery.

P. S. Mr. P. corrects the report of the Capuchin concerning inoculation in Georgia. One of their physicians, a most ignorant fellow, who lives by his profession here, avers that, among those who follow the true Georgian rites, not Romanists, the practice is common. It has its rise from mere superstition. He tells us, " That the tradition and religious belief of that people is, that an angel presides over that distemper; that therefore, to show their confidence in him, and to invite him to be propitious, they take a pock from the sick person, and, by a scarification, they insert it in one in health, generally between the fore-finger and thumb. It never misses its effect, and the patient always recovers. To attract the angel's good-will more effectually, they hang the patient's bed with red cloth or stuff, as a colour most agreeable to him. He has been assistant to this practice, and declares it to be common." The Capuchin acknowledges, that it might be among the Georgians the Doctor mentions, and not have fallen under his knowledge.

XXII. Extracts of Two Letters to Thomas Hollis, Esq. concerning the late Discoveries at Herculaneum. p. 109.

Near the royal palace at Portici, has been discovered a large garden, with a palace belonging to it. In one room of this palace was found a mosaic pavement, made up of different coloured stones. It represents a city surrounded with walls, having 4 towers, one at each corner; and has since been taken up, to be placed with other beautiful antique pavements in the king's gallery.

Extract of a Letter from Camillo Paderni, dated at Naples, Jan. 1755.

October 22, 1754, was found a bust in bronze, larger than the life, and of excellent Greek workmanship; which from some circumstances may be thought to be a Syrian king. It has eyes of white marble, like many other busts, which have been met with. November 27, was discovered the figure of an old fawn, or rather a Silenus, represented as sitting on a bank; with a tyger lying on his left side, on which his hand rested. Both these figures served to adorn a fountain, and from the mouth of the tyger had flowed the water. This Silenus was of bronze, and of good workmanship. The head was crowned with ivy, the body all over hairy, and the thighs covered with a drapery.

From the same spot were taken out, November 29, three little boys of bronze, of a good manner. Two of these are young fawns, having the horns and ears of a goat. They have silver eyes, and each a goat-skin on his shoulder, in which they anciently put wine, and through which here the water issued. The third boy is also of bronze, has silver eyes, is of the same size with the two former, and in a standing posture like them, but is not a fawn. On one side of this last stood a small column, on the top of which was a comic mass, that served as a capital to it, and discharged water from its mouth. All the figures before described are two palms in height without their bases.

December 16, in the same place were discovered another boy, with another mask, and 3 other fawns; in all respects like those which were found the 27th and 29th of November, except that there was no tyger. Besides these, they met with 2 little boys in bronze, somewhat less than the former. These likewise were in a standing posture, had silver eyes, and held each of them a vase, with handles, on his shoulder; hence the water flowed. They also dug out an old fawn, crowned with ivy, having a long beard, a hairy body, and sandals on his feet. He sat astride on a large goat skin, holding it at the feet with both his hands, from which had issued a larger quantity of water than from the others; though the fawn himself is of the same size with the former.

All the above-mentioned figures were taken out of a place not exceeding 8 palms square, and were covered with the ruins of the building; for they were not in a garden, but in a room paved with mosaic work, the remaining part of which we are now going on to examine. We have likewise found a large quantity of household furniture, made of earthen and iron ware, and some glass.

XXIII. On the Books and Ancient Writings dug out of the Ruins of an Edifice near the Scite of the old City of Herculaneum. Translated by John Locke, Esq. F. R. S. p. 112.

Within 2 years last past, in a chamber of a house, or more properly speaking, of an ancient villa, in the middle of a garden, has been found a great quantity of rolls, about a palm long, and round; which appeared like roots of wood, all black, and seeming to be only of one piece. One of them falling on the ground, it broke in the middle, and many letters were observed, by which it was first known, that the rolls were of papyrus. The number of these rolls, were about 150, of different sizes. They were in wooden cases, which are so much burnt, as are all the things made of wood, that they cannot be recovered. The rolls however are hard, though each appears like one piece. The king has caused infinite pains to be taken to unroll them, and read them; but all attempts were in vain; only by slitting some of them, some words were observed. At length Sig. Assemani, being come a second time to Naples, proposed to the king to send for one Father Antonio, a writer at the Vatican, as the only man in the world, who could undertake this difficult affair. It is incredible to imagine what this man contrived and executed. He made a machine, with which, by the means of certain threads, which being gummed, stuck to the back part of the papyrus, where there was no writing, he begins, by degrees, to pull, while with a sort of engraver's instrument he loosens one leaf from the other, which is the most difficult part of all, and then makes a sort of lining to the back of the papyrus, with exceedingly thin leaves of onion, if I mistake not, and with some spirituous liquor, with which he wets the papyrus, by little and little as he unfolds it. All this labour cannot be well comprehended without seeing. With patience superior to what a man can imagine, this good father has unrolled a pretty large piece of papyrus, the worst preserved, by way of trial. It is found to be the work of a Greek writer, and is a small philosophic tract, in Plutarch's manner, on music; blaming it as pernicious to society, and productive of softness and effeminacy. It does not discourse of the art of music. The beginning is wanting, but it is to be hoped, that the author's name may be found at the end; it seems however to be the work of a stoic philosopher; because Zeno is much commended. The papyrus is written across in so many columns, every one of about 20 lines, and every line is the 3d of a palm long. Between column and column is a void space of more than an inch. There are now unrolled about 30 columns; which is about a half of the whole; this roll being one of the smallest; the letters are distinguishable enough. Father Antonio, after he has loosened a piece, takes it off where there are no letters; and places it between two crystals for the better observation; and then, having an admirable talent in imitating characters, he copies it with all the lacunæ, which are very numerous in this scorched papyrus, and gives this copy to the Canon Mazzocchi, who tries to supply the loss, and explain it. The letters are capital ones, and almost without any abbreviation. The worst is, the work takes up so much time, that a small quantity of writing requires 5 or 6 days to unroll, so that a whole year is already consumed about half this roll. The lacunæ, for the most part, are of one or two words, that may be supplied by the context. As soon as this roll is finished, they will begin a Latin one. There are some so voluminous, and the papyrus so fine, that unrolled they would take up 100 palms space.

XXIV. On the several Earthquakes lately felt at Constantinople. By James Porter, Esq. p. 115.

This paper is quite unimportant, containing only some trite remarks on the wind and weather, and accounts of some very trifling earthquakes that lately happened, with no circumstances of any consequence.

XXV. Letters of Henry Eeles, Esq. concerning the Cause of the Ascent of Vapour and Exhalation, and those of Winds; and of the general Phenomena of the Weather and Barometer. p. 124.

It is agreed, that the ascent of vapour and exhalation through the air may be effected in two ways, by impulse, and an alteration of their specific gravity. That vapour does not generally ascend by impulse, may be proved by many familiar experiments, viz. put boiling water into a vessel; then empty it, and hold the vessel with the aperture downwards; the vapour, which is afterwards expelled from the vessel, must be in a direction downward; but we find, that as soon as it has got but a very little below the rim of the vessel, it has its direction altered, and ascends by the laws of specific gravity. The same thing may be observed in all boiling vessels, where the vapour is emitted in a direction downward; or, in cold weather, when the vapour of a man's breath may be seen, let him breathe downward, and the direction of his breath will be presently altered, as in the former case. Since then vapour ascends without any other impulse than that which is incident on all bodies ascending by the laws of specific gravity; it is necessary to inquire, how the specific gravity of vapour is altered, to cause its ascent. This is generally supposed to be done by filling vesicles of water with rarefied air, till the diameter of the vesicle be 10 or more times the diameter of a drop of water, composed of the same constituent particles; and that the vesicle, by this means, becomes specifically lighter than air. But Mr. E. thinks that this cannot be done so easily as it has been generally imagined; and when done, it will not be sufficient for the purpose; which he infers from the following considerations. First, the great difficulty in forming those vesicles, especially of the particles of dry bodies carried off by exhalation, and filling them with rarefied air, while the exterior air remains condensed. Secondly, that there is not any allowance made for the weight of the included air. Thirdly, the constituent particles of water are but very little, if at all altered in their specific gravity. Fourthly, that this thin vesicle can never be a sufficient boundary between the exterior condensed air and the interior air, so exceedingly rarefied.

Rejecting these popular opinions, as to the cause of the ascent of vapours and exhalations, Mr. E. observes that it now remains to inquire, by what means this may be done; since neither impulse, rarefaction of the air, nor any formation of their parts by expansion, seem sufficient for the purpose. There appears to him but one way of altering the specific gravity of the particles of vapour and exhalation, to render them lighter than air, which is by adding to each particle a sufficient quantity of some fluid, whose elasticity and rarity are exceedingly greater than that of the air. That the fluid or fire of electricity is such, will be easily granted; but how far it is adapted to this purpose, we must inquire from experiments. For the purpose is great; no less than all vegetation and animal life depending on the ascent and descent of vapour and exhalation.

Mr. E. says he has made some experiments, by which it appears that all fumes arising from fire, whether blazing or otherwise, and all steams rising from boiling or warm waters, and from all other fluids, and the breath of man, and of all other animals, and all the effluvia thrown off by perspiration, are strongly electrified. But he now only mentions a few. First, that desultory motion, by which it flies off from an electrified body to any number of non-electrics, which are brought within the sphere of its activity and affection, till it be equally diffused through all. Secondly, that the sphere of its activity is increased by heat. Thirdly, that this fire does not mix with air. Fourthly, that it intimately pervades water, and many other bodies, covering their superficies to a certain distance; which distance is not in proportion to the bulk of the body electrified, but in proportion to the state of activity of the electrical fluid. Fifthly, this electrical fluid readily joins with any fire which fumes, or rather with the blaze or fumes of any fire; but will not mix or fly off with the fire of red-hot iron, or any other metal, which does not fume.

Now, to show that this electrical fire or fluid is the principal cause of the ascent of vapour and exhalation, we need only prove that it attends all vapour and exhalation, and that in such quantity, as is necessary to render them specifically lighter than the lower part of the atmosphere. He does not undertake to determine, by what cause vapour and exhalation are detached from their masses, whether by the solar or culinary fire, or by the vibrations of the electrical fluid rendered more active by those fires; though he thinks the latter. But it is evident, that they are emitted in exceedingly minute distinct particles, and that these particles must pass through that electrical fluid; which surrounds the surface of the mass; and that, by that means they must be equally electrified with the mass, that is, they must be covered with the electrical fluid to as great a distance from their superficies as the mass is covered; which must always be in proportion to the state of activity of the electrical fluid. In which state, when they have passed the surrounding fluid, they must be repelled by it; and also repel each other; and if each particle of vapour, and its surrounding fluid, occupy a greater space than the same weight of air, they must be fitted to ascend till they come in equilibrium with the upper and rarer part of the atmosphere; where they must float, till their specific gravity is altered. As it is very difficult to assign the magnitude of each particle of vapour and exhalation, and that of the surrounding fluid; and to show that both, taken together, occupy a greater portion of space than the same weight of air; we can only apply to experiment, to show that it is possible that it may be so; and that will show, that in all probability it is so; since it is evident, that every particle must be endued with a portion of this electrical fire or fluid; and that there is not any other sufficient cause assigned for their ascending.

It is evident, that on electrifying any light matter, such as down, or the downy parts of feathers, their specific gravity is much lessened; and that, by holding another electrified body under them, they may be driven upwards at pleasure. It is also evident, from experiment, that the more you divide the parts of such bodies, the more of their specific gravity they will lose by being electrified; and by dividing them into very minute parts, that they ascend to a considerable height after they are electrified. Hence he thinks it highly probable, that the exceedingly small particles of vapour and exhalation may be, and are, sufficiently electrified, to render them specifically lighter than the lower air; and that they do ascend by that means. And that they will ascend proportionally higher, as the surrounding fluid is proportionally greater than the particle which is carried up.

Mr. E. then endeavours to show that the ascent and descent of vapour and exhalation, attended by this fire, is the principal cause of all our winds. It being admitted that wind is only air put into motion, many have been the conjectures how that motion is caused. Among which, the motion of the earth, and the air's being rarefied by the sun, seem to stand first. The trade winds being most regular, and occupying a considerable part of the globe, it has been thought proper first to account for them, from the afore-mentioned causes. But he thinks that these causes, by themselves, are not sufficient for the motion of those winds, and much less so for the irregular motion of all the other winds. If the apparent motion of the air was occasioned by the diurnal revolution of the earth from west to east, by the air's being left behind, the motion must be found more regular, and very different from what it is; for in that case the greatest motion must be at the equator, and from thence lessen gradually to the poles; and must be continued always equally one way, both day and night, and at all seasons. But we find quite the contrary: the most gentle gales blowing at the equator and between the tropics pretty steadily, one way all day long, and dying away at night; while high winds and storms, blowing all manner of ways, are found in the higher latitudes. Neither does he think that the sun's rarefying air can simply be the cause of all the regular and irregular motions found in the atmosphere; but he thinks the cause is the ascent and descent of vapour and exhalation, attended by the electrical fire, or fluid.

Now, all the vapour and exhalation, raised in the torrid zone, being buoyed up by the electrical fire, must add a column to the air, though of a different matter, at least 1000 times greater than the vapour and exhalation taken up; which column must necessarily force the adjacent part of the incumbent air upwards, and must as necessarily be reacted on by the incumbent air, to restore the equilibrium of the whole air. And as it cannot be readily forced down again, it must float off, at that altitude, toward those parts where little or no addition has been made to the atmosphere; and by that means must propel the air on the horizontal level with it, and that below it, as it is itself propelled by the weight of the incumbent air. And that motion must be from the equator, where the greatest quantity of vapour, &c. is raised, toward the poles, and partly to the west; as the column of vapour is always rising from east to west, as the earth turns toward the sun. For here we must confess, that the sun is the great agent in detaching vapour and exhalation from their masses; whether he acts immediately by himself, or by his rendering the electric fire more active in its vibrations; but their subsequent ascent Mr. E. attributes entirely to their being rendered specifically lighter than the lower air, by their conjunction with this electrical fire. The fire, which surrounds the vapour, beginning to condense, and the vapour to subside, in passing the tropics, becomes a greater pressure on the air beneath, and by that means forces some part back into the tropics, in the place of that air protruded by the ascent of the vapour, &c. and the remainder in a direction toward the poles. The common rotation of the air in coming in below, to supply the place of that part carried up by any fire, may explain this motion. To show how this motion must tend to the west, we must consider, that the column of air, raised by the ascending vapour, &c. is at its greatest altitude to the east, and therefore must press that air to the westward, which is continually protruded by the vapours, &c. beginning to ascend from east to west; and the compressed air at the tropics must tend to the westward, till their forces meeting make the motion entirely to the west. The air itself being rarefied, and carried up by the reflection of the intense heat of the sun, may be a considerable additional cause of these trade winds; but never can be the sole cause of all the erratic winds. To account for all the irregular winds within the tropics, he says, that where such happen, it must be by means of some tracts of land, which rise to a greater height above the horizontal level, than vapours generally do; by which the motion of the vapours is stopped, and the vapour accumulated by succeeding vapour, and the air, on which they float, is of consequence pressed into a new direction. And from hence may also be explained the cause of the rains, particularly so called in the sea language.

He next considers what becomes of the vapour, &c. floating from over the tropics toward the poles; which being less affected by the heat of the sun, reflected from the surface of the globe, the surrounding electrical fire begins to condense more and more as it moves toward the poles, and the vapours of course to descend; and that part most, which is most remote from, or is farthest left behind by the sun; and of consequence the higher column of air must tend that way to restore the equilibrium; which motion, at this side the equator, must be to the north-east; and as the vapour, &c, fall again to the earth, the motion must be more to the east. Hence our south-west and westerly winds, which blow a considerable part of the year.

But as this system is too regular to account for the phenomena of the erratic winds, he considers whence they arise. He had before observed, that tracts of land rising into the atmosphere will stop the regular motion of the vapour, &c. and that the vapour being accumulated by succeeding vapour, the subjacent air must be pressed into new directions Now this cause, added to the daily dilatation of the electrical fire, and the contraction at night, and the coalition of the vapours, to occasion their total descent, will be sufficient to produce a very great variety of winds on this side the tropic.

It now remains to show, how the general phenomena of the weather and barometer arise from this system. First, Why it generally rains in winter, while the wind is south, south-west, and westerly. Secondly, Why north-west winds are generally attended by showers in the beginning, and become more dry, as they are of longer continuance. Thirdly, Why north and north-east winds are generally dry. Fourthly, Why the east wind continues dry and dark for a considerable time together. Fifthly, Why squalls precede heavy and distinct showers; and why a calm ensues for some little time after they are passed. Sixthly, Why storms and high winds seldom happen in a serene sky without clouds. Seventhly, Why the vapours, in warm seasons, coalesce to form those distinct dense clouds, which produce thunder and heavy showers. Eighthly, Why the barometer falls lowest in long continued rains, attended by winds; and why it rises highest in long continued fair weather; and why the intermediate changes happen. Ninthly, Of land-breezes and sea-breezes, and water-spouts.

First, the vapours passing the tropics into colder regions, have their surrounding fire condensed by degrees; which must increase their specific gravity, and lessen their repulsive power: by which means they must both descend and approach each other, till at last they form dense visible clouds; and these clouds are also accumulated by other succeeding vapours, of like specific gravity, till they form clouds, which are often several hundred yards in depth, as is often seen, in passing through them up the sides of very high mountains. In clouds of such depth, he thinks the coalition of their particles to form drops, may arise from their motion, and the order of specific gravity. Hence he thinks the excess of electrical fluid will run off among the other particles; by which means the enlarged particles have their specific gravity increased, and are enabled to descend to a lower region of the air. And the more particles they impinge on, in their descent, the more their specific gravity and velocity will be increased; and the more their velocity is increased, the more particles will they impinge on, till they fall from the clouds in drops; whose size will be according to the depth and density of the cloud they have passed through.

Having remarked on several of the other particulars above enumerated, in a diffuse and uninteresting manner, Mr. E. then adverts to something of land-breezes and sea-breezes, a phenomenon which sometimes happens in fair settled weather, when the wind blows out from the land at night, and in from the sea at day-time. The land-breeze is occasioned by the descent of the clouds, and the particular formation of the land; for if the land rise into a hilly country from the sea, when the clouds and vapours ascend at night, which they often do by the electrical fluid being condensed, they must press the air down the land toward the sea in their fall; as may appear from the smoke of any fire running down the side of a hill, in the evening of a damp day, when the clouds are on the descent. And the sea-breeze is occasioned by the clouds ascending in the day-time, which must impel the incumbent air upwards, and make room for the sea-breeze to flow in; but, beside the mere ascent of clouds, there is an exceedingly greater quantity of vapour raised from the land than from the sea. For the same extent of land has an exceedingly greater surface than the same extent of sea; which may appear from the various forms of vegetables and animals, &c. and the greater the surface, the greater will be the evaporation. Beside, the more irregular these surfaces are, the greater will be the reflection and refraction of the sun's beams, which will increase their power. And it is also necessary that the evaporation should be much greater from vegetable and animal fluids, than from fluids in a quiescent state, to carry on a circulation for the great work of nutrition. Now the ascent of these vapours must beget a circulation of the air inward from the sea; in the same manner as the ascent of vapours from any fire brings in the air below to that fire.

As to water-spouts, he says they are oddly described by the learned, as being great columns of water sucked up from the sea by the clouds. But he says he never saw any such; nor could he find, on inquiry from many honest men, who have sailed almost all the known seas, that they ever met any such; and therefore he does not believe that there are any such. There is indeed an appearance something like their description, which may have given rise to their conjectures; but this is no more than a very heavy shower from a very dense cloud, which is drawn into a conical form, and a very narrow compass at bottom, before it arrives at the sea, which it dashes with great violence in its fall.

Dr. Birch, the secretary, by order of the Society, having desired to know the experiments, by which Mr. E. found all ascending vapours and exhalations to be electrified; answers, at first he only supposed they must be so, according to the reasonings in his letter; but on trial, with a very simple apparatus, he convinced himself that they were so. He extended a fine string of silk, 8 feet horizontally, and from the middle suspended 2 pieces of such down as grows on the turf-bogs, by 2 pieces of fine silk, about 12 inches each in length; and then, by rubbing a piece of sealing-wax on his waistcoat, he electrified the pieces of down; and then brought sundry burning things under them, so as to let the smoke pass in great plenty through and about them, to try whether the electric fluid would run off with the smoke; but he observed that the down was but a little affected by the passage of the smoke, and still remained electrified. He then brought sundry steams from the spout of a boiling tea-kettle, and otherwise, in the same manner, and still found that the down remained electrified. He then breathed on them in great plenty, but found that the down still remained electrified. He then joined the palms of his hands together, with the fingers extended perpendicularly under the down, which still remained electrified; though the subtile effluvia, thrown off by perspiration, passed in great plenty through the down; as may appear by holding one or both the hands in the same manner under any light matter floating in the air, which will be driven upward, with as great velocity as an electrified feather is by any electrified body held under it.

The electricity remaining in the electrified down after these experiments, made it appear that the smoke and steams must be either electrics, or non-electrics electrified. It was easy to suppose them non-electrics, as they arise from non-electric bodies; and the more, because the highest electrics by a discontinuity and comminution of their parts, long before they come to be as minute as the particles of ascending vapour, become non-electrics, or conductors of electricity. For glass, resin, wax, &c. all become non-electric, even in fusion. But to try whether the steams, &c. were non-electrics, he only bedewed the wax and glass with his breath, steams, &c. from his hand to the end of the wax and glass; and then touching the electrified down with the end of the wax or glass, he found that the electrical fire immediately passed from the down into his hand, through the steams, &c. which rested on the wax and glass. Which he thinks sufficiently proves the steams, &c. to be non-electric; and he thinks that it as plainly appears, that they are all electrified while ascending, because the electrical fire in the down does not join with them in their passage through it; which otherwise it would do with them, or any non-electric not electrified.

XXVI. Remarks on a Petrified Echinus of a singular kind. By Ja. Parsons, M. D., F. R. S. p. 155.

This echinus was found on Bunnan's-Land, in the parish of Bovingdon in Hertfordshire, which is a clay, and supposed to have been brought with the chalk, dug out of a pit in the field. The round echinites are for the most part found in chalk-pits, and they are in general, when recent, the most tender in their shells; so that the chalk is the most favourable bed for them to be preserved in long enough to be petrified; whereas in other kinds of matter, these would be mouldered and destroyed before the petrification could commence; and it is very singular, that almost all those in the chalk are filled with flint, or partly chalk and partly flint, and sometimes with crystal. Now, as all flints and agates are nothing else but crystal debased by earth, and as it is in beds of chalk that these as well as multitudes of large stones are found, one would be almost induced to believe, that chalk degenerated into flint; or, in other words, that flint was produced by chalk originally. And Dr. P. says he had many specimens, that seem to prove it; in some of which they seem to show the gradual change from the one to the other, not at all like a sudden apposition of chalk to flint.

Other kinds of echinites, such as the echini cordati, or heart-shaped echinite, the pileati or conic, the galeati or helmet-shaped, with several other kinds, are often formed of other species of stony particles. But the present fossil, being one of the oval kind, with large papillæ, is the echinometra digitata secunda rotunda vel cidaris mauri of Rumphius, which, with the other oval echinites, are very rarely found out of chalk; and it is remarkable, that whether they are filled with chalk, flint, or crystal, their shells break with a selenitical appearance, just as the lapides judaici, and all other species of echinites found in chalk-pits, do.

XXVII. On Toxicodendron. By the Abbé Mazeas, F.R.S. From the French. p. 157.*

The Abbé Sauvages, of the Royal Acad. of Montpellier, communicated a discovery of a plant, the juice of which adheres, without the least acrimony, to a cloth, with more force than any other known preparation. The colour is black,

* The vegetables mentioned in this paper, as well as in the following letter by Mr. Miller, belong to the Linnean genus *rhus*.

and the plant, which produces it, is the toxicodendron carolinianum foliis pinnatis, floribus minimis herbaceis. Abbé M. found also a plant of the same growing in a garden at St. Germain, then only about 2 feet high. This tree is remarkable for its leaves, which are continued like wings the whole length of the twigs. He pulled off one of the leaves, the juice of which produced a brownish colour on his ruffle, but did not change black in less than 2 or 3 hours.

He examined all the plants of the same class. Near this was the toxicodendron triphyllum folio sinuato pubescente, T. 611. Hederæ trifoliæ Canadensi affinis planta peregrina, arbor venenata quorundam, H. R. par. 84. Arbor trifolia venenata Virginiana folio hirsuto, Raii. hist. 1799. This plant was not yet above 3 feet high; its leaves are hairy; their pedicles, ribs, and fibres, are red; a leaf being pulled off, a milky juice issued from the pedicle, which being put on linen, became a finer black than the former, in less than half an hour.

In this botanical garden he saw another species of toxicodendron; this however was only a shrub, and appeared to be at its full growth. It is the toxicodendron triphyllum glabrum, T. 611. Hedera trifolia Canadensis Com. 96, vitis sylvestris trifolia. Park. Theat. 1556. This plant is remarkable for having an infinite number of black points scattered on the surface of its leaves, which seemed to be a juice extravasated through the punctures of insects. A leaf being pulled off, a milky juice flowed out, which, the instant it was exposed to the sun, became the finest and deepest black he had ever seen.

The Abbé thinks that if these two trees of Carolina were of their proper height, they would produce as fine a colour as this last shrub. He put the linen marked with the three black spots into a boil of soap, and it came out without the least diminution of the colour of the spots. When this linen was dried, he threw it into a strong lye of the ashes of green wood; and again it came out without the least alteration of the 3 shades of the spots, produced by the 3 plants. He took a handful of the leaves of the toxicodendron glabrum, to try if it might be of use in dying; and made a very strong decoction of it; and while boiling he dipped linen in it: it was tinged green, but, besides its not being a good green, the whole surface was unequally coloured; for several places took a fine black: whence he concluded, that the resinous juice of the internal parts of the plant was the only part capable of producing the desired effect. He was confirmed in this notion, after having let the decoction settle; it first let fall a black resinous juice in small quantity, like the opium of the shops: then a large quantity of a white sediment like a salt, which was quite tasteless on the tongue. The water appeared greenish above, and blackish towards the bottom of the vessel.

He would have tried some experiments on the roots of this plant; but, as there was only one in the garden, he was afraid of injuring it. Perhaps the fruit or seeds might produce some kind of dye. Probably in making incisions in the bark, a juice might be obtained which might be turned to some use; for the blacks of our painted cloths, which are preparations of iron with nut-galls, after a certain number of washings, are quite spoiled, and only leave a rusty colour behind. But it is not so with the toxicodendron foliis pinnatis, since the Abbé Sauvages assures, that it was 5 years since his linen, marked with the juice of this plant, has retained the black spots, notwithstanding the great number of washings in lye it had gone through.

On the Same. By Mr. Philip Miller, F.R.S. p. 161.

That the above communication of the Abbé Mazeas might not appear in the Transactions of the R. S. as a new discovery, Mr. Miller gives the following brief account of what has been written on this subject.

Dr. Kæmpfer, in his Fasciculus Amænitatum exoticarum, has given a figure and description of this plant, which are so accurate, as to leave no doubt of its being the same plant as the Carolina toxicodendron. His book was printed at Lemgow, in 1712. His title of the plant is arbor vernicifera legitima, folio pinnato juglandis, fructu racemoso ciceris facie. And by the inhabitants of Japan it is called sitz vel sitz dsju, as also urus seu urus no ki. In the same book there is a figure and description of the wild varnish-tree, which he calls, arbor vernicifera spuria sylvestris angustifolia; and the inhabitants, fasi no ki; but the varnish which comes from this tree is of little esteem.

The seeds which were sent to the R. S. some years ago, for those of the true varnish-tree, by the Jesuits at China, prove to be of this wild sort; and the account which those fathers sent of the manner in which the varnish is procured, being so very different from that which is mentioned by Dr. Kæmpfer, that he here transcribes it, as follows. They first slit the bark of the branches of the shrub, in different places, with a knife: from these wounds there flows out a white clammy juice, which soon turns black when exposed to the air: the same juice is contained in the leaves and stalks of the plant. This juice has no other tasteable quality but that of heating without turning sour, but it is dangerous to handle, being of a poisonous nature. When they make these incisions in the branches of the trees, they place wooden vessels under them, to receive the juice as it drops from the wounds; and when these become dry, and will afford no more juice, they make fresh wounds in the stems of the shrubs, near their roots, so that all the juice is drawn out of them. They then cut down the shrubs to the ground, and from their roots new stems arise, which in 3 years is fit to tap again. This native varnish scarcely wants any preparation; but if any dirt should happen to mix with it, the Japonese strain it through a coarse gause, to cleanse it; then put it into wooden vessels, covering it with a little of the oil called toi, and stretching a skin over it to prevent its evaporating. The varnish exhales a poisonous vapour, which occasions great pains in the head, and causes the lips of those who handle it to swell: on which account the artificers, when they use it, are obliged to tie a handkerchief over their nose and mouth, to prevent these effects.

The shrub is chiefly cultivated in the provinces of Tsi, Kocko, and Figo: and the best varnish in the world, he says, is produced about the city Jassino: but there are many other sorts of varnish, which are collected in Siam, Corsama, and other provinces, which are much inferior in their quality to this, and are produced by different plants: but one of the best among those, he says, is produced from the Anacardium, or Cashew-nut-tree. This is procured by perforating the bodies of the trees, and placing a hollow tube into the hole, under which is put a wooden vessel, to receive the liquor, as it flows through the tube; and when they have obtained as much of the juice as will flow out, they stop the holes made in the trees. This juice is white when it proceeds from the wounds, but changes black when exposed to the air. This varnish is used, without any mixture, for staining black; but the Chinese mix with it native cinnabar, or a red kind of earth, to make a different colour.

The plant, which the Abbé de Sauvages mentions, is also figured and described by Dr. Dillenius, in the Hortus Elthamensis, p. 390, by the title of Toxicodendron foliis alatis, fructu rhomboide, where he also quotes the description from Dr. Kæmpfer, with the account as above mentioned; and he has added all the synonyms from the different authors, who have mentioned the plant, and makes no doubt of its being the same with that of Japan, which, he says, should not seem strange, that a varnish-tree should be found in America, near the same latitude with Japan; since the Genseng, the Bignonia, commonly called Catalpa, with many other plants, are found to be natives of both these countries. And he questions, if the tea-tree might not be discovered in America, if persons of skill were there to search for it. And he is surprised, that the inhabitants of the English colonies in America have not attempted to procure the varnish, by which a considerable profit may arise to them, as the plant grows naturally in so great plenty there.

Mr. Catesby, in his Natural History of Carolina, vol. i. p. 40, has given a very good figure and description of this plant: he calls it toxicodendron foliis alatis, fructu purpureo pyriformi sparso. And he says the inhabitants of Carolina and the Bahama islands call it, poison-tree, and poison-ash, as the other 2 sorts of toxicodendron are called poison-oak in Virginia and New England. Mr. Catesby takes notice, that from the trunk of these trees is distilled a liquid, black as ink, which the inhabitants say is poison; but does not mention its being used there. There are two accounts of the poisonous quality of this tree, printed in the Phil. Trans of the R. S. N° 367. The first was sent by the Hon. Paul Dudley, F.R.S. from New England, and the other was communicated by Dr. Wm. Sherrard, F.R.S. By both these accounts it is very plain, that this species of toxicodendron grows naturally in Virginia and New England, in as great plenty as Carolina, where all the species are the most common under-wood, in the lands which have not been cleared. He adds, that as these shrubs are so very common in our northern colonies, and the anacardium, or cashew nut-tree, is also common in our southern colonies of America; it were to be wished that the inhabitants of both would make some experiments to collect this varnish, which may not only produce much profit to themselves, but also become a national advantage.

XXVIII. On the Method of Constructing a Table for the Probabilities of Life at London. By the Rev. William Brakenridge, D.D., F.R.S. p. 167.

The great Dr. Halley, who had a singular faculty of applying his mathematical knowledge to the purposes of life, was the first who particularly attended to this subject. In the year 1692, from the bills of mortality at Breslau, he reduced it into a sort of science; and gave a table of the probabilities of life, that hitherto has been justly esteemed the most exact of any thing of the kind; from which he and others have deduced many propositions, that are highly useful. But a doubt having arisen, whether that could properly, or with any accuracy, be used by us at London, as we are in a different country, and perhaps in a different way of life, Dr. B. has been at some pains to inquire into this, and satisfy himself about the objections. And he imagines that he can now show how that table may be altered, to suit our case with sufficient exactness.

In the London bills of mortality, for the last 30 years, there is always added an account yearly of the number of burials under each age, at the distance of 10 years, and of children more particularly under 2 years, between 2 and 5, and between 5 and 10; which numbers are curious and useful. And though there may sometimes be some inaccuracies and omissions, these numbers are as exactly given as in our case can be expected: and what may be objected, is not so much to the incorrectness of them, as to what arises from our circumstances, that will not allow them to be proper to show the probabilities of life in all its periods.

But if we compare the numbers of the dead, in the several periods at Breslau, with those at London, we shall plainly see that the former show the decrements of life in a natural and regular way, and free from the difficulties and objections found in those of London. In the infant state, under 2 years of age, there is a 5th lost by death; but afterwards, as they gather strength, the deaths are diminished till between 10 and 20; and from that age the mortality gradually increases, till after the age of 40; when the number of the dead continues nearly the same, though the probability of life continually decreases till the age of 80; and then at length, the living being almost all exhausted, the burials are greatly diminished. All which seems to be agreeable to the course of nature; but contrary to what we see in the London bills, especially after 50 years of age. However, they both agree in this, that the most healthy age is between 10 and 20, and the infant state under 5 years of age the most uncertain for life.

Indeed it must be acknowledged, that in computing the Breslau table, Dr. Halley had great advantages, which have made it so perfect. He had the number of births given, besides the burials at the different ages, in an inland town, where there is no great concourse of strangers. But with us at London, the number of births is not known; because of the number of Dissenters of various denominations, both foreigners and natives, of whose baptisms there is no account taken; which makes our bills at present very imperfect. For none are put into our bills but those who are baptized according to the form of our established church. And therefore there are some thousands omitted, and yet many, perhaps the one-half of them, who are not baptized with us, bury with us; which greatly perplexes our bills. And under this disadvantage it appears very difficult to make an accurate computation of the decrements of life through the different ages; though this defect he imagines he shall be able nearly to supply.

There have indeed been some ingenious men who have thought, that our London bills are correct enough to form a table from them, which may better agree with our circumstances than that which Dr. Halley has given us. And Mr. Smart was the first who endeavoured to do something in this way, from our bills only, about 18 years ago. But, in the table made by him, he seems to have been greatly mistaken; for he has made no allowance for the accession of strangers, but considered the numbers of the dead, in all the periods of life, as all come from those born here; whereas it is evident that the strangers, above 20 years of age, are at least equal to them. And this has brought this paradox into his table, that young people between 12 and 18, at London, are much more healthy than at Breslau, or in any country place in England. For according to him, in the 13th year, 2 die only out of 479; but at Breslau there die 6 out of 634; that is, there is double the number die more at Breslau than at London; which appears impossible. But between 30 and 40, he makes them much more unhealthy than they are; for at 40 he supposes one to die in 29; whereas there does not die above one in 30, all ages taken together, with infants included. Another ingenious gentleman, having seen this inconsistency, has endeavoured to correct it, by supposing that the number of strangers that come to settle in town, after 25 years of age, is inconsiderable; and that above that age, the numbers of burials may be considered, as arising from the natural degrees of mortality; and then by proportion, increasing the numbers of the living corresponding to all ages below 25; so that the table, altered in this manner, is the same with Mr. Smart's above that age. And it must be confessed, that this correction is very proper, and worthy of its author. But still the table is greatly defective, as he has made no allowances for the recess of great numbers who, after they have been a number of years in town, leave it, if they survive; and of many others who, after the age of 50, retire from business into the country. And which is so very obvious, that our burials are fewer than by proportion they ought to be after 50 years of age, as mentioned above, and by consequence the people appear more healthy after that age; so that after 70 they seem more healthy than at Breslau. For at 75 there appears from this table to die 4 out of 45, whereas at Breslau there die 10 out of 88.

And that a great number retire from the town, after the age of 50, or before, is further evident, if we suppose, even according to this corrected table, that one in 25 die at the age of 50. For then the number of people alive, between 40 and 50, will be greater than 2604 multiplied by 25, or 65100: which ought to be exhausted by all the deaths in the subsequent period. But all the deaths which ought to arise from that number of living, in the following years to 90, according to the bills in the 3d column, is 5315 multiplied by 10, or 53150; which is less than the people that were alive between 40 and 50, by 11950, or more. And therefore above 11000, of those between 40 and 50, must have retired from town.

But now, as our bills are defective, it is next to be considered, what we at London are to do at present, and what method of computation we are to follow? And Dr. B. imagines it is very obvious what may be done. Our bills may be used so far as 14 or 20 years; for there is certainly no increase of our people till the age of 14; because few young people come to town till they are fit to be apprentices or servants. And between 14 and 20, though many come at that time, yet there is an emigration of a great number from hence to sea, to other countries, the universities, and country academies, that nearly balances the accession of strangers. And then, after 20 years of age, the Breslau bills will be sufficiently correct, to show the probability of life within and about the city. And if so, a table may be made from both bills, that will agree with our case here with sufficient exactness. For he cannot find that there is any difference in the bills, above the age of 20, that can be depended on.

And from all considerations, he thinks it may be allowed, till it is otherwise demonstrated, by bills formed in a different manner from what they are at present, that the probabilities of life are much the same at London as at Breslau, at the age of 20 or after 14. And if we take this for granted, we shall from thence be able to form a useful table, for those within our bills, by accommodating and joining the bills of both places together. And we may also nearly determine the number of infants born here, which hitherto has not been considered.

Now, from the births, which are found = 19561, and the numbers of the dead in the different periods known by our bills, it will be easy to form a table of the decrements of life; because the dead in the intermediate years may be found by proportion from the Breslau table. And accordingly Dr. B. computed the following, which is constructed from the London and Breslau bills together; which he thinks is a surer method of computing for us at London, than from either of them alone. The first part to the 21st year, is done from our bills, and the other part from the Breslau; but it is formed in such a manner, that it goes on as if from the bills of one place only. For, after the age of 20, it is continued by proportion, by making the dead at London in the decennial periods, to have the same ratio to each other, as the dead at Breslau. It supposes 1000 persons born in one year, and shows the annual decrease of them by death till 87 years of age, which may be considered as the utmost period of life. The intermediate numbers, marked d, show the dead in each year. The use of this table is well known to all who can compute the value of annuities for lives.

Age.	Pers.		Age.	Pers.		Age.	Pers.		Age.	Pers.		Age.	Pers.		Age.	Pers.		Age.	Pers.		Age.	Pers.	
1	1000	323 d	12	403	4 d	23	361	4 d	34	311	5 d	45	248	6 d	56	176	6 d	67	99	7 d	78	28	6 d
2	677	127 d	13	399	4 d	24	357	4 d	35	306	6 d	46	242	6 d	57	170	6 d	68	92	6 d	79	22	5 d
3	550	45 d	14	395	4 d	25	353	4 d	36	300	6 d	47	236	6 d	58	164	6 d	69	86	6 d	80	17	4 d
4	505	32 d	15	391	4 d	26	349	4 d	37	294	5 d	48	230	7 d	59	158	6 d	70	80	7 d	81	13	4 d
5	473	26 d	16	387	3 d	27	345	4 d	38	289	6 d	49	223	7 d	60	142	6 d	71	73	7 d	82	9	3 d
6	447	13 d	17	384	4 d	28	341	5 d	39	283	5 d	50	216	7 d	61	136	6 d	72	66	7 d	83	6	2 d
7	434	9 d	18	380	4 d	29	336	5 d	40	278	6 d	51	209	7 d	62	130	7 d	73	59	7 d	84	4	1 d
8	425	7 d	19	376	3 d	30	331	5 d	41	272	6 d	52	202	7 d	63	123	6 d	74	52	6 d	85	3	1 d
9	419	6 d	20	373	4 d	31	326	5 d	42	266	6 d	53	195	7 d	64	117	6 d	75	46	6 d	86	2	1 d
10	413	6 d	21	369	4 d	32	321	5 d	43	260	6 d	54	188	6 d	65	111	6 d	76	40	6 d	87	1	
11	407	4 d	22	365	4 d	33	316	5 d	44	254	6 d	55	182	6 d	66	105	6 d	77	34	6 d			

XXIX. Of a Sheep, showed alive to the Royal Society, in November 1754, having a Monstrous Horn growing from his Throat; the stuffed Skin of which, with the Horn in situ, was placed in the Museum of the Society. By James Parsons, M.D., F. R. S. p. 183.

This animal was bred in Devonshire, with the preternatural horn appearing at its birth. The novelty of the thing made the farmer spare the life of the lamb, and bring it up till it grew to the size of a well-grown sheep, pretty large of its kind, and about 3 or 4 years old. When it was brought before the Society, the owner said the horn weighed 26 lb.; and the creature swung it about, and raised it up with amazing strength. When he was fed, he moved forwards, letting the horn drag between his fore-legs, by which he was enabled to lay his nose to the ground; for the skin, by which it hung, was flexible, and though reduced to a neck, with respect to the circumference of the horn, yet it was hollow as well as flexible, leaving an open passage from the flesh of the neck to the cavity of the horn, and its contents. Sometimes the horn would come into such a position, as to twist the skin, which gave the sheep great uneasiness; but from experience he knew how to relieve himself, and from custom became ready at that, as well as bringing it between his legs to favour his feeding. It was in length along the convex or anterior surface, 2 feet 7 inches; and on the concave side 2 feet 1 inch; its greatest circumference 2 feet 2 inches, middle circumference 1 foot 6 inches; and near the apex 1 foot; and its weight is now 15 lb. though emptied of its contents.

It was said that on opening him there was found, in the top of the horn next the throat, which was hollow half-way down, a skull of a contracted round form, with blood-vessels running on it, and a bag filled with grumous blood, among which was a substance like a sheep's liver and lungs; and a perfect sound kidney, like that of a fresh loin of mutton. And this was attested by the names of 3 house-keepers of credit, who were present when the animal was opened, and who, if required, were ready to make oath of it.

XXX. A Dissertation on the Cancer of the Eye-lids, Nose, Great Angle of the Eye, and its neighbouring Parts, commonly called the Noli-me-tangere, deemed hitherto Incurable by both Ancients and Moderns, but now shown to be as curable as other Distempers. Addressed to the R. S. of London by Mons. Daviel, Surgeon and Oculist to the King of France, &c. Translated from the French by James Parsons, M.D., F.R.S. p. 186.

The examinations M. D. had made in these kinds of tumors had informed him, that cancers of the lids, nose, and adjacent parts, have all their seat in the periosteum, and perichondrium; and that a cure cannot be expected without taking them entirely off: for the vessels that go from the cancerous tumor are so strongly connected with the periosteum and perichondrium, that they seem but one body, which becomes at length so greatly swelled, that the very bone is often affected. When a wen or wart (which is often the beginning of a cancer) begins to appear, and it is attempted to be pulled off, it becomes irritated, and spreads so that the edges are reversed, and become callous and livid, accompanied with a pain, and all other symptoms which characterize the cancer. These kinds of wens, warts, and tubercles, which are situated in the great angle of the eye, or on the lids, or the nose, often shoot out their roots on the cartilages, that is, on the very membranes which cover them, and the roots sink in sometimes to the substance of the cartilage itself, which they swell and tear in the end.

The more that cancers are touched with caustics, the more they are irritated; therefore there is but one method, but it is a sure one, of curing them, and hindering their progress; which is, to take them off with a cutting instrument, destroying the periosteum and perichondrium, or even the lids, if the cancer has penetrated them in their substance, with their cartilages: which the following observations will prove:

Observ. 1.—On a cancerous upper-lid. August 11, 1736, M. Daviel was called to Madame de la Fague, a nun, at Bourdeaux, 45 years old; for a tumor on the upper lid of the right eye, which she had for 20 years: it began by a small wen, and increased by degrees, so as very much to incommode her. She applied to a surgeon, who began by applying some drops of a liquid caustic, which enraged the tumor still more; which he appeased again by anodyne medicines; and then the tumor remained a long time without any sensible increase; though she felt a continual sharp pain in it. But, as even the least disorders are impatiently borne, she was willing to be relieved, and consulted another surgeon, who took off the tumor with a cutting instrument, and who, seeing that the ulcer, which was the result of the operation, did not heal, but on the contrary made great progress in its erosion, and became callous, he touched it with lapis infernalis; and sometimes with a liquid caustic: which so much the more increased the evil, and made her resolve to suffer no more applications, because all that had been tried made her worse and worse. She was now a long time in this state, when M. D. was called to consult with several other practitioners, who, having examined the case, agreed with him that there was no other method to be taken but the operation, not only to save the eye, but to prevent an incurable cancer, which threatened her life. Therefore he proposed the total extirpation of the lid: which proposal being approved of by all, as the only method of saving the eye, the operation was performed in the following manner:

He passed a crooked needle, with a waxed thread, under the lid, by which he suspended and drew up the lid and tumor, which he cut off with the crooked scissars, as much as he could under the orbit, separating the whole to the division of the lids; a small hæmorrhage ensued, but which was soon stopped with dry lint, and a dry compress and bandage.

She remained 24 hours without being dressed; was bled twice in the arm, after the operation: he then dressed her up with light dossils, armed with the linimentum Arcæi, and she had not the least accident from the day of the operation to the 25th of the same month, when she was perfectly cured, without any deformity in her eye: and though the lid was cut away very high, the eye remained very neat and well, performing its several functions properly when he left Bourdeaux; and the 13th of August 1742, having had an opportunity of taking a journey to that town, he saw the patient again, whom he found extremely well, seeing perfectly with that eye: but what he found very singular was, that the skin of the lid descended pretty low, to the cornea, which it almost covered; so that the whole globe was in a manner hid. He only observed that this resembled a lid without hairs.

Observ. 2.—On another cancerous tumor in the great angle of the eye. July 2, 1736, Margaret Combaucaut, of Carcastone in Languedoc, 60 years old, had a cancerous tumour, for 16 years, in the great angle of the right eye: it began by a little wart, which itched violently, and made her scratch it very often, which so irritated the tumor, that in a little time it became as large as a dried fig flatted, with its edges turned outward and callous. It reached from the commissure of the lower lid, an inch and half below it, even to the right ala of the nose, which proved extremely troublesome to her. He found, after a strict examination, that it adhered to the bone. She said she tried all the remedies that she imagined would do her any good; but that, far from relieving her, they rather made her worse, and her disease became the more insupportable; and that she had taken a resolution to undergo any thing to be freed from a disorder which had afflicted her for 16 years.

Having consulted Mr. Fabre, an able physician of that place, they were both of opinion, that she could not be cured without an operation, which he accordingly proceeded to as follows: he took off the tumor entirely to the periosteum, but did not lay the bone bare; for he thought it sufficient for a complete cure to take away all the callosities; but he was mistaken; for instead of the prospect of a succeeding cure, he was unhappy enough to see the swelling increase, and the wound seem larger than before. He used in vain all the remedies commonly thought of in such cases; he scarified the edges of the ulcer, to bring it to suppuration; but it became more hard and callous than before the operation, and much more painful. He therefore resolved to cut away all that remained of the tumor, with the periosteum, which appeared very much swelled. This second operation had so much success, that the swelling, and every other bad symptom, disappeared almost suddenly; and in 3 days the wound looked red and very well, without any pain, and the cicatrix was perfectly formed on the 15th day from the operation, without any sensible exfoliation of the bone, or the least deformity or staring of the eye. She had remained very well ever after; for he saw her the 10th of August 1741, at Carcastone, in perfect health; and the cicatrix of the part very even. He observes, that he laid the entire bone bare, wherever the tumour touched, even down to the ala of the nose of that side.

Observ. the 3d was on a cancerous tumor of the same nature, and in the same situation, and the treatment just the same; it was as large as a filbert, and the officer was afflicted with it 20 years. It differed from the former only in this, that the year before the officer came to Marseilles, to put himself under Mons. Daviel's cure, the tumor broke, and discharged a very fetid acrimonious matter, which, running into the eye, brought on a troublesome ophthalmia, and the edges were livid, and had a very terrible aspect. As to his operation, it consisted, as before, of a total extirpation of the cancer, periosteum and all, to the bare bone. He dressed the bone with dry lint only, and his digestive was a mixture of the linimentum Arcæi, with the unguentum styracis: and in about 19 days he was so perfectly cured, that when he returned to his friends, several of them asked him, on which eye the operation had been made?

Observ. 4, differed in nothing from the former.

Observ. 5, On a cancerous tumour on the nose, which reached from the root of the nose down to the middle of the cartilage. He treated it in the same manner, taking off the whole with the periosteum; and, as it was partly upon the cartilage, he also cut away the perichondrium, laying that, as well as the bone, bare: and the cure was completed, without leaving any deformity behind, in 18 days.

Observ. 6. Of a cancerous tumor on the great angle of the right eye of a woman at Marseilles, of 70 years old. This he treated exactly in the same manner, and she was cured in 20 days.

The 7th observation mentioned another cancerous tumor on the nose, and its cartilage, of a gentleman, which was circumstantially the same with the former: it was cured in 5 days. After this case he makes this conclusion: that from all that has been already said, it is plain, that the seat of the cancers of the eye lids, nose, and other neighbouring parts, is absolutely in the periosteum and perichondrium, as well as the fat; and that there can be no hopes of a cure without taking off these membranes, with the fat, and even any parts of the very cartilages that may be contaminated: but that in this manner they are as curable as cancers on other parts of the body, notwithstanding what all oculists have said to the contrary.

The 8th observation was on a cancer on the lower eye-lid of a woman, cured in the same manner.

The 9th observation treats on a cancer,as large as a large filbert, in the angle, and on the lower lid of the eye of a gentleman; which began by a small tubercle in the angle, and was pulled off, and grew again several times. Mons. Daviel was consulted, in the presence of another surgeon, Mons. Maillot, and declared for taking the tumor and eye-lid entirely off; making this prognostic, that if any part was left behind, the eye would be deformed and staring; but the other surgeon thought, that half the lid with the tumor would be sufficient for the cure: Mons. Daviel therefore only cut away half the lid with the tumor; with which he also took off a large quantity of hard white fat, and dressed up the part as usual; but in the progress the lid was turned outward, and then they resolved on the total extirpation of the lid; which, being obliged to depart from thence, he left to Mons. Maillot, who performed it with such success, that his cure was complete in 15 days, without the least deformity whatever.

The 10th Observation is a case of the same nature with the former, with this difference, that when he had taken off the tumor and under lid in the same manner as usual; the patient continued getting well till the 9th day from the operation; when Mons. Daviel perceived a small fungus in the middle of the tumor, which he touched with the lapis infernalis, which produced very ill effects: the eye grew painful, the conjunctive swelled very much, the wound, which was half healed up, opened afresh, and became ragged. This made him set about cutting away all the bad flesh he could perceive, with the inequalities of the conjunctive, which was much swelled: he scarified the cornea, and in the inner surface of the upper lid, which was also greatly tumefied, and even opened it on the upper surface. Thus, after having emptied the vessels well, he fomented the whole with a decoction of marshmallows, mullein, violet-leaves, camomile-flowers, melilot, leaves and flowers of rosemary, thyme, lavender, rue, and marjoram, of each half a handful, in a sufficient quantity of water; to a quart of which he put a bit of camphor the size of a nut. The frequent application of this that day produced so good an effect, that all her pain ceased: he also bled her in the arm and foot, ordering emollient clysters. She was purged some days after, with manna and cassia, which did very well; and she was perfectly cured, without the least deformity, and could see better than before the operation.

XXXI. Of Four Roman Inscriptions, cut on Three Large Stones. By John Ward, LL.D., V.P.R.S. p. 196.

The stones were found in a field near a mile from Wroxeter, formerly a Roman station called Uriconium,* in the months of September and October 1752. The first of them was discovered by a plough striking against it; and by spitting the ground the other two were discovered, not far from the first, in the like situation. The first and last lay separate from their bases, which being taken up,

* In the year 1701, a Roman sudatory was discovered at this place, a draught of which, with some account of it, was published in the Phil. Trans. N° 306, which seems to have escaped the observation of Horsley, Brit. Rom. p. 419.—Orig.

several broken pieces of urns, and dust of a greyish colour, were found with them, which seemed to have the appearance of ashes.

N° 1, is by the scale 6 feet 8 inches high, and about 2 feet 3 inches wide above the base. It has a pediment top, with a pine apple rising from the middle of the cornice, on each side of which is a lion, and in the area of the pediment a kind of rose. The inscription, which is cut in the plane of the stone, may be thus read; Caius Mannius, Caii filius, Pollia tribu, Secundus Pollentinus, miles legionis vicesimae, annorum LII, stipendiorum XXXI, beneficiarius legati principalis, hic situs est.

N° 2 contains two inscriptions, and is in height 2 feet $7\frac{1}{3}$ inches, by 2 feet $4\frac{2}{3}$ inches in breadth. It is not flat, as the former, but gently convex crosswise, the lower part being divided into 3 pannels; on the first two of which are the inscriptions, but the other seems never to have had any on it. The upper part is ornamented with a pediment, in the area of which are the remains of a face with curled locks, and 2 snakes under it; and on the cornice 2 figures like dolphins. The first inscription may be read thus: Diis Manibus. Placida annorum LV, curam agente conjuge annorum XXX. And the other in this manner: Diis Manibus. Deuccus annorum XV, curam agente patre.

N° 3 is 6 feet 11 inches high, and about 2 feet broad above the base. It has also a pediment at the top, the area of which is filled with a large flower. The inscription, it exhibits, may be read in the following manner; Marcus Petronius, Lucii filius, Menenia tribu, vixit annos XXXVIII, miles legionis XIIII geminæ, militavit annos XVIII, signifer fuit, hic sepultus est.

XXXII. On an American Wasp's West, shown to the Royal Society. By Mr. Israel Mauduit, F. R. S.* p. 205.

M. de Reaumur distinguishes wasps into three classes, from the different situations in which they place their nests; some choosing unfrequented parts of houses, some little cavities in the earth, and others the branches of trees for that purpose. The first of these is the largest sort, or hornet; the second is the common sort here in England; and the last is more frequent in America.

The nest, then shown to the Society, was sent from Maryland; where they are found on the lower kinds of trees, in the thickest parts of the woods. This was built on a dogwood-tree, or the cornus mas Virginiana; and hung quite detached from the rest of the tree by an extreme branch, of little more than an inch circumference: which, with its smaller divisions running through the substance of the nest, answered the purpose of pillars, to unite and support the several floors

* This wasp's nest, which is not described with sufficient accuracy, is probably that of the vespa nidulans of Fabricius.

of the fabric. The figure was a conoid, or an acuminated oval; its longer diameter 20 inches, the shorter near the base 12. It was perforated on 2 opposite sides, for the wasps to enter and go out at. The shell was composed of paper; the sheets of which at its upper end were larger and more distinct. They were of an ash-colour, of different shades, and streaked or marbled, and, being lightly laid on each other, formed a wall of from $1\frac{1}{2}$ to 4 inches thickness in the several parts of it. The lax hollow manner, in which they were joined to each other, rendered them a more effectual security from rain; as they attracted water in common with all other substances, made of the same materials; and would have been more easily soaked through, if they had been closer compressed together. For the same reason the apex of the cone was of the greatest thickness, and the base of a stiffer and more cellulose texture. This substance appeared to be a true paper; but, by the exact economy of nature, wrought to that degree of perfection only, which was necessary to serve the single purpose it was intended for. Being examined by the microscope, it appeared to be of a coarser grain, a shorter staple, and of a much looser texture; and was a rare, though not a singular instance, of a natural production falling far short of the artificial one of the same kind. The inside structure of these nests, is well described by M. de Reaumur.

XXXIII. Abstract of a Letter from the Magistrates of the City of Mascali, in Sicily, concerning a late Eruption of Mount Etna. From the Italian. p. 209.

On Sunday, March 9, 1755, about noon, mount Etna began to emit a great quantity of flame and smoke, with a most horrible noise. At 4 o'clock the air became totally dark, and covered with black clouds; and at 6 a shower of stones, each of which weighed about 3 oz. began to fall, not only all over the city of Mascali, and its territory, but all over the neighbourhood. This shower continued till a quarter after 7, that by the darkness of the air, the fall of stones, and the horrible eructations of the mountain, the day of judgment seemed to some to be at hand. After the stones had ceased falling, there succeeded a shower of black sand, which continued all the remainder of the night. The next morning, at 8 o'clock there sprung from the bottom of the mountain, as it were, a river of hot water, which in the space of half a quarter of an hour, not only overflowed to a considerable distance the rugged land, near the foot of the hill, but, on the waters suddenly going off, levelled all the roughness and inequalities of the surface, and made the whole a large plain of sand. The stones and sand, which remain where-ever the inundation of the water reached, differ in nothing from the stones and the sand of the sea, and have even the same saltness. After the water had ceased flowing, there sprung from the same opening a small stream of fire, which lasted for 24 hours. On Tuesday, about a mile below this opening, there arose another stream of fire, in breadth about

400 feet, like a river, which overflowed the adjoining fields, and actually continues with the same course, having extended itself about 2 miles, and seeming to threaten the neighbourhood.

XXXIV. Of the Charr Fish in North Wales. By the Rev. Mr. Farrington, of Dinas, near Caernarvon. p. 210.

This species they call torgoch, or red belly. This redness in the female, paler or deeper according to the season, resembles that of the fins of a roach, a fish very common in many rivers of England, though we have none of them in this country. The male is not adorned with that beautiful hue, yet he is finely shaded, and marbled on the back and sides with black streaks, on a kind of pellucid light sky-coloured ground. The shape is like a trout, but much more elegant and delicate. Three lakes or large pools, at the foot of Snowden, afford being and subsistence to this remarkable finny race. There is a communication between them. About a fortnight in December the charrs make their appearance; never wandering far from the verge of these lakes, or the mouths of the rivers issuing from them; but traverse from one end to the other, and from shore to shore indifferently, or perchance as the wind sits, in great bodies; so that it is a common thing to take in one net, 20 or 30 dozen in a night, at this place; though not above 10 or a dozen fish in all at any other. Thus in winter frosts and rigours, they sport and play near the margins of the flood, and probably deposit their spawn; but in the summer heats they keep to the deep and centre of the water, abounding in mud and large stones, as the shoaler parts do with gravel. After Christmas they are seen no more till the following year.

XXXV. A Method proposed to restore the Hearing, when injured by an Obstruction of the Tuba Eustachiana. By Mr. J. Wathen, Surgeon. p. 213.

Whatever obstructs that passage leading from the ear into the nose, called tuba eustachiana, so as to hinder the ingress of the air through it into the cavity of the tympanum, is universally deemed destructive to the sense of hearing. Hippocrates observed, that in a quinsy of the fauces, the patient became deaf, by its compressing and closing up this tube.* Many practical writers assert the same to have happened from adjacent ulcers, &c.;† and Mr. W. had known a swelled tonsil occasion deafness. This canal opens into the lateral and anterior

* Coac. 11. n. 35.

† Haller in Boerhav. de auditu, p. 380, and 416. Tulpius l. n. 35, a tumore palati. Valsalva, cap. v, p. 90, a polypo. et ulcere (viz. a certain yeoman had an ulcer above the uvula, on the left side, which communicated with, and corroded part of, the orifice of the left tuba eustachiana; which, when he stopped with a tent dipped in medicine, he immediately lost his hearing in that ear, but recovered it as soon as the tent was taken out).—Orig.

part of the cavity of the tympanum; is so shaped that it first decreases, as it descends towards the posterior parts of the nose, becoming very narrow; then suddenly diverging, is much enlarged, opening into the posterior part of the nose by an elliptic orifice, a little prominent, turning inwards and forward, placed laterally, and just above the velum pendulum palati. This canal then is composed of two distinct cones, the extremities of which unite together, but their bases diverge differently; it is likewise lined with a porous membrane, full of cryptæ and mucous cells, continued from and like to the membrane of the nares.*

When therefore we consider the structure of the eustachian tube, and its free communication with the atmosphere, we may reasonably suppose it subject to inflammation of its membrane, and concretion of its mucus, from cold, &c. like the external meatus; and though its mucus is of a very different nature, it is nevertheless liable to inspissate by heat, when its thinner parts are exhaled.† And from the form of this passage we may easily conceive, that an obstruction, pretty far advanced, is not to be removed without difficulty, and that in proportion, as it is more or less complete, the hearing will be more or less injured. Why then may not this be suspected as sometimes the cause of deafness? perhaps it is not unfrequently so; e. g. When a patient is somewhat deaf from cold, and the outer ear has been examined, and found clear of hardened wax, &c. it is yet not uncommon to find himself suddenly relieved by a great noise in his ear. This is probably owing to the breaking away of the congealed mucus, and the instantaneous rushing of the air into the tympanum; so that when this disorder is but slight and recent, nature seems frequently to relieve herself; but when more confirmed, her efforts are ineffectual for its removal. These considerations inclined him strongly to think the hearing might suffer from that cause, and he was much confirmed in it by the following very remarkable case.

Richard Evans, aged 35, was very deaf in both his ears, yet no visible disorder in the external meatus. It arose from cold, and had subsisted several years, during which time no art or means could procure him the least relief. In August, 1755, he died of the small-pox, at the hospital in Cold-bath-fields. Mr. W. took that opportunity to examine the eustachian tube of each ear, and found them both stuffed quite full of congealed mucus. This was the only visible cause of his deafness, the other parts appearing in their natural state. As

* Haller in Boerh. de Auditu, p. 378. Not. e Physiologia. Haller. de Auditu, § 485. Valsalva, cap. 2, p. 32. idem fig. xiv.—Orig.

† Morganni and others tell us, that they constantly find the cavity of the tympanum in infants always much clogged with mucus; and Mr. Douglas has often observed the same in adults, and is of opinion that it is concomitant with an obstructed tube in general, and that the injection is equally as effectual as if the tube only was obstructed.—Orig.

all these concurring circumstances strengthened him in his opinion, they likewise incited him to make trial of an operation that was some time before proposed to the Academy of Sciences, by Mons. Guyot; but the author having never practised it, he wanted the recommendation of facts to support and enforce it; it was therefore rejected by them as impracticable.*

Mr. W. first introduced his probe, a little bent at the end, through the nose into the tubes of several dead subjects; and, having thereby acquired a facility, he did the same on a person that was very deaf, and on whom all other means had proved ineffectual; no sooner had he withdrawn the probe, than he said, he could hear much better. This success excited his further endeavours, so that he had pipes of different sizes adapted to a syringe, and he had since injected the meatus internus in the following manner, with success. The pipe is made of silver, about the size and length of a common probe, and a little bent at the end: this being fixed to an ivory syringe, full of liquor (viz. a little mel rosarum in warm water), is introduced between the ala and septum of the nose, with its convexity towards the upper part of the aperture of the nares, and thus continued backwards, and a little downwards, till it comes near the elliptic orifice; then its convexity is turned toward the septum, by which the inflected extremity enters the tuba eustachiana with ease; the liquor is then impelled through it into the tube, by which the sordes, if any, being diluted, is washed out, and regurgitates through the nose, or mouth, or both, with the injection; and, if the quantity be large, may be seen.

[Then follows an account of 6 different cases, in which the operation was successfully performed.]

After the detail of these cases Mr. W. remarks, that he had endeavoured to ascertain the symptoms that indicate an obstructed tube, but had not been able to do it with any degree of certainty; nor could he see the great utility of it, could it be done; for the only disorders of the ear, that at present admit of surgical helps, are those of the external meatus, ulcerated and swelled tonsils, &c. all of which are generally visible; and when they are not the cause of deafness, little or nothing is ever attempted, the patient being left to shift for himself. But now another probable chance at least is given to the unhappy sufferer, and being the only one (e. g. the others either improper, or tried before without success),

* Hist. de l'Acad. 1724, p. 53. Besides, Mons. Guyot proposed doing it by the mouth, which is quite impossible, as evidently appears to any one that will give himself the trouble to examine into it. Convinced of this, Mons. Petit (who has lately published a new edition of Palfin's anatomy) proposed, and that learned and skilful anatomist Mr. John Douglas first demonstrated the possibility of, passing the probe, &c. through the nose into the eustachian tube; and this he has constantly shown to those who have attended his public lectures; and to him Mr. W. freely acknowledged himself indebted for the hint, by which he was incited to make trial on the living, of an operation of so much importance to mankind.—Orig.

may be made use of without delay, or attendance to accompanying symptoms, at least till they render themselves more conspicuous and certain than he had hitherto been able to find them; and as the operation is not at all dangerous, it neither has, nor will, he believed be thought painful by those who desire to recover their hearing.

XXXVI. A Chemical Essay on the Action of Quicklime on the Volatile Alkaline Salt. By I. A. Schlosser, M. D. of Utrecht. p. 222.

As the true nature of quicklime was unknown at the date (1755*) of this essay, the theory which Dr. S. has offered concerning the different phenomena produced by the action of burnt lime on the volatile alkali, is wholly erroneous. It is therefore deemed unnecessary to be more particular in the notice of this paper.

XXXVII. An Account of a very remarkable Case of a Boy, who, notwithstanding that a considerable Part of his Intestines was forced out by the Fall of a Cart upon him, and afterwards cut off, recovered, and continued well. By Mr. John Needham. p. 238.

On the 3d of January 1755, Mr. N. was called to the son of Lancelot Watts (a day-labourer, living at Brunsted) a servant boy to Mr. Pile, a farmer at Westwick, near North-Walsham, Norfolk, aged 13 years. He was overturned in a cart, and thrown flat on his face, with the round, or edge of one side of the cart, bottom upwards, whelmed across his loins, the upper part of the body lying beyond the wheel at right angles. In this helpless condition he continued some time, and was found with a very large portion of the intestines forced out at the anus, with part of the mesentery, and some loose pieces of fat, which Mr. N. took to be part of the omentum, hanging down below the hams, double, like the reins of a bridle, very much distended and inflamed. He had a continual nausea, and violent retchings to vomit, and threw up every thing he took. The pain of the stomach and bowels was exquisite, attended with convulsions; his pulse low and quick; and frequently he fell into cold sweats. After using an emollient and spirituous fomentation, Mr. N. reduced the parts, though to no purpose; the vomiting immediately returned, and forced them out again. Next day the fever increased, the nausea and retchings to vomit continued, the parts appeared livid and black, with all the signs of a mortification. On the 3d day the mortification increasing, he cut off the intestine, with the mesentery, close

* It was about a year after the above date that Dr. Black's experiments on quick-lime, which demonstrated so clearly the difference between mild and caustic calcareous earth, mild and caustic alkalis, &c. and which laid open so vast a field of discovery in gaseous chemistry, were first communicated to the world.

to the anus, being 57 inches in length. He had had no stool from the time of the accident, but soon after the operation there was a very large discharge of blackish and extremely offensive fæces, which continued several days, lessening by degrees. He soon became easy, and the nausea and vomiting abated. Mr. N. gave him tinct. cort. Peruv. simpl. twice a day; and, as he complained at times of griping pains, he took now and then tinct. rhabarb. vinos. and had recovered a good state of health. For some time he had 6 or 7, or more stools in a day; afterwards commonly 3 or 4, all loose, which come soon after eating; and frequently he was obliged to hurry out to ease himself, during his meals.

Mr. N. 3 times tried to discover a passage through the coats of the rectum, with his finger, and he thought he always felt an opening, just above the sphincter, towards the spine; the circumference of which was full, and protuberated, seemingly as large as his finger, the lower edge of which was harder than the rest; the patient complained of pain, when the upper part was pressed.

On the 7th of May the boy walked from Brunsted to North-Walsham, 7 miles, was perfectly well, and walked back again that afternoon.

XXXVIII. Experiments on the Sensibility and Irritability of the several Parts of Animals. By Richard Brocklesby, M. D., F. R. S. p. 240.

After apologizing for the cruelty exercised in these experiments, which Dr. B. made for the purpose of ascertaining the validity of Haller's doctrine, respecting the irritability of animal fibres, Dr. B. proceeds to state, that his first experiment was made by cutting 4 inches of a young lamb's skin, which covered the great tendon of the hinder leg, known to anatomists by the name of the tendo àchillis. This of course caused violent struggles, and other marks of the injury felt; and on touching the extremity of the skin, while united to other parts of the animal, it cried loud, urined, and voided its excrement, when he poured diluted spirit of vitriol on the edges of the skin that were fixed to the contiguous parts; but did not express much pain by irritating the raised skin, at the farthest extremity of its separation, by an infusion of dilnted spirit of vitriol. Nearer however to the fixed parts underneath, the sensation in the raised part of the skin continued much longer.

He then made the butcher cut into the tendon half way, and divide it upwards more than 2 inches, and attentively stood over the animal, to watch his motions, and discover if there was any apparent pain; but while that was doing, he could discern none, nor any marks of sensation in the animal, while he handled and pulled the cut tendon, nor yet any on touching it with dulcified spirit of nitre, and sharp acid spirit of vitriol; and what yet surprised him more, was to find the creature as insensible on the tendon, as if it was a mere piece of glue, when he put a strong muria of sea-salt and nitre all over it; and after a very few minutes he laid the raised part of the tendon in its natural direction, on the correspondent fixed part, and they were both exactly congruous; so that the loose part had not contracted itself, nor was at all shorter, after these repeated trials, than its correspondent fixed part. He then put the creature on its legs, to see whether it had suffered so much, that it could not use the leg; but it was found to walk, though favouring greatly that side where so much had been done; however, it walked fairly on all its legs. After about 5 minutes torment, the butcher ended all its pains, and he performed the same processes on a sheep just destined to be slaughtered, in which the Dr. found all the appearances as abovementioned.

He was induced to make 2 other very cruel experiments on different animals, by laying bare their patella's of the knees; having cut off all the skin round about, he then pricked and touched with the afore-mentioned escharotics the capsular ligaments of these joints, without discovering any tokens of pain; but as soon as the sharp fluids had spread over the surface, so as to reach the extremity of the skin, the creature underwent as much pain as cutting before had caused.

He desired the butcher to take off as much skin from the forehead, as was necessary to perform the operation of the trepan; and before he began to apply the instrument to the sheep's forehead, he vellicated the pericranium with the end of a knife, but could not observe the membrane sensible, or thrown into contractions; and when the operation was over, and the bone taken from the subjacent dura mater, he poured on this membrane dulcified spirit of nitre, and diluted spirit of vitriol, and powdered common salt, but without perceiving any agitations whatever, brought on by these substances acting on these living parts; though in some creatures he was dubious, whether sea-salt and nitre in powder did not create some sense, though no manifest contractions of the dura mater.

But every muscular part, which he cut while the animals were alive, discovered little sensibility of pain, though great propensity to irregular spasms of the fibres; and the muscles on the thorax, and especially the carneæ columnæ of the heart, retained irritability last of all other muscular parts, even till long after the animal's expiration.

He laid the pungent liquors and salts, as above, on various parts of the animal, yet alive; as on the fat, cellular membrane of the neck, leg, and other parts within the skin, the liver, pancreas and spleen, and could not find them endowed either with remarkable sensibility or irritability; nor had the bladder any remarkable symptoms of irritability, further than might be occasioned by its muscular fibres; though the well-known symptoms of the calculus show its great sensibility.

He tried the effects of a strong aqueous solution of opium on the irritated parts of muscular fibres, but could not perceive an opiate manifestly to compose these spastic motions of the parts, as Haller alleges they do, though in some trials he fancied there were grounds for such a conclusion. However this is no argument against the internal use of opiates, where the solids are greatly irritated.

He adds one more experiment, made on the intestines of a lamb: after he had taken them from the carcase, he poured diluted spirit of vitriol on them, as well as several other pungent substances; and on the touch of all of them, the intestines renewed their contraction, which before had totally ceased, and surprised him with a motion almost as strong as is found in the process of chylification; and this continued till the external cold had indurated and stiffened the fatty membrane of the omentum.

These were some of many experiments of a like nature, which the importance of these facts in daily practice of medicine required to ascertain, or reject; and, from the result of his repeated trials, he was induced to coincide with most of the conclusions drawn by Drs. Haller, Castell, and Zimmerman; that no part is sensible but the nerves only, and that some parts are irritable without sensibility, accompanying them in any great degree; while others are altogether without sense, at the same time that they are incapable of being irritated at all.

Dr. B. adds, that he had thus communicated to the Royal Society the result of his experiments on this subject. Whether he should, by prosecuting the subject still farther, be able fairly to make out, that irritability, as it is distinguished from sensibility, depends on a series of nerves different from such as serve either for voluntary motion and sensation, he could not then say. But whatever might be his future conclusions, he would establish nothing hypothetical, but endeavour by fair deductions to approach towards truth, as near as the abstruse nature of the subject would permit; and as he thought he had actually found some variation from the common practice in rheumatisms, built on the established fact of great irritability in the muscular fibres, succeed, to the relief of suffering patients, he could not dismiss this subject, without relating, that only with gentle and continued frictions on the pained rheumatic parts with common sallad oil, 2 poor patients, who lately applied for his advice in obstinate rheumatisms, were, by thus relaxing the crispation of the solids, surprisingly relieved, without any further medicine. So that after bleeding, where it is indicated, which above all things he found to abate irritability, it might deserve to be tried, how far animal oils, applied by friction long continued to the aggrieved parts, both in the gout, rheumatism, and other painful diseases, would ease the tortures, without repelling or obstructing the matter, which nature is labouring to throw off. But he forbore to enlarge, as the experiments he had hitherto made on the subject of irritability, were scarcely sufficient to obtain what Lord Bacon calls the vindemiatio prima in this science. When he should receive sufficient information to be convinced within himself, he should not be wanting to communicate what might tend to advance this branch of natural knowledge, and to promote a true theory of diseases, on which all rational practice must be established.

XXXIX. *Of Worms in Animal Bodies.* By *Frank Nicholls, M. D. Med. Reg. and F. R. S.* p. 246.

Fish are, to appearance, more subject to worms* than other animals; the cod often shows small slender worms, coiled up like snakes, on the surface of its liver; and the bley in the Thames, about the month of July, is often distressed by a long flat worm, which, by possessing and eating its liver, prevents the fish from compressing itself to that specific gravity, which is necessary for its quiet continuance under the water; so that it is obliged to skip about on the surface of the water, till it becomes a prey to its foes, or dies suffocated, by being so often out of water, and deprived of that action of the water which is analogous to the force of the air to us in breathing.

Among the many cases, which Dr. N. had seen, two seem to deserve particular attention, as well because they are greatly prejudicial to the farmer, as because, when generally known, they may possibly lead to a method of successful cure. The first of these is a species of dropsy, incident to bullocks and sheep. On opening these animals, when dead of this rot, the liver is always found affected. A small flat worm,† resembling a sole, and often many of them, is found in the gall-duct, by the butchers termed flooks [flukes]. It is the property of this worm, that it always builds a wall of stone for its defence; which wall is ramified like the gall-duct, within which it is formed. This stony tube, when completed, blocks up the gall-duct, and stops the passage of the gall; which thereby surcharging the duct, and dilating the orifice of the lymphatics, returns again into the blood, and gives the yellow teint to the eyes, which is the first symptom of this disease, and generally precedes the loss of flesh, and the swelling of the belly. It seems probable, that whatever can increase the acrimony of the bile, must be useful in preventing this disease; but when the stony pipe is formed, no method seems capable of promoting its discharge, or dissolution.

The other case is termed the husk, and is a disease to which bullocks are very subject, while young; for it rarely affects those of more than a year old. The creature is seized with a short dry cough, by which it is perpetually teized; in consequence of which he wastes in flesh, and grows weaker and weaker till he dies. On opening the lungs of a calf dead of this distemper, he found the wind-

* The worm here alluded to, is the *ligula abdominalis.* Linn. Gmel.

† This worm is the *fasciola hepatica*. Linn.

pipe, and its branches, loaded with small taper worms* of about 2 inches long, which were crawling about, though the animal had been dead many hours; and the farmer assured him that they always found these worms in this distemper, and knew of no method of cure. Dr. N. had great hopes however, that fumigations, either with mercurials, as cinnabar, or with fetids, as tobacco, properly used, might prove of great service.

XL. On some Remarkable Insects of the Polype Kind, found in the Water; near Brussels in Flanders. By T. Brady, M.D. p. 248.

The draught of the plant sent is found in summer-time, in all sorts of ditch or stagnant waters: its colour is white, and its transparent body, when seen with the naked eye, is in length between one and a half and two lines; but when viewed with a good microscope, whose focus is about 8 lines, it appears as in pl. 15, fig. 1, with leaves,† branches, and fruit, and indued with such sensibility that at the least noise made in the room, or on any thing touching the table where the microscope stands, or the water in which it lies, it contracts itself with such activity and swiftness that the eye cannot follow it in that motion, till it reduces itself into the shape in fig. 2. The extension or dilation goes slower, and requires about half a minute before it comes to the form in fig. 5. It can live in its own standing-water for 8 or 10 days, and then looks as in fig. 6, as most trees do in winter-time. It is remarkable that the leaves, which are like bells, live some time after they fall, and retain that faculty of contraction and dilatation; and when viewed with the great magnifier, whose focus is about 2 lines, it appears as in fig. 4. The trunk is as in fig. 3. The number of its branches are undetermined, but commonly found to be between 6 and 12. He had not tried if it did not regenerate, when cut like polypes: but he could see a vast difference between it and the polype a bouquet, mentioned by Trembley.‡ The other curious insect, represented in fig. 7, is found in the same standing-waters with the plant, and is seen with the naked eye, like a little flat round leaf, whose diameter is about one line and a half; but when put in a microscope, it shows a circle surrounded with crowned heads, tied by small thin tails to a common centre, whence they advance towards the circumference, where they turn like a wheel, with a great deal of vivacity and swiftness, till they cause a kind of a vortex, in which are seen all smaller insects or bodies either attracted or driven, which probably serve as nourishment for those little crowned things, which in all appearance are, as well as the plant, a sort of insects of prey, that live on smaller creatures. When one of those little heads

* These worms belong probably to the species of ascaris called ascaris vituli. Linn. Gmel.

† Vorticella anastatica. Linn.

‡ Vorticella socialis. Linn. Gmel.

has wheeled a while, it rests, and another turns out; and sometimes 3 or 4 are seen wheeling at a time. He had seen last year some much more regular, that formed an orderly circle, with their crowns to the circumference, and their thin bodies like so many radii joined to the centre. Their motion is all straight towards the edge of the circle, and never to the right and left, as if every head had its proper limits to act on.

The fruit of the plant, which resembles an orange, has a kind of chain about it, that turns as the crown does in the other insect. The trunk or stock of the plant is its gut, or stomach; for he saw, that something descended through it, as it were through a gut. Besides it has no support of any fixed point, but is always swimming in the ditch-water, but shows no great local motion. Other insects were seen preying on it, which resemble small hogs, and are very busy in eating its leaves, which are probably the cause of its looking so bleak and withered when dead.

XLI. New Astronomical and Physical Observations made in Asia; and communicated by Mr. Porter, Ambassador at Constantinople, and F.R.S. p. 251.

Observed Latitudes of the following places.

Aleppo. Lat. North........	36^h	12′	Antioch..............	36^h	10′	
Mount Cassius...........	36	4	Diarbekir	37	54	
Seleucia in Syria	36	3	Bagdad	33	19	54″

Immersion of ω Virginis under the Moon, observed June 10, 1753, *at Diarbekir, near the Seraglio of the Bachaw.*

The Immersion of the Star at night..........	9^h	48^m	4^s
The Emersion	9	39	47

The nitre is produced by a combination of the universal acid with the natrum of the ancients, as appears by observations. The asafœtida is drawn from a ferulaceous plant of the thapsia kind, which is very common in Media, &c. I have had the good luck to find the small nardus Indica: It is a gramineous plant, of which some bear spicaceous flowers, both male and female, and others only female ones. It is a valuable thing to botanists, as they are hitherto ignorant of the true genus of this plant, though the root has been in use ever since the age of Dioscorides. This country is so dry, that electrical experiments often succeed without any stand of bitumen, pitch, silk, glass, &c. Our carpets and beavers are mostly sufficient to retain the electrical virtue, and prevent its spreading to the floor. Ten men standing upright, one before the other, have been made electrical, and, being touched, have produced sparks.

XLII. Some Observations, proving that the Fetus is in Part Nourished by the Liquor Amnii. By Malcolm Fleming, of Brigg, M.D. p. 254.

July 25, 1753, being informed that a calf, come to full maturity, was just then brought forth dead in this town (Brigg, in Lincolnshire,) which had been alive, and appeared strong a very short time before its birth; Dr. F. begged it of the owner, such instances being rare. The skin being of value, for it was an extraordinary large calf, it was sent to his house flayed. He first examined the thorax, which was his chief motive for begging it. He here adverts to the experiment of the lungs of a new-born animal sinking in water. After cutting out the lungs and heart, he clipped off a piece of the former with sharp scissars, about an oz. weight, or more, and threw it into a basin full of water. It quickly sunk to the bottom, and settled there. Immediately after, he blew into the remaining part of the lungs, through the trachea; and though he could by that means distend them but very little, because the air flowed out readily through the cut bronchia, and therefore acted but faintly on the other parts; yet a piece about the same size as the first, clipped off in the same manner, and thrown into the same basin, constantly kept at the top. This might seem foreign to his present purpose; but he thought proper briefly to mention it here, not only on the account of the importance of the experiment, but likewise to show, that he was not misinformed in the account of the calf's being brought forth dead, and that it had not even respired; much less taken any nourishment after exclusion, to influence the appearances described below.

Having opened the abdomen, he observed the thick intestines, especially the rectum, extremely distended with an incredible quantity of meconium; which for several inches above the anus was formed into distinct scybala or balls. He made an incision in the rectum, where it was very turgid, about 2 inches from the anus, and let out about 25 or 30 of these scybala; which he laid on clean paper to dry, that he might examine them at his leisure. About 3 or 4 days after, when they were dry and brittle, and of the colour and consistence of aloes, he was surprised to find, on examination, every ball stuck full of tough, thick, white hairs, some of which were an inch long, or more. There seemed to be some scores in each, though, being shrunk with drying, they scarcely exceeded the bulk of an ordinary pea. This unexpected appearance set him on considering, whence these hairs had come; how got they there? and he could think on no other tolerable solution of the difficulty than to conclude that they belonged originally to the calf's skin; and, being loosened by maceration in the liquor amnii, were propelled into the stomach and intestines; till they were at length entangled in the meconium. He was confirmed in the belief of this by being informed, on inquiry, that the calf's skin was white; a circumstance unknown to him before, it having been sent flayed. From this persuasion it was natural to infer that if hairs loosened from the skin of the fetus, and floating in the liquor amnii, can find a way into the intestines, and get entangled in the meconium, it is impossible but the liquor amnii must enter and pass through the whole alimentary passage along with them; as a fluid may certainly penetrate where hairs cannot: but no good reason can be assigned, or even conceived, why hairs should be admitted where the fluid is excluded.

The only reasonable scruple that remained to be got over was, that this being but a single instance, a general conclusion was not to be too hastily drawn from it; that it was possible there might be some morbid concretions in the meconium of this particular calf, resembling hairs, which concretions in a common and natural way might be wanting; or some preternatural communication between the primæ viæ in this subject, and the liquor amnii, not to be found in the generality of other fetuses. But he afterwards received some of the first dung of other calves, in which he also found a great number of strong hairs all over; so as to leave no room for doubting but that this appearance is general in the meconium of calves, in a natural way.

The reader will please to observe that in neither of these instances he could be deceived, if he had ever so little reason to trust to the judgment and fidelity of those who supplied him with what he wanted. The colour and consistence of the meconium of a fetus is so very peculiar, and so widely different from that of fæces formed out of ingested aliments, that none, who have any knowledge in these matters, can mistake the one for the other. In the mean time he omitted not to open the embryos of the cow-kind, such as he could procure in the shambles of the market-town he lived in, and to examine their meconium. The 2 most advanced towards maturity, which he met with, had stiff long hairs about the mouth, the eye-brows, the ears, and navel, and a good many on the end of the tail; but none on their skins. In neither of these, any more than in the younger embryos which he examined, was there so much as a single hair to be found in the meconium; for this plain reason, if he judged right, because they had not got hairs on their bodies of long enough continuance to become loose, and float in the liquor amnii.

But as opportunities of coming at fetuses of this species, especially such as are remarkably nearer to maturity than those 2 just now mentioned, are rare, he tried to supply that defect by opening those of other animals. Accordingly he procured 6 puppies, of the butcher-dog kind, brought forth at the full time at one litter. Having taken out the whole meconium of every one of them, after the strictest search he could find no hairs in any part of it. He had likewise an opportunity of opening a colt that died either in the birth, at the full time, or immediately after, before its meconium was discharged; which he found in great quantities in its rectum and colon. But neither here could he spy a single hair, though he examined whole pounds of it, and that portion most carefully which was lodged in the rectum, near the anus.

These observations might seem at first view to clash with and contradict those he had related: but, on closer consideration, they would be found in reality to confirm them, for this reason, that puppies and colts, when brought forth, have no loose hairs on their bodies; but calves have in great numbers. In the puppies and colt, which he examined, the hairs were so firmly rooted on their skins, that he could scarcely pull any off with his thumb and fingers; whereas in a mature calf, new brought forth, many are found quite loosened at their roots, and only adhering to their skin by the moisture on it. Therefore in the latter species hairs from the surface may be, and actually are, incorporated with the liquor amnii, and along with it enter the mouth and alimentary canal, which cannot be the case in the former. From these facts Dr. F. infers that the liquor amnii is in a constant natural way received into the mouth, stomach, and intestines, and therefore must contribute to the nutrition of the fetus.

XLIII. On the Success of Agaric in Amputations, &c. By Mr. William Thornhill, late Surgeon to the Infirmary at Bristol. p. 264.

Mr. T. here states that he had employed the agaric successfully in 4 cases of amputation.

XLIV. A Lunar Eclipse observed at Elbing, March 27 *and* 28, 1755. *By John Mendes Sachetto Barbosa, F. R. S., and Prof. of Philos. and Physic.* p. 265.

27^d	10^h	51^m	15^s	the beginning was certain.
28	1	27	40	the end of the real eclipse.
	1	31	30	the penumbra certainly ended.

XLV. On the Number of People in England. By the Rev. Wm. Brakenridge, D. D., Rector of St. Michael Bassishaw, London, and F. R. S. p. 268.

There seems to be only two ways of discovering the number of people in England, where at present there are no capitation taxes; either by the number of houses, or the quantity of bread consumed. As to the first, it is evident that if the number of houses could be determined, it would then be very easy to compute nearly the number of people. For it might be easily known by trial what number, at an average, could be allowed to each house, and from thence the whole number of people deduced. In a former letter Dr. B. assigned 6 to a house in town, which he found to be the nearest number, in some parishes, by an account taken; but he thinks it is still more plain in the country that 6 is the number to be fixed on, where people do not go so much into single life, and where there are not so many lodgers. For if we consider that for every marriage there are four births, on an average, as Dr. Derham, Major Graunt, and others have shown, and which Dr. B. found to be true from the registers both in the town and country; consequently, allowing for deaths, there cannot be 3 children that survive from every marriage to mature age, and indeed not much above 2, as appears from Dr. Halley's table of the probability of life. Therefore every family, where there are children, one with another, cannot consist of more than between 4 and 5 persons, besides servants or inmates: which shows plainly that families, where there are children, cannot be estimated at more than 6 to a house, and where there are no children they cannot be reckoned more at an average.

The number then being 6 to be assumed, let us next consider what number of houses is to be supposed. That Dr. B. might come at some certainty in this, he applied to one of the public offices, where he thought they could very likely give an account of them; and he there found, that before the year 1710, and near about that time, an account had been taken of all the houses throughout England and Wales, in order for some assessment upon them; and the number then amounted to 729048. In which it may be supposed that a number of cottages were omitted that might be improper for that assessment; but he thinks there could not possibly be above a 4th part of that number more: for surely the surveyors, if they had any care of the public revenue, would never omit above one in 5. Let us therefore suppose, that there might be a 4th part of that number more; and then those omitted will be about 182262, and the whole number of houses could not exceed 911310.

If now we take 911310 for the number, it is evident, if we allow 6 persons to a house at an average, the number of persons in England and Wales, before the year 1710, could not be above 5467860. And since that time, 45 years ago, by a method of computing which he shows below, the increase could not be above 789558; and so the whole number of people now must be about 6257418; or six millions, all ages included; for it must be remembered that in our wars, since 1710, there could not be fewer lost than 200000, which is to be deducted from that number.

As to the other way of determining this, by considering the quantity of bread consumed, it may perhaps at first view appear more uncertain; but it will, he thinks, from some things that may be observed, at least help to ascertain the above number. For it is plain, if the quantity of wheat that is produced in England could be known, it would then be very easy to make the computation, as it might be nearly discovered, by a little observation, what each person at an average might consume. But the great difficulty is to find out nearly the quantity of wheat; and there seems to be no way at present of knowing it, but by considering what proportion it may have to the barley; for the quantity of that is nearly known from the malt-tax. Now, if we compare the quantity of the wheat in England, it is evident, that there is at least as much ground sowed with the one as with the other. For there are vast tracts of land that will not bear good wheat, but are frequently sowed with barley; and even those lands that will produce good wheat, they are often alternately sowed with it: the land that is rich and well manured, after one crop of wheat it is usual to sow it with barley. And if this be admitted that the quantity of land sowed with the one is equal to that sowed with the other, there must then be a much greater quantity of barley; because the same number of acres will produce much more of it, and generally in a greater proportion than 3 to 2.

If then we assume that the barley used in malt is to the wheat used in food at home, as 3 to 2, we shall then be able to compute the quantity of each of them in this manner: the malt-tax from the year 1747 to the year 1753 inclusive, amounted to the sum of 4,254,813l. of which the 7th part, the tax for one year, is 607830l. and as the tax is 4 shillings on every quarter of barley, it follows that there are 3039150 quarters of barley consumed yearly in malt; and therefore there must be 2026100 quarters of wheat consumed at home. Now, as it is known, that labouring healthy people at an average consume about one quarter of wheat in the year, which is about 512lb. of flour, or 1lb. 6oz. in a day, we may allow that healthy and unhealthy, grown people and children, do not consume the half of that quantity, one with another. And therefore, that we may make the consumption of each person at an average as small as can reasonably be imagined, we will suppose that 3 people, children included, do not consume more than one hearty labouring person, that is one quarter in the year, or each person about 7 oz. in a day; and by this supposition the above number of quarters of wheat 2026100, consumed at home, will be sufficient for 6078300, or six millions of people. And this quantity of a quarter to 3 persons, though it appears too little, may be admitted, as in some of the northern countries they use some oat-bread and rye-bread; and every healthy person may, one with another, be allowed to consume this quantity at least. From this calculation it seems that there cannot be above 6 millions of people in England. And as, from the other method of computing from houses, we found the number to be about 6,257400, from which at least 200,000 is to be taken for those lost in the wars since 1710, or near that time; it appears that both these calculations confirm each other, and that the number of people may be considered at about 6 millions, or rather less. In which, according to Dr. Halley's rule, there will be about 15 hundred thousand men able to carry arms.

Dr. Derham, from the computations of Mr. King, supposes there is about 5½ millions of people in England; to which, if we add the increase that may be since that time, the number will be near about what we have made them. But Sir William Petty has endeavoured to make them, in his time, no less than 7369000, by supposing them to be in proportion to the assessment, then 11 times greater than that in the city of London. In which, with regard to the city, he was certainly mistaken, as Dr. B. showed last year; for the number at that time, in 1682, was not much above 504000, and therefose 11 times that, viz. 5544000 must, according to his own hypothesis, be the number of people in England. And if we allow 1355000 to be the increase in about 73 years since that time, the number could not be now, according to that assessment, above 6899000. From which we ought at least to subtract 400000, which may be justly allowed for loss in our wars since 1690; and the remainder 6499000 is not half a million more than we have made them. The people then being computed at 6 millions, or rather less, it appears that England is but thinly peopled. For not only the exportation of at least 400,000 quarters of wheat annually shows plainly that we want people to consume it at home, and that we maintain in bread about a million of foreigners abroad: but if we examine more particularly, we shall find that the country is capable of supporting one-half more inhabitants, or 9 millions.

But in Ireland the case is still worse: for if there is but a million of people, as is commonly supposed, and according to Mr. Templeman 27400 square miles, which is 17,536,000 acres, and a 4th or more be supposed waste; then there will be at least 12,000,000 good acres. And consequently if 4 acres in that country be allowed sufficient, at an average, for the maintenance of one person, Ireland, if duly cultivated, could maintain 2 millions more people than it has now, or 3 times its present number of inhabitants. And in Scotland, if there be, as is said, but a million and a half of people, for at present I know no way to compute them, and 27700 square miles, or 17,728,000 acres, and $\frac{1}{3}$ be supposed waste, which is not too much in that country, then there will be 11,000,000 good acres; of which, if we suppose that 5 acres of that soil is not more than sufficient for each person, then there may be provision for 2,200,0000 people, or more, with the advantages of fishing, that is 700000 more than there are at present. From all which it is plain, that if the land in both the British isles was duly cultivated, they might sustain about 6 millions more people than they do now; that is as many more people as England now contains. And here, by the way, it may be observed, if we extend our thoughts to the whole globe of the earth, and compare the quantity of land with the number of people, we shall find that it will maintain above 26 times the present number of mankind.

The proportion being given of the living to the dead in one year, and also the proportion of the births to the dead, the number of the people being unknown;

to find in what time the people shall be in any given proportion, to what they are at present. Suppose n to be the unknown number of the people at present, and let the living be to the dead, in one year, as l to 1, and the dead to the births as 1 to b, the proportion given to what their number is at present as p to 1, and the number of years required to be y. It is plain then, that the dead at the end of the first year will be $\frac{n}{l}$, and the births $\frac{bn}{l}$, and the whole number of people must be $n + \frac{bn}{l} - \frac{n}{l}$. In like manner, at the end of the 2d year, the dead will be $\frac{ln + bn - n}{l^2}$, and the births $\frac{lbn + bbn - nb}{l^2}$, and the whole number of people must be $n + \frac{bn}{l} - \frac{n}{l} + \frac{lbn + bbn - nb}{l^2} + \frac{n - ln - bn}{l^2} = (\frac{l + b - 1}{l})^2 n$. And so at the end of the 3d year the number of people will be $(\frac{l + b - 1}{l})^3 n$. From which at length it is evident by induction, that the number of people at the end of the required number of years will be $(\frac{l + b - 1}{l})\ n$. But as the proportion is then to be as p to 1, we shall have $(\frac{l + b - 1}{l})^y n = pn$, and thence $(l + b - 1)^y = pl^y$. And because the logarithms of equal quantities must be equal, we shall have $y \times \log. (l + b - 1) = \log. p + y \times \log. l$, and also $y = \frac{\log. p}{\log. (l + b - 1) - \log. l}$. And therefore the number of years y is determined by the logarithms of known quantities, when the people shall be in the given proportion of p to 1.

It may be observed that the quantity $(\frac{l + b - 1}{l})^y n$ may be considered as the ordinate of the logarithmic curve, whose abscisse is the index y, and that the ordinate passing through the beginning of the abscisse, where $y = 0$, must be equal to n.

If now it be required to know when the people shall be doubled; let us substitute in the above formula, instead of b, l, p, the respective numbers 1.12, 40, 2. and it will be $y = \frac{\log. 2}{\log. (40 + 1.12 - 1) - \log. 40}$; and then the logarithms being taken we shall have $y = \frac{0,3010300}{0,0013009} = 231$; which shows that, according to the present state of births and burials, the people could not be doubled in less than 231 years. And by the same method it appears, changing the signs of $b - 1$, that 230 years ago, in the time of Henry the 8th, the number could not be above $\frac{1}{2}$ of what it is now, that is about 3 millions.

And so if we were to find, when the number of people in England would be increased to 9 millions, which, by what has been said above, is near about the outmost that can be maintained, from the natural produce of the country; we should then have $p = \frac{3}{2} = 1.5$, because 9 millions is to the present number as 3 to 2, and also $y = \frac{\log. 1.5}{\log. (40 + 1.12 - 1) - \log. 40} = \frac{0,1760913}{0,0013009} = 135$; which shows that, at the present rate of births and burials, it must be 135 years before England can be fully peopled.

If we suppose, as Sir William Petty does, that the burials are to the births as 9 to 10, that is 1 to 1.111, which is something less than that of Dr. Derham's proportion, and that 1 dies in 40 in a year; if we substitute these numbers in the formula, we shall then find the time of doubling to be 250 years. For then it will be $y = \frac{\log 2}{\log. (40 + 1.111 - 1) - \log. 40} = \frac{0,3010300}{0,0012035} = 250$; which shows how far Sir William was mistaken in his method of calculation, when he made the time to be 360 years.

After the same manner, the number of years being given, it will be easy to find the proportional increase. Suppose after 45 years. For then we should have $45 \times \log. (l + b - 1) - 45 \times \log. l = \log. p$; which will give $45 \times 0.0013009 = \log. p$, and therefore $p = 1.1443$, from which if n be equal to 5,467,860, we have $pn = 6,256,872$. So that it appears if there was 5,467,860 people in England at the year 1710, when the above-mentioned survey was made, there is now 6,250,000; if none were to be deducted on account of our wars, and emigrations to our colonies since that time.

From what has been found above, that $(l + b - 1)^y = pl^y$, it is evident, that the ratio of the increase in any number of years may be determined, without the number of people being known, or their proportion to the annual increase; and also that any one of the quantities l, b, y, p, may be found, the others being known. But if the ratio of the number of people to the annual increase be known; and consequently the proportion, of the number in any one year, to the number next year known, we shall then have a very simple equation. For if we suppose the number of people in any one year, to be to that number with the increase added in the next year, as 1 to r, we shall then have $nr^y = np$, or $r^y = p$. And, in like manner, if the proportion of the number of people to their increase, in a given cycle of years, had only been known, and that cycle be c, we should then have $nr^{\frac{y}{c}} = np$, or $r^{\frac{y}{c}} = p$. From which formula it would be easy to calculate the numbers of mankind, in all ages through the world, if we suppose them to arise from a given number, and the rate of increase known, in any period of years. And this may sometimes be of use to discover the number in any age, that might be possible to reason on, and to find out the truth of any hypothesis.

XLVI. An Attempt to Explain Two Roman Inscriptions, cut on two Altars, which were dug up some time since at Bath. By John Ward, LL.D. and V.P.R.S. p. 285.

These two inscriptions were found near the same time and place, with that which has been already published in the 48th vol. of the Philos. Trans. The altars, which contain them, are in the possession of Dr. William Oliver, physician at Bath, who has placed them in his garden, and who transmitted draughts of them, with their inscriptions, taken by the Rev. Mr. Borlase, F.R.S. And after that, Mr. Prince Hoare sent casts of the inscriptions in plaster of Paris.

The inscription on the higher altar may, Dr. W. thinks, be thus read in words at length:

Peregrinus Secundi filius, civis Trever, Jovi Cetio, Marti, et Nemetona, votum solvit libens merito.

The person, who dedicated this altar, calls himself PEREGRINVS SECVNDI FILIVS; each of which names occurs several times in Gruter, as a cognomen, which often stands alone, when the person named is sufficiently distinguished by it. Having given us his own name, and that of his father, he proceeds to acquaint us with his country, and stiles himself CIVIS TREVER, a people who inhabited that part of Belgic Gaul between the Maese and the Rhine, which is now the electorate of Triers; and were conquered by Cæsar, with the rest of the Gallic nations. Their chief city, which was situated on the Moselle, being made a Roman colony in the reign of Augustus, is by Tacitus called Colonia Treverorum, but by others more frequently Augusta Treverorum, and now Triers.

The 3 following lines of the inscription contain the names of 3 deities, to whom this altar was dedicated. The first of these is here called IVPITER CETIVS. Ptolemy makes mention of a large mountain in Germany, which he calls Κέτιος, and describes as the eastern boundary of Noricum, by which it was separated from Pannonia, now Hungary. From this mountain it seems highly probable, that the name Cetius might be given to Jupiter, as its tutelar deity.

The 3d and last name here mentioned, is NEMETONA, which Dr. W. had no where else met with; but as it stands connected with the two former by the particle ET, it must, he thinks, denote some deity, and by the termination a goddess. The last line of the inscription acquaints us with the cause of erecting this altar, which was the performance of some vow, formerly made by Peregrinus. And it is not improbable, that he had laboured under some bodily disorder, which occasioned his going to Bath for the benefit of the waters, which in the time of the Romans were in so high esteem. And the good success which he met with by the use of them, may be concluded from the tenor of the inscription, wherein he makes his acknowledgement to the deities above-mentioned, for the benefit he had received through their favour, in consequence of his addresses to them for that purpose. For as it was a common notion of the ancient pagans, that all human affairs were under the direction of their deities; so in any danger or misfortune they used to solicit them for relief, with vows and promises of erecting altars and other buildings to their honour, in case of a favourable answer. Which, when performed, they were said votum solvere, as the letters v. s. here imply.

The other inscription, on the lower altar, when expressed in words at length, may be read in the following manner:

Sulevis Sulinus Scultor, Bruceti filius, sacrum fecit libens merito.

That the first word SVLEVIS denotes a name given to certain rural goddesses, called Sulevæ, is plain from an inscription found on a stone at Rome, and published by Fabretti, in which they are joined with Campestris. The 2 next words, SVLINVS SCVLTOR, must, he thinks, stand for the names of the person who dedicated this altar; as the 2 following, BRVCETI F. acquaint us with that of his father. The words SACRVM FECIT, in the last line, are of the same import with dedicavit; in which sense likewise sacrum alone is often used. And some times the reason of the dedication is added, as, sacrum, voto suscepto, fecit, in Gruter. But that not being mentioned here, must remain unknown.

There is nothing said in either of these inscriptions, which can afford any light towards settling the time, when they were erected. But so far as appears from the form of the letters, they may not improbably be supposed of somewhat a later date, than that mentioned before, as found near the same place.

XLVII. Of a remarkable Echinus. By Gust. Brander, Esq. F.R.S. p. 295.

This echinus was of a very singular species. It appeared to be of a middle nature between the echinus and the star-fish. It came from the island of Bourbon in the East Indies, and he could not learn that it was any where described. See fig. 10, pl. xi.

XLVIII. Of an Impression on a Stone dug up in the Island of Antigua, and the Quantity of Rain fallen there for 4 Years. By the Rev. Francis Byam. p. 295.

This stone was brought from a quarry for a building in the town of Antigua: the quarry is in the side of a mountain, and is about 300 yards higher than high-water mark, and about 2 miles from the sea. When the mason struck it with his hammer it split in two, and discovered the exact figure of a fish, on each stone, called an old wife.

The quantity of rain that fell in Antigua, was in 1751, 51.8 inches; in 1752, 43.3 inches; in 1753, 32.8 inches; in 1754, 75.2 inches.

XLIX. On the Stones mentioned in the Preceding Article. By Mr. Arthur Pond, F. R. S. p. 297.

The impression of this fish is in a chalky kind of stone, of a pale ochrey colour; some parts, when scraped, are white, and all the impression is of a yellowish brown, nearly the colour of brown ochre. The impressions of the bones and fins are very perfect; and the cavity, that contained the back-bone, extremely sharp and delicate. When Mr. P. first saw it, 2 or 3 of the vertebræ were in it. All the cavities of the bones are now sufficiently open to contain them, and it is probable that most, if not all of them, were in the stone, when it was first split. Between the rib-bones and the two long fins, which come down from the head, which parts were only fleshy, there is no impression, the stone having united quite through; and on the upper part of the fin, by the side of the cheek, is a deep impression of a very small cockle-shell. The impression on the counter-part of the stone is much the same, except that the tail is wanting.

L. On the Effects of Lightning in the Danish Church, in Wellclose-square. By Gustavus Brander, Esq., F. R. S. p. 298.

On Monday, Nov. 17, between 6 and 7 o'clock, there was, among many others, one most amazing flash, accompanied with a clap of thunder, that equalled in report the largest cannon. The next morning, the minister observing the church clock to be silent, they went into the belfry, and found the wire and chain, that communicated from the clock in the belfry, to the clapper in the turret, where the bells hang, were melted; and that the small bar of iron from the clock, that gives motion to the chain and wire, just where the chain was fastened, was melted half through, the bar being about $\frac{3}{4}$ of an inch broad, and half an inch thick. By several links of the chain, and of the wire, it is observed, that the lightning took effect only in the joints. But whether it entered by communication, from the wire exposed to the air in the small turret, through the roof of the belfry, or at the windows, there being several panes broken in the south and west corners, is uncertain; though Mr. B. presumes rather the first way, as it is very possible, that the bare report of the thunder might have occasioned the latter.

The pieces of the wire and chain were scattered over the whole belfry, nor could it be discerned, that the wood-work, or ought else, had suffered.

LI. Electrical Experiments, made in Pursuance of those by Mr. Canton, dated Dec. 3, 1753; *with Explanations. By Mr. Benjamin Franklin, F. R. S. Dated Philadelphia, March* 14, 1755. p. 300.

Principles.—1. Electric atmospheres, that flow round non-electric bodies, being brought near each other, do not readily mix and unite into one atmosphere, but remain separate, and repel each other. This is plainly seen in suspended cork balls, and other bodies electrified.

2. An electric atmosphere not only repels another electric atmosphere, but will also repel the electric matter contained in the substance of a body approaching it; and, without joining or mixing with it, force it to other parts of the body, that contained it. This is shown by some of the following experiments.

3. Bodies electrified negatively, or deprived of their natural quantity of electricity, repel each other, (or at least appear to do so, by a mutual receding) as well as those electrified positively, or which have electric atmospheres. This is shown by applying the negatively charged wire of a phial to two cork balls, suspended by silk threads, and by many other experiments.

Preparation.—Fix a tassel of 15 or 20 threads, 3 inches long, at one end of a tin prime conductor; (mine is about 5 feet long, and 4 inches diameter) supported by silk lines. Let the threads be a little damp, but not wet.

Exper. 1.—Pass an excited glass tube near the other end of the prime conductor, so as to give it some sparks, and the threads will diverge.—Because each thread, as well as the prime conductor, has acquired an elastic atmosphere, which repels, and is repelled by, the atmospheres of the other threads: if those several atmospheres would readily mix, the threads might unite, and hang in the middle of one atmosphere, common to them all.

Rub the tube afresh, and approach the prime conductor with it, crossways, near that end, but nigh enough to give sparks; and the threads will diverge a little more. Because the atmosphere of the prime conductor is pressed by the atmosphere of the excited tube, and driven towards the end where the threads are, by which each thread acquires more atmosphere.

Withdraw the tube, and they will close as much.—They close as much, and no more, because the atmosphere of the glass tube, not having mixed with the atmosphere of the prime conductor, is withdrawn entire, having made no addition to, or diminution from, it.

Bring the excited tube under the tuft of threads, and they will close a little. —They close, because the atmosphere of the glass tube repels their atmospheres, and drives part of them back on the prime conductor.

Withdraw it, and they will diverge as much.—For the portion of atmosphere, which they had lost, returns to them again.

Exper. 2.—Excite the glass tube, and approach the prime conductor with it, holding it across, near the opposite end, to that on which the threads hang, at the distance of 5 or 6 inches. Keep it there a few seconds, and the threads of the tassels will diverge. Withdraw it, and they will close.—They diverge, because they have received electric atmospheres from the electric matter before contained in the substance of the prime conductor; but which is now repelled and driven away, by the atmosphere of the glass tube, from the parts of the prime conductor, opposite and nearest to that atmosphere, and forced out upon the surface of the prime conductor at its other end, and on the threads hanging to it. Were it any part of the atmosphere of the glass tube, that flowed over and along the prime conductor to the threads, and gave them atmospheres (as in the case when a spark is given to the prime conductor, from the glass tube), such part of the tube's atmosphere would have remained, and the threads continue to diverge; but they close on withdrawing the tube, because the tube takes with it all its own atmosphere, and the electric matter, which had been driven out of the substance of the prime conductor, and formed atmospheres round the threads, is thereby permitted to return to its place.

Take a spark from the prime conductor, near the threads, when they are diverged as before, and they will close.—For by so doing you take away their atmospheres, composed of the electric matter driven out of the substance of the prime conductor, as aforesaid, by the repellency of the atmosphere of the glass tube. By taking this spark, you rob the prime conductor of part of its natural quantity of the electric matter; which part so taken is not supplied by the glass tube; for when that is afterwards withdrawn, it takes with it its whole atmosphere, and leaves the prime conductor electrized negatively, as appears by the next operation.

Then withdraw the tube, and they will open again.—For now the electric matter in the prime conductor, returning to its equilibrium, or equal diffusion, in all parts of its substance, and the prime conductor having lost some of its natural quantity, the threads connected with it lose part of theirs, and so are electrized negatively, and therefore repel each other, by Pr. 3.

Approach the prime conductor with the tube near the same place as at first, and they will close again.—Because the part of their natural quantity of electric fluid, which they had lost, is now restored to them again, by the repulsion of the glass tube forcing that fluid to them from other parts of the prime conductor: so they are now again in their natural state.

Withdraw it, and they will open again.—For what had been restored to them is now taken from them again, flowing back into the prime conductor, and leaving them once more electrized negatively.

Bring the excited tube under the threads, and they will diverge more.—Because more of their natural quantity is driven from them into the prime conductor, and so their negative electricity increased.

Exper. 3.—The prime conductor not being electrified, bring the excited tube under the tassel, and the threads will diverge.—Part of their natural quantity is thus driven out of them into the prime conductor, and they become negatively electrized, and therefore repel each other.

Keeping the tube in the same place with one hand, attempt to touch the threads with the finger of the other hand, and they will recede from the finger.—Because the finger being plunged into the atmosphere of the glass tube, as well as the threads, part of its natural quantity is driven back through the hand and body, by that atmosphere, and the finger becomes, as well as the threads, negatively electrized, and so repels, and is repelled by them. To confirm this, hold a slender light lock of cotton, 2 or 3 inches long, near a prime conductor, that is electrified by a glass globe, or tube. You will see the cotton stretch itself out towards the prime conductor. Attempt to touch it with the finger of the other hand, and it will be repelled by the finger. Approach it with a positively charged wire of a bottle, and it will fly to the wire. Bring near it a negatively charged wire of a bottle, it will recede from that wire in the same manner, that it did from the finger; which demonstrates the finger to be negatively electrized, as well as the lock of cotton so situated.

LII. Extract of a Letter concerning Electricity, from Mr. B. Franklin to Mons. Delibard, inclosed in a Letter to Mr. Peter Collinson, F. R. S. Dated Philadelphia, June 29, 1755. p. 305.

You desire my opinion of Pere Beccaria's Italian book. I have read it with much pleasure, and think it one of the best pieces on the subject, that I have seen in any language. Yet as to the article of water-spouts, I am not at present of his sentiments; though I must own with you, that he has handled it very ingeniously. Mr. Collinson has my opinion of whirlwinds and waterspouts at large, written some time since. I know not whether they will be published; if not, I well get them transcribed for your perusal. It does not appear to me, that Pere Beccaria doubts of the absolute impermeability of glass in the sense I mean it; for the instances he gives of holes made through glass by the electric stroke, are such as we have all experienced, and only show that the electric fluid could not pass without making a hole. In the same manner we say, glass is impermeable to water, and yet a stream from a fire-engine will force through the strongest panes of a window. As to the effect of points in drawing the electric matter from clouds, and thereby securing buildings, &c. which, you say, he seems to doubt, I must own I think he only speaks modestly and judiciously. I find I have been but partly understood in that matter. I have mentioned it in several of my letters, and except once, always in the alternative, viz. that pointed rods erected on buildings, and communicating with the moist earth, would either prevent a stroke, or, if not prevented, would conduct it, so as that the building should suffer no damage. Yet whenever my opinion is examined in Europe, nothing is considered but the probability of those rods preventing a stroke, or explosion; which is only a part of the use I proposed from them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage.

I thank you for communicating M. de Buffon's relation of the effect of lightning at Dijon, on the 7th of June last. In return give me leave to relate an instance I lately saw of the same kind. Being in the town of Newbury in New-England, in November last, I was shown the effect of lightning on their church, which had been struck a few months before. The steeple was a square tower of wood, reaching 70 feet up from the ground to the place where the bell hung, over which rose a taper spire, of wood likewise, reaching 70 feet higher, to the vane or weather-cock. Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimblet hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about 20 feet below the bell. The wire was not thicker than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell.

The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them, except making the gimblet-holes, through which the wire passed, a little larger, and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged, and some stones in the foundation-wall torn out, and thrown to the distance of 20 or 30 feet. No part of the afore-mentioned long small wire, between the clock and the hammer, could be found except about 2 inches, that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, 3 or 4 inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall. These were the effects and appearances: on which I would only make the few following remarks; viz.

1. That lightning, in its passage through a building, will leave wood, to pass as far as it can in metal, and not enter the wood again till the conductor of metal

ceases. And the same I have observed in other instances, as to walls of brick or stone. 2. The quantity of lightning, that passed through this steeple, must have been very great, by its effects on the lofty spire above the bell, and on the square tower all below the end of the clock pendulum. 3. Great as this quantity was, it was conducted by a small wire and a clock pendulum, without the least damage to the building, so far as they extended. 4. The pendulum rod being of a sufficient thickness, conducted the lightning without damage to itself; but the small wire was utterly destroyed. 5. Though the small wire was itself destroyed, yet it had conducted the lightning with safety to the building. 6. And from the whole it seems probable, that if ever such a small wire had been extended from the spindle of the vane to the earth, before the storm, no damage would have been done to the steeple by that stroke of lightning, though the wire itself had been destroyed.

LIII. On the Effects of Lightning at Dorkin in Surrey. By Mr. William Child. p. 309.

Monday, July 16, 1750, a storm arose about 7 o'clock in the evening. During the preceding part of the day the air was of a very red fiery appearance, accompanied with frequent thunderings. About 6 o'clock the wind rose, and blew exceedingly strong, and in a very short time the hemisphere became uncommonly dark; the flashes of lightning were much stronger, and came in very short intervals of time, and the thunder-claps long and loud, attended with a very hard rain for near half an hour, in which time came the strongest flash of lightning he ever saw, and instantly with it the most terrible burst of thunder. Several persons, who were near, saw, at the same time, in different places about Mr. Worsfold's house, large balls of fire, which, as they fell on the houses or ground, divided into innumerable directions.

The lightning entered Mr. Worsfold's house on the south side of the roof, close in a small angle of a stack of chimneys, that stand out several feet above the tiling, and falling perpendicular through the roof, met with a small crank, which was in a passage between the north and south chambers: to which crank hung a bell, and from the crank went a wire both ways into the two chambers. It ran along the wire that went into the back or south chamber, melting it to the end, and when it left it split the post of a bed, that stood in the chamber, as if it had been cleft with wedges. It followed the course of the other wire into the north chamber, which turned towards the east, and went partly round the room, following its direction in every angle where the wire went,* till it reached

* These wires conducting the lightning, as far as they went, confirms Mr. Franklin's opinion, that if they had been extended to the earth, the great damage that ensued might have been prevented. —Orig.

the end, which was joined by a string, to which hung a handle for ringing the bell, it being close by the side of the bed: but the greatest force of the lightning seemed to fall perpendicularly down the side of a wall in the chamber. Against the chimney were hung several barometers, the glasses of which were all shattered to pieces, and forcing away the plastering of the wall, entered the shop, piercing through the two upper shelves, and the parcels of nails, &c. that were on them. And here it is observable, that from the perpendicular course it took the same direction in the shop, as in the chamber over it, but in almost as many lines as there were shelves, leaving very visible marks of its course. Near its perpendicular course in the shop, on one of the shelves, it pierced through 7 box irons, making a small hole about the size of common shot on one side, and leaving a roughness on the opposite side of each box where it came out. The several parcels of nails, tacks, hinges, &c. that lay in the course it took, were very plainly affected by it: some of the small tacks in particular were soldered together, 6, 7, 8, or 10 in a clump, as if scalding metal had run over them. The papers of the parcels were burnt in small holes. At one end of the shelves hung several long pendulums, the springs of which were melted so that they fell to the ground; and the lightning spreading its remaining force to some littered straw and packing paper, that lay about the shop, set fire to them, which was happily extinguished without doing any further damage. Mr. Worsfold was in his shop the whole time, but received no hurt.

LIV. On the great Benefit of Blowing Showers of Fresh Air up through Distilling Liquors. By Stephen Hales, D. D., F. R. S. p. 312.

The great importance of having a sufficient supply of fresh water in ships, has been the occasion of many laudable attempts to make sea-water fresh and wholesome: but all the attempts and discoveries hitherto made have laboured under this great and material objection, viz. the great quantity of fuel that was necessary to distil, with a slow progress, a small quantity of water, by any methods of distillation hitherto known. But Dr. H. had discovered an easy and effectual method to distil great quantities of water with little fuel; which he was led to by the following incidents; viz. Mr. Shipley, secretary of the society for the encouragement of arts, manufactures and commerce, brought him acquainted with Mr. William Baily of Salisbury-court, the author of many ingenious contrivances; who showed him, in a small model of a tin vessel, a method, by which he has happily increased the force of the engine to raise water by fire, viz. by lifting up some of the boiling water, at every stroke, by means of a conical vessel, with small holes in it, full of tow; by which the quantity of the ascending steam was considerably increased. This led him to think, that a greater quantity of liquor might also by this means be distilled; but on trial he found the increase to be only a twelfth part, though considerable in the expanded form of a steam. Hence he was led to try what would be the effect of causing an incessant shower of air to ascend through the boiling liquor in a still; and this he found on trial to be very considerable. There was another circumstance also, which probably conduced to lead him to this thought, viz. About six months before, Mr. Littlewood, a shipwright at Chatham, came to communicate to him an ingenious contrivance, soon to sweeten stinking water, by blowing a shower of fresh air through a tin pipe full of small holes, laid at the bottom of the water. By this means he said he had sweetened the stinking bilge water in the well of some ships; and also a butt of stinking water in an hour, in the same manner as Dr. H. blew up air through corn and gunpowder, as mentioned in the book on Ventilators.

The method which he used to blow showers of air up through the distilling water, was by means of a flat round tin box, 6 inches diameter, and an inch and half deep; placed at the bottom of the still, on 4 knobs or feet half inch high, to make room for the liquor to spread over the whole bottom of the still, that the heat of the fire may come at it. In larger stills this box must be proportionably larger, and have higher feet. And as the mouth of the still is too narrow for the tin box to enter, which box ought to be within 2 inches as wide as the bottom of the still; therefore the box may be divided into 2 parts, with a hinge at one edge or side, and a clasp at the other, to fix it together, when in the still. This box must be of copper for distilling sea-water. The air-pipe, which passes through the head of the still, will help to keep the air-box from moving to and fro by the motion of the ship; or, if that should not be found sufficient, 3 or 4 small struts may be fixed to the sides of the air-box. They must reach to the sides of the still. The cover and sides of the air-box were punched full of very small holes, a 4th of an inch distant from each other, and about the 20th part of an inch in diameter. On the middle of the cover or lid of this air-box, was fixed a nosil more than half inch wide, fitted to receive, to put on, and take off the lower end of a tin pipe, 20 inches long, and passed through a hole in the head of the still: 4 inches of the upper end of this pipe were bent to a crook, almost at a right angle to the upright stem, to unite the crook to the widened nose of a pair of kitchen double bellows, by means of a short leathern pipe of calves-skin. See pl. 15, fig. 8.

The double bellows were bound fast to a frame, at the upper part of the iron nose, and at the lower handle, the more commodiously to work them. And that the upper half of the double bellows may duly rise and fall, to cause a constant stream of air (besides the usual contracting spiral springs withinside), several flat weights of lead must be laid on the upper part of the bellows, near the handle, with a hole in their middle, to fix them on an upright iron pin fastened on the bellows; that by this means the weights may the more commodiously be put on or taken off. For, according to the different depths of the liquor in the still, so will the force of the included air, against the upper board of the bellows, be more or less. Wherever the stills are fixed in ships, the air may be conveyed to them from the bellows, either through a small leathern pipe, distended with spiral coils of wire, or through Bamboo canes, or broad small wooden pipes, like hollow fishing rods. In several distillations of a quart at a time, Dr. H. found the quantity distilled by ventilation to be more than the double of that in the usual way. So that the quantity by ventilation may, at a medium, be estimated the double of the usual distillation. It is the well-known property of moving air, to carry along with it a considerable quantity of adjoining vapour, as also of falling water to carry much air down along with it. It is to be hoped therefore, that so considerable an increase in the quantity distilled will be of great benefit to navigation, as it may be done in less time, and with less fire.

In the account of Mr. Appleby's process, for making sea-water fresh, published by order of the lords of the admiralty, in the Gazette of Jan. 22, 1754, it is said that a still, which contains 20 gallons of water, will distil 60 gallons in 10 hours, with little more than one bushel of coals; and therefore 120 gallons in 20 hours, with little more than 2 bushels of coals. And by ventilation 240 gallons, or a tun; and 24 gallons may be distilled in 20 hours, making an allowance for the times of heating those stills full of cold water; and still a larger and wider will distil a tun in 24 hours; which will more than suffice for a sixty gun ship, with 400 men, whose provision of water for 4 months is about 110 tuns. And larger ships may either have proportionably larger stills, or else two of them. As for merchant ships with few men, a small still will be sufficient.

There are holes in the feet of the iron frame or stove of these stills, to screw them down to the deck. They were fixed at the fore-castle before the mast, in King Charles the 2d's time, when they thought they had discovered the way to distil sea-water, free from the noxious spirit of salt, and from the nauseous bitter taste. Or, if it be thought proper, one part of the ship's boiler may be made use of, by adapting a still-head to it.

Doctor Butler, in his lately published method of procuring fresh water at sea, proposes the pouring in more sea-water into the still, through a funnel fixed in a small hole in the head or upper part of the still, when more than half the former water is distilled off; by which means the water in the still will soon acquire a distilling heat; and this to be repeated several times; but then it will be requisite to add each time more chalk, in such proportion as shall be found requisite. It will be well to try this method in hopes to increase the quantity of water distilled. The hole in the head, or upper part of the still, is to be stopped with a small plate of copper, so fixed as to turn to and from over the hole. Doctor Butler used capital soap-lees, in the proportion of a wine quart to 15 gallons of sea-water, which sufficed for 4 or 5 times repeated pourings-in of more sea-water into the still. But as a small quantity of chalk has the same good effect, and is cheaper, and more easily to be had, it is therefore preferable to soap-lees.

When there is a fire in the cook-room, the sea-water might be ready heated to put into the still, without any additional expence of fuel, in the following manner: about the year 1718, Mr. Schmetou, a German gentleman, got a patent here for heating great quantities of water, with little expence of fuel. Having fixed a spiral iron worm-pipe, in such a brick stove or chimney as women heat their irons in, thus causing the water to run from a vessel, through the worm-pipe, several feet length round, in the fire. About 30 years after, Dr. H. acquainted Mr. Cramond of Twickenham with this, hoping it might be of benefit in distilling sea-water. On which he procured such a spiral iron worm-pipe, about 20 feet long, and $\frac{6}{10}$ inch diameter; the diameter of the spiral coil was about 14 inches. This Dr. H. fixed in a brick stove in his garden, with its upper end fixed to a vessel, which contained 45 gallons of water. He found the event of this first trial to be as follows, viz. When the water ran full bore, at the rate of a gallon in 17 seconds, the heat of the water was found, by a mercurial thermometer held in the stream, at the lower end of the pipe, to be 80 degrees above the freezing point, 180 degrees being the heat of boiling water. When, by means of a turn-cock, a gallon of water was 2 minutes in running, then the heat was 140. At which rate the 45 gallons would be an hour and half in running through the iron pipe; at which rate 25 gallons will run through in 50 minutes, with so considerable a degree of heat; and if it was an hour running, the heat would approach still nearer to a boiling heat, when first put into the still, which would forward the distillation, if wanted.

He pumped the heated water up again into the upper vessel; and thus continued to circulate the heating water, till its heat was 160 degrees in the upper vessel, viz. within 20 degrees, or $\frac{1}{9}$ of boiling, the heat requisite for plentiful distillation. He was in hopes, that if the water in the upper vessel could have been brought to a due degree of heat, and a still-head were fixed on it, with its cooling worm-tub, then water might have been distilled in ships, by having the iron worm-pipe fixed in the chimney of the cook-room: but he found, that when the heat of the water in the upper vessel was 160 degrees; then, in running through the iron worm-pipe again, it was so over-heated as to expand in the pipe, into an explosive vapour, which hindered the running of the water. However he thought it not improper to give an account of this attempt, though it failed.

Now that several effectual means are discovered, to make distilled sea-water wholesome, and also to distil it in much greater quantity in the same still, in the same time, and with nearly the same quantity of fuel; it is reasonable to believe, that it will be of great benefit to navigation, not only in saving much stowageroom, for other important purposes; but also in procuring fresh sweet wholesome water, instead of stinking putrid water, hitherto used; which must needs have a tendency to promote that putrid distemper, the scurvy. And if due care be taken to exchange for fresh air, the putrid close confined air of ships, which has occasioned the death of millions of mankind; then navigation will become more healthy, and with little more danger to health and life than at land, except from storms.

Dr. H. distilled 3 gallons of sea-water, with the proportion of 6 oz. of Mr. Appleby's lapis infernalis, and 6 oz. of calcined bones to 20 gallons of sea-water, as he directs. This water lathered well with soap, and boiled peas well. He distilled also some sea-water with half an ounce of stone-lime to a gallon, from the Clee hills in Herefordshire, which having been preserved 10 months in a firkin, had slackened to dry powder. This distilled water also lathered well with soap, and boiled peas well; which proves that the lime, which is a fixed body, does not distil over with the water. Afterwards General Oglethorpe informed him, that his father, Sir Theophilus, told him, that lime was one of the ingredients, which he and the rest of the patentees, in Charles the second's time, called the cement, with which they made distilled sea-water wholesome. He distilled also some sea-water with the like proportion of powdered chalk, which boiled peas well, and was better tasted than the waters distilled with lapis infernalis, or lime. He distilled also some sea-water with an ounce of chalk to a gallon, but found no difference in the taste of this, and that which had but half an ounce of chalk to a gallon: so that half an ounce of chalk to a gallon of water will be sufficient; but where the sea-water is salter, or more bituminous, more chalk may be added if needful.

Dr. Alston, of Edinburgh, in the preface to the 2d edition of his Dissertation on Quick-lime and Lime-water, says, That "the like effect was found in distilling sea-water with lime; that it neither precipitated a solution of silver in aqua-fortis, nor a solution of corrosive sublimate in water, nor did it form a pellicle of various colours on its surface, as did the water distilled by Mr. Appleby's process." And indeed lime of oyster-shells had the same good effect, but required two distillations, perhaps by using it in too small a proportion. Hence it is probable, that the chalk, the lime, the lime in the lapis infernalis, and the lime in Dr. Butler's soap-lees, seize on and fix not only the bittern salt, but also the bitumen of the sea-water, as we learn from the like effect in the purification of the salt of hartshorn. That the saline spirit arises chiefly from the bittern salt, and not from the more perfect sea-salt, is probable from hence, viz. That in distilled 3 gallons of common water, made as salt as sea-water with common salt; no spirit of salt arose, even though the distillation was carried so far as to leave the salt, though very damp, to lie in heaps, and it was incrusted on the sides of the still, for about 3 inches from the bottom.

It is also a considerable advantage, that water thus distilled by ventilation, being thus replete and freshened with air, has for present use a more agreeable taste than water distilled without ventilation, which requires the standing a longer time to have its more disagreeable adust taste go off. And as the volatile oil of peppermint arises on the wings of the ventilating air during the distillation; so also may that part of the bitumen, which is volatilized by heat; as also the volatile urinous salts of the sea-water, which arise from animal substances, be sublimed in the same manner. It was observable, that the water distilled fast, even though the water in the still was below the surface of the tin airy box, through which the greatest part of the ascending shower of air rushed. Hence the ventilating air, in ascending among the vapours, carries them off fast. Hence it is to be suspected, that this method of ventilation will not do well for simple waters, or fermented vinous spirits; because they being very volatile, much of them may be carried off in waste. It was also observable, that in these distillations of sea-water, no whitish clouds appeared on dropping in solution of corrosive mercury, not even when considerably more than 4 parts in 5 of the water had been distilled over. And it was the same with the mixture of lapis infernalis, lime, and chalk; whence it is probable, that the lime and chalk seize on and fix the more volatile bittern salt, as does also the lime in the lapis infernalis. And it is well known, that sugar, that sweet salt, cannot be made without lime, on which, as its centre of union, it fixes and granulates. And whereas with a solution of silver in aqua-fortis, which was much weakened and diluted with water, there appeared a faint degree of whitish cloud, in all the above-mentioned distillations, though not with the stronger solution of mercury, till the distillation was carried on much beyond 4 parts in 5 of the water in the still; when both solutions caused remarkably white clouds, especially the solution of mercury; which indicates the quantity of the spirit of salt which was raised during the former part of the distillation to be exceedingly small, since it could not seize on, nor disengage the aqua-fortis from the stronger solution of mercury, though it did in a very small degree in the weak solution of silver, so as to let loose a very little of the silver, which thus caused the faint clouds. When a drop of the solution of mercury was dropped into the distilled water, after a drop of the solution of silver, it resorbed the silver cloud, and made the water clear, by means of the great proportion of acid aqua-fortis that was in it.

Now in order to make some estimate of the very small quantity of spirit of salt in these several distilled waters, Dr. H. dropped a drop of the solution of silver into an ounce, or 480 grains of pure rain water, which gave no clouds; but on dropping in a drop of sea-water, which weighed a grain, the white clouds were strong. And since sea-water can dissolve 9 times more salt than it has in it; therefore, supposing the drop to be so fully impregnated with salt, then the salt would be the 480th part of the ounce of water. But as there is 9 times less salt, therefore the proportion of the quantity of spirit of salt will be but the 4320th part. And how much less must be the proportion of salt in these distilled waters, which is not sufficient to make a sensible impression on solution of mercury, and but a faint one on much diluted solution of silver? Such distilled sea-water will not therefore probably be unwholesome; almost all spring-waters have some degree of salt in them: but if there were more of the spirit of salt, a very small quantity of pot-ash, or pearl-ashes, or salt of tartar, combined with it, will turn it into common salt, the quantity of which would be extremely little.

Since double the usual quantity of vapour may by way of ventilation be carried off, common salt may thus be made much sooner, cheaper, and better; because, as there is much less fire used, so proportionably, less of the fine acid spirit of the salt, in which its virtue consists, will be evaporated away: for it is well known that the salt is best, which has undergone the least action of fire in making. This more speedy method of evaporating will also be useful, in making many other evaporations; as in making pot-ash, &c.

LV. On the Great Benefit of Ventilators in many Instances, in Preserving the Health and Lives of People, in Slave and other Transport Ships. By Stephen Hales, D. D., F. R. S. p. 332.

Captain Thomson, of the Success frigate, in a letter to Dr. Hales, dated London, Sept. 25, 1749, says, "that during the ventilation, the lower deck hatches were commonly kept close shut; by which means the air was drawn down into the hold, from between the decks, through the seams of the ceiling, along the timbers of the ship; by which means they found the foul air soon drawn off from between decks. Their rule for ventilating was for half an hour every 4 hours: but when the ventilating was sometimes neglected for 8 hours together, then they could perceive, especially in hot weather, a very sensible difference by that short neglect of it; for it would then take a longer time to draw off the foul air. Their general rule was, to work the ventilators till they found the air from them sweet. All agreed that they were of great service; the men being so sensible of the benefit of them, that they required no driving to work that, which they received so much benefit by. They found this good effect from ventilation, that though there were near 200 men on board, for almost a year, yet he landed them all well in Georgia, notwithstanding they were pressed men, and drawn out of jails, with distempers upon them. This is what he believes but few transports, or any other ships, can boast of; which he imputes to the benefit received by the ventilators. It is to be remarked, that the crew of this ship, which lay wind-bound for 4 months, with the expedition fleet which soon after invaded France, were very healthy all the time, when they were very sickly in all the ships of that expedition. This certainly occasioned all kinds of grain provisions to keep better and longer from weevels than otherwise they would have done; and other kinds of provisions received benefit from the coolness and freshness in the air of the ship, which was caused by ventilation."

Mr. Cramond also informed Dr. H. that he found the good effects of ventilators on board a slave ship of his with 392 slaves, 12 of which were taken on board, just before they sailed from Guinea, ill of a flux, which 12 all died; but the rest, with all the Europeans in the ship, arrived well at Buenos Ayres. And a similar letter, on the good effects of ventilation, &c. was also sent by Captain Henry Ellis, who mentions particularly that in one voyage in the year 1755, not one of 312 slaves died; and all his 36 sailors arrived alive and well at Bristol. Also the Earl of Halifax often informed Dr. H. of the great benefit they found by the use of ventilators, in several Nova Scotia transport-ships, 12 to one more have been found to die in unventilated than in ventilated ships. It is indeed a self-evident thing, that the changing the foul air frequently in ships, in which there are many persons, will be a means of keeping them in better health than not doing it. It is the high degree of putrefaction (that most subtile dissolvent in nature), which a foul air acquires in long stagnating, which gives it that pestilential quality, which causes what is called the jail distemper. And a very small quantity, or even vapour of this highly attenuated venom, like the infection or inoculation for the small-pox, soon spreads its deadly infection.

LVI. Of Some Trials to cure the Ill Taste of Milk, which is occasioned by the Food of Cows, either from Turnips, Cabbages, or Autumnal Leaves, &c. Also to Sweeten Stinking Water, &c. By Ste. Hales, D.D., F.R.S. p. 339.

The above method of blowing showers of air up through liquors, will be of considerable use in several other respects, as well as in distillation, as appears by the following trials, viz.

Dr. H. had been informed, that it is a common practice to cure the ill taste of cream from the food of cows, by setting it in broad pans over hot embers or charcoal, and continually stirring it, till scalding hot, and till cool again. But when he attempted to do this much sooner, and more effectually, by blowing showers of air up through it, he soon found it to be impracticable, by reason of its great degree of frothing up. The ill taste must therefore be got out of the milk, before it is set for cream; which he was told had been practised, and that with some benefit, by giving the milk a scalding heat, without stirring it.

May 22, He ventilated some ill-tasted new unheated milk of a cow, which was purposely fed with crow-garlic mixed with cut grass. After 15 minutes ventilation the taste was a little mended; in half an hour's blowing it was something better. At the hour's end it had the same taste, but was sensibly better than the unventilated milk.

August 23, 4 quarts of ill-tasted new milk, from a cow, which had fed 84 hours on cabbage-leaves only, and drank during that time very little water, were put into a leaden vessel, 8 inches in diameter, and 30 inches deep. The leaden vessel was heated in a large boiler, and set into a vessel of hot water; to give the milk a scalding heat, and also keep it hot. In 10 minutes ventilation it was perfectly cured of its ill taste; and after standing 24 hours in a broad pan, there was a thick scum, which was half cream and half butter, free from any ill taste; the skimmed milk was not sheer or thin: so here is a method to make good butter from ill-tasted milk. The froth of the milk was so great, by reason of a too brisk ventilation, as to make it froth over the vessel, which was 30 inches deep; if it had not been kept down, by constantly lading and breaking the very large bubbles of froth. But when the ventilation is more gentle, the froth has risen but 3 inches from 6 quarts of milk, which was 9 inches deep. The cabbage milk was but 6 inches deep. He repeated the like operation the same day, with the evening milk of the same cow; but giving it only a heat, that he could bear his fingers in, for a little time; with this degree of heat, after 45 minutes ventilation, the milk, though much better tasted, yet was not so completely cured as the former milk. Hence we see how necessary heat is to volatilize the rancid oil (which gives the ill taste) to such a degree as to cause it to fly off by ventilation. It was observed, that what was milked from this cow a week after she had done eating the cabbage, had an ill taste. He had not as yet had an opportunity to try to cure, in the same manner, the ill taste of milk, which is occasioned by cows feeding on autumnal leaves, or turnips.

He ventilated 3 gallons of stinking Jessops well purging water. On first blowing, the smell of the ascending vapour was very offensive, which offensiveness abated much in 5 minutes: in 11 minutes the smell was much better: in 20 minutes the water seemed sweet both in smell and taste; and not sweeter at the end of 45 minutes: 15 or 20 minutes will probably suffice.

July 20th, 3 gallons of stinking sea-water were ventilated; in 5 minutes it was much sweetened, and no ill smell in the ascending air, though at first it was very offensive: at the end of 10 minutes it had a small degree of ill taste; after 20 minutes no ill taste or smell. It frothed near a foot high during part of the ventilation: this from the bitumen, &c.

Some sea-water, which was made to stink with flesh and isinglass being put into it; was not made perfectly sweet, not even by a ventilated distillation, and an hour's more ventilation after it was distilled; so that the putrefaction with animal substances is not easily completely cured by ventilation. When the water was 27 inches deep in the leaden vessel, no air could be blown up through it by the force of the bellows. But at 18 inches depth the air could freely be blown up in showers, through the water; therefore when it is requisite to blow up through great depths of water, the bellows may be worked with a lever, as smiths' bellows.

As it is found by experience, that the milk and butter of cows, which drink stinking water, has a very bad taste, this plainly shows that the water retains its putrid quality when mixed with the blood. Whence it is much to be suspected, that the stinking water, which is drank in ships, by retaining its putrid quality, even when mixed with the blood, may thereby promote that putrid distemper the scurvy, as well as some other distempers. And much more does the putrid close air in ships, which is mixed with the blood from the lungs, promote putrid and other disorders. By the same means also pestilential infections are taken in: for as the salutary properties of good air are conveyed by the lungs, so are also the malignant qualities of bad air. Thus also the putrid water in marshy aguish countries, may be a cause of agues, as well as the putrid air, which they breathe; which, as well as the putrid water, may probably carry some of its putrid quality into the blood through the lungs. This method therefore of sweetening stinking water, by blowing showers of air up through the stinking water of some aguish places, may be beneficial.*

Live fish may well be carried several miles, by blowing now and then fresh air up through the water, without the trouble of changing the water: for this ventilation will not only keep the water sweet, but also enrich it with air, which is necessary for the life of fishes; with which air they supply their blood, by breathing the water, thin spread, between their gills; but stinking water will kill fish. He also found that much of the heating oil may be got out of tar-water, by blowing showers of air up through it when scalding hot, for 15 or 30 minutes, the longer the better; the less volatile and more salutary acid remaining. H.

Explanation of the Figures.—Pl. 15, fig. 8, (oopr) a tin or copper air-box, 6 inches diameter, and an inch and a half deep from (o to p).

The lid of the box full of holes, one 20th inch diameter, and about a quarter of an inch distant from each other. (gikl) a nozel soldered to the lid of the air-box, into which the tin pipe (agikl) is fixed so as to take in and out; this pipe to be 2 feet long, and $\frac{6}{10}$ inch diameter. (ab) a bend in the pipe 5 inches long, to which is fastened the leathern pipe (ccdf) 6 inches long; to which the nose of the bellows is fixed at (df).

Fig. 9, (giklooxx) the lid of the box, whose rim (oxox), is a quarter of an inch deeper than the box (op fig. 8), that the air-holes (o) may be pierced in its upper part; and the lower part is scolloped with wide scollops, for the air to pass through the holes (pp fig. 8.)

* It has been shown of late years by Mr. Lowitz of Petersburgh, that putrid water may be rendered sweet and wholesome by filtration through pulverized charcoal.

Fig. 10, (ab) the milk-boiler, with the broad rim (cd), and perpendicular rim (cedf) soldered to the horizontal rim; the perpendicular rim to enter the circular groove (ef) 4 inches deep full of sand, to prevent the ascent of the smoke from the fire-stove.

LVII. On the Return of the Comet, expected in 1757, *or* 1758. *By T. Barker,* Esq. Dated Lyndon, near Uppingham, Rutland, Dec.* 17, 1754. p. 347.

As we expect the comet of 1531, 1607, and 1682, to return in 1757 or 1758, it is proper to be aware where to look for it. But that will be very different, according to the time of the year it comes; and its period is not sufficiently known to fix the month of its next perihelion, which should be July 25, 1757, according to its last period; but the length of that before would make it Oct. 25, 1758. Mr. B. has therefore, in 12 short tables, given the apparent path of the comet, supposing its perihelion any month in the year, with its curtate distance from the earth; and the first 2 articles of each are the places which it would probably begin to appear in. These will show in general the course of the comet, especially at its first appearance, which is most wanted; but cannot be depended on where its motion is swift, and may be 40° in a day, the beginning of May, or middle of October. From these tables, compared with the scheme, he made another, where the comet would begin to be seen any month in the year.

To construct the places, on a large sheet of pasteboard, he divided the circumference of a circle, of 10 inches radius, into degrees, for the magnus orbis. On the right point of the ecliptic and focal length he drew a parabola like that observed in 1682, round the sun, the centre of the circle, and marked every 4th day's motion from the perihelion, and the line of its nodes. The co-sine of the comet's inclination set off on perpendiculars to this, towards the several points of the parabola, forms the projection of it, or points in the plane of the ecliptic over which the comet is at any time perpendicular.

To find the comet's place at any time, count how long it is before or after its perihelion, and mark the place in the projection of the parabola: lay one edge of a parallel-ruler through that point, and the place the earth is then in, and the other edge passing through the sun, will cut the magnus orbis at the geocentric longitude of the comet: the tangent of the comet's inclination making the perpendicular from the comet's projected place to the line of nodes, the radius is the tangent of its apparent latitude, making the curtate distance of the comet from

* Mr. Barker died at Lyndon, in May 1803, at an advanced age. He was of an ancient and respectable family in Rutland. His father was a celebrated Hebrew scholar, and his mother was daughter of the pious and learned Wm. Whiston, in whose Memoirs may be seen frequent notices of the family. Besides Mr. B.'s regular Annual Registers of the Weather since the year 1771; and several other papers, in the Phil. Trans., he was author of some other separate publications, both on astronomy and theology.

the earth the radius. For expedition thus; draw two lines, making an angle of 17° 56′: on one of them set off the perpendicular from the comet's projected place, and raise a perpendicular to the other; or, which is the same, from the comet's real place in the parabola; and let fall a perpendicular, that is the tangent of the geocentric latitude.

One observation of a known comet will, on such a scheme, determine in some measure its whole course; for, from the earth's place, draw the observed longitude of the comet, where that cuts the projection of the parabola is the comet's place; to which if the observed latitude agrees, it confirms it: then the other data being already known, and one place given, its whole course may be traced. Such a scheme may be also of use to find the periods of comets, where the description of one is not good enough to find its orbit by; for if an old comet was seen in August, in ♒, or in ♋, with south latitude, or very bright in January, it cannot be the comet of 1682; but if in November in ♉, near the ecliptic, it may. It then remains to see, whether the rest of the description will agree with the course it would in that case take; if it does, then, as the account is more or less perfect, there is a greater or less probability of its being the same.

A Table showing where the Comet may be expected to begin to appear any Month.

			Lat.	
January		Scarcely to be seen		
February	end	Retr. between 30° and 15° ♐	Small increasing S.	7 weeks after perihelion.
March	begin	30 and 15 ♑	Small N. or S.	a month after perihelion.
	end	30 and 0 ♒	Small N. decreasing.	
April	begin	15 and 0 ♓	Small N. decreasing.	2 or 3 weeks after.
	end	Stat. 10 ♈ and 20 ♓	Small N.	
May	begin	middle ♈	N.	about perihelion.
	end	Dir. begin. ♉		1, 2, or 3 weeks.
June	begin	begin. ♉	N. increasing	
	end	end ♉		
July	begin	begin. ♊		2 to 5 weeks before.
	end	middle ♊		
August		end ♊		5 to 8 weeks before.
September		Stat. 25 and 30 ♊	Small increasing N.	2 months before perihel.
October		Retr. end ♊	Small S. or N.	2 or 3 months.
	begin	begin. ♊	Small S.	
Novem.	mid.	5 ♊ and 20 ♉		3 months before perihel.
	end	begin. ♉		
Decem.	begin	begin. ♉ end ♈	Small S. or N. very faint	11 to 14 weeks.
	end	begin. ♈		

LVIII. An Extraordinary and Surprising Agitation of the Waters, though without any perceptible Motion of the Earth, having been observed in various Parts of this Island, both Maritime and Inland, on the same Day, and chiefly about the Time that the more Violent Commotions of both Earth and Waters*

* See the note on the letter from R. Philips.—Orig.

so extensively affected many very distant Parts of the Globe;* *the following Accounts, relating to the former, were transmitted to the Society; in which are specified the Times and Places when and where they happened.*

1. *At Portsmouth, in Hampshire. By Mr. John Robertson, F. R. S.* p. 351.

On Saturday, Nov. 1, 1755, about 35 minutes after 10 in the morning, there was observed in the dock-yard at Portsmouth, an extraordinary motion of the waters in the north dock, and in the basin, and at two of the jetty-heads. In the north dock, whose length is about 229 feet, breadth 74 feet, and at that time about 17½ feet depth of water, shut in by a pair of strong gates, well secured, his majesty's ship the Gosport of 40 guns, was just let in to be docked, and well stayed by guys and hawsers. On a sudden the ship ran backwards near 3 feet, and then forwards as much, and at the same time she alternately pitched with her stern and head to the depth of near 3 feet; and by the libration of the water, the gates alternately opened and shut, receding from each other near 4 inches.

In the basin, whose length is about 240 feet, breadth 220 feet, and at that time about 17 feet depth of water, shut in by two pair of gates, lay the Berwick of 70 guns, the Dover of 40 guns, both in a direction nearly parallel to the Gosport; and a merchant ship of about 600 tons, unloading tar, lying in an oblique direction to the others. These ships were observed to be agitated in like manner with the Gosport, and the tar-ship to roll from side to side: the swell of the water against the sides of the basin was observed to be 9 inches; one of the workmen measured it between the librations.

The Nassau, a 70-gun ship, lying along side a jetty head, between the north dock and the basin; also the Duke, a 90-gun ship, lying against the next jetty-head, to the southward, both in a direction nearly at right angles to the others, were observed to be rocked in the same manner, but not quite so violently: these 2 ships lay in the harbour. The dock and basin lie nearly east and west, on the west side of the harbour.

2. *In Sussex, and the Southern Parts of Surrey. By Philip Carteret Webb, Esq., F. R. S.* p. 353.

In his garden at Busbridge, near Godalmin in Surrey, on Saturday the first of November 1755, at half an hour after 10 in the forenoon, Philip Smith, John Street, and John Johnson, the gardeners, were alarmed by a very unusual noise in the water, at the east end of the long canal, near which John Street and John Johnson were then at work. On looking that way, they observed the water, in that part of the canal, in great agitation, attended with a considerable

* This agitation of the waters, observed in various parts of Great Britain, happened on the very same day with the memorable earthquake at Lisbon.

noise. The water soon raised itself in a heap or ridge, extending lengthwise about 30 yards, and between 2 and 3 feet above the usual level of the water; after which the heap or ridge heeled or vibrated towards the north, or left side of the canal, with great force, and flowed about 8 feet over the grass walk on that side of the canal, quite up to the arch. On the water's returning back into the canal, it again raised itself into a heap or ridge in the middle; after which the heap or ridge heeled or vibrated with greater force towards the south, or right hand side of the canal, and flowed over the grass walk, and through the rustic arch on that side; and drove a small stream of water, which runs through it, 36 feet back upwards, towards its source. During this latter motion, the bottom of the canal, on the north side, for several feet in width, was quite bare of water. The water being returned into the canal, the vibrations became less and less, but so strong as to make the water flow several times over the south bank of the canal, which is not so high as the north bank. In about a quarter of an hour from the first appearance the water became quiet and smooth as before. The motion of the water was, during the whole time, attended with a great perturbation of the sand from the bottom of the canal, and with a great noise, likened by the gardeners to that of water turning a mill. During the whole time the weather was remarkably still, there not being the least wind; and there was no tremor or motion of the earth felt on the sides of the canal.

The canal is near 700 feet long from west to east, and is about 58 wide: there is a small spring, which constantly runs through it. The water at the east end, where this appearance was observed, usually pens from 2 to 4 feet, being gradually deeper to the west end, where it pens to about 10 feet. No motion was taken notice of in the water at the west end of the canal, the first vibration, which drove the water over the grass walks, was from south to north. The grass walk on the north side of the east end of the canal is 14 inches, and that on the south side about 10 inches higher than the usual level of the water: the highest part of the walk, over which the water flowed, is about 20 inches above the water-level.

Mr. W. was informed, that the water was affected about the same time in the following places. In a mill-pond, at Medhurst in Sussex, the sudden agitation and swell of the water rolling toward the mill was so remarkable, that the miller imagined a sluice had been opened at the upper end of the pond, and had let a back-water into it; but on search it was found to be shut as usual. Below the mill the swell of the water was so great, as to drive the stream upwards, back into the conduit of the mill. At Lee, in the parish of Whitley, in Surrey, about 5 miles from Busbridge, between Busbridge and Medhurst; the water in a canal or pond belonging to Mr. Luff was so violently agitated, that the gardener, on the first appearance, ran for help, thinking a number of otters were under the water, destroying the fish. In a mill-pond, near Guildford in Surrey, a like swell and agitation of the water was observed by several persons, one of whom stood all the time on a bridge of wood, over the pond. Not the least tremor or motion of the earth was felt in any of these places, or at the bridge at Guildford.

3. *In the Parish of Cobham. By Swithin Adee, of Guildford, M.D., F.R.S.* p. 357.

A man, in the parish of Cobham, was watering a horse in hand, at a pond close by the house, which is fed by springs, and had no current. The time he fixes was about 10 in the morning, but their clock goes too slow. While the horse was drinking, the water ran away from the horse, and moved towards the south with swiftness, and in such a quantity, as left the bottom of the pond bare; then returned with such impetuosity, as made the man leap backwards, to secure himself from the sudden approach of the water. It went back again to the south, with a great swell, and returned again. On inspecting the place, Dr. A. found the water must have risen above 1 foot. The ducks were alarmed at the first agitation, and flew all instantly out of the pond. The man observed, that there was a particular calm at this time of day. You will observe here were two fluxes and two refluxes seen distinctly.

4. *At Medhurst. By Mr. John Hodgson.* p. 358.

As to the ponds near Medhurst, every body agrees, that there was an extraordinary swelling of the water. The water was thrown several feet above its banks, both at north-mill, at south-pond, and the pond in Lord Montacute's park; and at the first of these, on its retreat, left some fishes on dry land.

5. *At Cranbrook in Kent. By Wm. Tempest, Esq. F.R.S.* p. 360.

The people here are very much alarmed on account of an earthquake, which happened last Saturday (Nov. the 1st). I felt nothing of it, but some people fancied they did. I do not hear that the earth moved; only the waters of several ponds, in this and the adjacent parishes, were in such motion, that they overflowed their banks, then returned back, and overflowed the other side.

6. *Near Tunbridge. By John Pringle, M. D., F. R. S.* p. 360.

The pond at Eaton-bridge, near Tunbridge, is about an acre in size, and across it is a post and rail, which is almost quite covered by the water. Some people heard a noise in the water, and imagining something had tumbled in, ran to see what was the matter; when, to their surprise, they saw the water open in the middle, so as that they could see the post and rail a good way down, almost to the bottom, and the water dashing up over a bank about 2 feet high, and perpendicular to the pond. This it did several times, making a great noise. They did not feel the least motion on the shore, nor was there any wind, but a dead calm.

7. *In the River Thames, near Rotherhithe. By Mr. Henry Mills.* p. 361.

Being in one of his barges, unloading some timber, between 11 and 12 o'clock, he was surprised by a sudden heaving up of the barge from a swell of the water, not unlike what happens when a ship is launched from any of the builders' yards in the neighbourhood. After the barge had alternately risen and sunk 3 or 4 times, with a motion gradually decreasing, the water became quiet again.

8. *In Peerless Pool, near Old-street, London. By Tho. Birch, D.D. Secret. R.S.* p. 362.

On the reports, received from several gentlemen, that the agitation of the waters observed in many parts of England, Scotland, Ireland, Holland, &c. on Saturday Nov. 1, 1755, had been likewise noticed in Peerless Pool, near Old-street road, being curious to have as authentic and circumstantial an account as possible of a fact, which he had not heard to have been remarked in any other part of London, or its suburbs, Dr. B. went thither on Saturday Dec. 6, 1755, and took down the following particulars relating to it, from the mouth of one of the two waiters there, who were eye-witnesses of it. He being engaged between the hours of 10 and 11 in the morning, with his fellow-waiter, in some business near the wall inclosing the ground, which contains the fish-pond, and accidentally casting his eye on the water, was surprised to see it greatly moved without the least apparent cause, as the air was quite calm. This occasioned him to call to his companion to take notice of it, who at first neglected it, till being urged to attend to so extraordinary an appearance, he was equally struck with the sight of it. Large waves rolled slowly to and from the bank near them, at the east end, for some time, and at last left the bed of the pond dry for several feet, and in their reflux overflowed the bank 10 or 12 feet, as they did the opposite one, which was evident from the wetness of the ground about it. This motion having continued 5 or 6 minutes, the two waiters stepped to the cold bath near the fish pond, to see what passed there; but no motion was observed in it by them, or by a gentleman who had been in it, and was then dressing himself, and who, on being told of the agitation in the fish pond, went directly thither, with the waiters, and was a third witness of it. On the ceasing of it, they all 3 went to the pleasure bath, between which and the fish pond the cold bath is situated; but they found the said pleasure bath then motionless, but to have been agitated in the same manner with the fish pond, the water having left plain marks of its having overflowed the banks, and risen to the bushes on their sides. The motion in the fish pond had been also observed by some persons in a house belonging to Mr. Kemp, the master of Peerless Pool, situated at a small distance from that pond, and commanding a full view of it.

9. *At Rochford in Essex. By the Rev. Mr. Thomlinson.* p. 364.

At a pond in a close of Mr. Sly's, adjoining to the church-yard, the water was observed to flow a considerable way up the mouth of the pond, and then returning, to flow up the opposite side, repeating this sort of motion for about a quarter of an hour. The motion of the water in the pond was only from east to west, and from west to east, alternately.

10. *In Berkshire, near Reading. By Mr. Rd. Philips.* p. 365.

On the 1st of November last, at about 11 o'clock in the morning, as Mr. Pauncefort's gardener was standing by a fish pond in the garden, he felt a most violent* trembling of the earth, directly under his feet, which lasted upwards of 50 seconds; immediately after which he observed that the water in the pond was in a very unusual motion, and suddenly thrown on the opposite side, leaving that on which he stood quite dry, for the space of 2 yards, and continued in that state for about 2 minutes, when it returned as before, and collecting in or near the middle of the pond, rose about 20 inches above the level of the water on each side. and continued so for 2 minutes in violent agitation, which the gardener described to be like the boiling of a pot.

At the same time Capt. Clarke, at Caversham in Oxfordshire, a mile distant from Reading, was alarmed with a very great noise, as if part of the house had been falling down; on examination however it did not appear that the house was at all damaged; but a vine, which grew against it, was broken off, and 2 dwarf trees, such as are used in espalier hedges, were split by the shock.

11. *Near Reading in Berkshire. By the Rev. J. Blair, LL.D., F.R.S.* p. 367.

At Earley-court, near Reading in Berkshire, in a small fish pond near the house of Edward Pauncefort, Esq. the water was observed, about 11 o'clock in the forenoon, to be in a strong agitation, like that of the tide coming in. The first motion of the water was from the south end of the pond to the north end, leaving the ground or bottom of the fish pond on the south end without water, for the space of 6 feet. It then returned, and flowed at the south end, so as to rise 3 feet up the banks, and immediately went back again to the north, where it likewise flowed 3 feet up the banks; and in the time between the flux and reflux, the water swelled up in the middle of the pond like a ridge, or rising part of the land. This motion or agitation of the water, from south to north, and from north to south alternately, backwards and forwards, lasted about the space of 4 minutes of time; and there seemed to be little or no motion in the direction of east and west, the weather being perfectly calm during the whole time.

* This is the only account that mentions any tremor of the earth to have accompanied the agitation of the waters in this island; and the next account of the very same matter does not take the least notice of any.—Orig.

12. *In Oxfordshire, at Shirburn Castle, the Seat of the Earl of Macclesfield, Pres. R. S. Communicated by his Son, the Lord Viscount Parker, F. R. S. then on the Spot.* p. 368.

On Saturday November 1, a little after 10 o'clock in the forenoon, walking in the garden at Shirburn castle, he perceived the gardener, who was coming towards him by the end of the moat, on a sudden stop short, and look earnestly into the water. He went towards him, and perceived immediately a very strange motion in the water. There was a pretty thick fog, not a breath of air, and the surface of the water all over the moat was as smooth as a looking-glass; yet in that corner of the moat near which he stood, the water flowed into the shore, and retired again successively, in a surprising manner. The flux and reflux were quite regular. Every flood began gently; its velocity increased by degrees, till at last, with great impetuosity, it rushed in till it had reached its full height, at which it remained for a little while, and then again retired, at first gently ebbing, at last sinking away with such quickness, that it left a considerable quantity of water entangled among the pebbles, laid to defend the bank, which run thence in little streams over the shore, now deserted by the water, which at other times always covers it. As the slope of the sides of the moat is very gentle, the space left by the water at its reflux was considerable, though the difference between the highest flood and lowest ebb of these little tides, was but about $4\frac{1}{4}$ inches perpendicular height; the whole body of water seeming to be violently thrown against the bank, and then retiring again, while the surface of the whole moat all the time continued quite smooth, without even the least wrinkle of a wave. He sent persons to several other ponds, in all which the agitation was very considerable. The swells, that succeeded each other, were not equal, nor did they increase or diminish gradually; for sometimes, after a very great swell, the next 2 or 3 would be small, and then again would come a very large one, followed by 1 or 2 more as large, and then less again.

13. *In Devonshire and Cornwall, at Plymouth, Mounts-Bay, Penzance, &c. By John Huxham, M. D., F. R. S.* p. 371.

Saturday, November 1, about 4 p. m. we had (just about high water) an extraordinary boar, as the sailors call it. The sea seemed disturbed about 20 minutes before, though there was very little wind that day, or for some days before. One of our surgeons, who had then just crossed the ferry at Creston, a mile to the south-east of Plymouth, said, that the tide had made a very extraordinary out (or recess) almost immediately after high water (about 4 p. m.) left both the passage-boats, with some horses, and several persons, at once quite dry in the mud, though the minute or two before, in 4 or 5 feet water; in less than 8 minutes the tide returned with the utmost rapidity, and floated both the boats again, so that they had near 6 feet water. The sea sunk and swelled, though in a much less degree, for near half an hour longer. It was said, that at the next morning's tide there were several very large surges. This boar drove several ships from their moorings, and broke some of the hawsers, and twirled the ships and vessels round in a very odd manner. At Crunill-passage, over another arm of the sea, about 2 miles west of Plymouth, the same phenomena were observed; and in Stone-house lake, that communicates with that arm of the sea, the boar came in with such impetuosity, that it drove every thing before it, tearing up the mud, sand, and banks, in a very shocking manner, and broke a large cable, by which the foot passage boat is drawn from side to side of the lake.

You will please to observe, that it happened not here till about 4 p. m.; at Portsmouth, about 11 a. m.; in Holland about 11 a. m.; at Kinsale, &c. in Ireland not till 3 or 4 p. m.

14. *On the Coast of Cornwall. By the Rev. William Borlase, of Ludgvan, A. M., F. R. S.* p. 373.

A little after 2 o'clock in the afternoon, about half an hour after ebb, the sea was observed at the Mounts-bay pier to advance suddenly from the eastward. It continued to swell and rise for the space of 10 minutes; it then began to retire, running to the west and south-west, with a rapidity equal to that of a mill-stream descending to an undershot-wheel; it ran so for about 10 minutes, till the water was 6 feet lower than when it began to retire. The sea then began to return, and in 10 minutes it was at the before-mentioned extraordinary height; in 10 minutes more it was sunk as before; and so it continued alternately to rise and fall between 5 and 6 feet, in the same space of time. The 1st and 2d fluxes and refluxes were not so violent at the Mount pier as the 3d and 4th, when the sea was rapid beyond expression, and the alterations continued in their full fury for 2 hours; they then grew fainter gradually, and the whole commotion ceased about low water, 5¼ hours after it began.

Penzance pier lies 3 miles west of the Mount, and the reflux was first observed there 45 minutes after 2; the influx came on from the south-east, and south-south-east. Here the greatest rise was 8 feet, and the greatest violence of the agitation about 3 o'clock. Newlyn pier lies a mile west of Penzance. Here the flux was observed first, as at the Mount, and came in from the southward (the eastern current being quite spent) nearly at the same time as at the Mount and Penzance, but in a manner somewhat different; it came on like a surge, or high crested wave, with a surprising noise. The first agitations were as violent as any; and after a few advances and retreats at their greatest violence, in the same space of time as at the Mount, the sea became gradually quiet, after it had risen 10 feet perpendicular at least. This is near 5 feet more than at the Mount pier, and 2 feet more than at Penzance. The agitations of the sea at Moushole, another pier in this bay, did not materially differ from those at Newlyn.

In the little harbour of Heyle, about 4 miles north of the Mount on the Severn sea, the agitation did not make its appearance till an hour and a little more after the ebb began, which must be full an hour later than with us. In this inland half-tide harbour it continued visible but an hour and half; the greatest flux was about the middle of that time, the surge being at that time 7 feet high; but in general it rose and fell but 2 feet only, owing probably to the force and quantity of water being broken in its advances into so retired a creek. At Swansea, in Wales, farther up in St. George's channel, where their ebb is later still than in Heyle, the agitation was proportionably later, and was not observed till after 2 hours ebb, near 3 quarters after 6. At Kingsale, in Ireland, more indeed to the north of us, but more open to the Atlantic ocean than Swansea, and farther to the west, the agitation reached not a full hour after us, but above 2 hours sooner than at Swansea; all tending to show, that the force came from the south and south-west.

What relations these little palpitations, or tremulous rebounds of the sea, had to the dreadful convulsions on the coasts of Spain and Portugal, whether they were the fainter parts of that deplorable shock at Lisbon, or the expiring efforts of some similar subterraneous strugglings of nature farther to the west, under the Atlantic ocean, will remain uncertain, till more facts and dates appear; but by the accounts from abroad, this first of November seems to have been a day of universal tremor to all the sea-coasts of the western parts of Europe.

I would not be thought to suggest, sir, that a shock so far off as the coast of Spain could be so immense, as to propagate a motion of the water quite home to our shores. I should rather imagine, that there were several shocks, and some much nearer to us, but all perhaps from one and the same cause diffused in different portions, and permeating more contracted or dilated, but still communicating passages; I should imagine, that this cause affected the seas and land, in proportion to its own force, and the superior or weaker resistance of the incumbent pressure; that where it found the least resistance of all, there it found its vent, and the swell its cure.

Many other similar accounts were also given, as observed both in the sea and inland lakes: as at Swansea, on the coasts of Norfolk and Lincolnshire, &c.; the lakes in Cumberland; a pond near Durham, at half past 10 o'clock; at Loch Ness, Loch Lomond, &c. in the north of Scotland, about 10 o'clock.

It appears also, by communications sent from abroad, that the like agitations of the water were observed at the Hague, Leyden, Harlem, Amsterdam, Utrecht, Gouda, and Rotterdam, and also at Bois-le-Duc; about 11 o'clock on the 1st of November; and likewise at Kingsale and Cork, in Ireland, between 2 and 3 o'clock.

15. *Of an Extraordinary Alteration in the Baths of Toplitz in Bohemia, on the 1st of November,* 1755. *By Father Joseph Steplin, of Prague.* p. 395.

A report being brought that at Toplitz, a village famous for its baths, and 9 Bohemian miles north-west from Prague, the source of these baths had undergone some change, in order to know the truth of this, Father Steplin requested the president of the Supreme Royal Council to send him an exact account of it, in answer to the several questions which he proposed to him. By this means he procured the following: that in the year 762 those baths were discovered; from which time the principal spring had constantly thrown out the hot waters in the same quantity, and of the same quality. On the 1st of November, 1755, between 11 and 12 in the morning, the chief spring cast forth such a quantity of water, that in the space of half an hour all the baths ran over. About half an hour before this vast increase of the water, the spring became turbid, and flowed muddy; and, having stopped entirely near a minute, broke forth again with prodigious violence, driving before it a considerable quantity of a reddish oker, crocus martialis. After which it became clear, and flowed as pure as before; and continues still to do so; but it supplies more water than usual, and that hotter, and more impregnated with its medicinal quality.

16. *Concerning the Agitation of the Waters, Nov.* 1, 1755. *By Mr. De Hondt, of the Hague.* p. 396.

We had at 11 o'clock a phenomenon, which astonished every body. In absolutely calm weather there was observed of a sudden so violent a motion in the water, that the ships were struck against each other, and broke the cables which fastened them. It was felt at the same time at the Hague, Leyden, Harlem, Amsterdam, Gouda, Utrecht, Rotterdam, and Bois-le-duc. At the Hague it was but slight; and no motion was felt in the ground.

17. *On the same. By M. Allamond, Professor of Philosophy at Leyden, and F. R. S.* p. 397.

Between half an hour after 10 and 11 in the morning, in some of the canals of this city, the water rose suddenly on the quay, situated on the south. It returned afterwards to its bed, and made several very sensible undulations, so that the boats were strongly agitated. The same kind of motion was perceived here in the tuns of water of 2 brewhouses, and in those of 3 brewhouses at Harlem. The branches of the Roman Catholic church at Rotterdam, which hung from long iron rods, made several oscillations. A tallow-chandler at the Hague was surprised to hear the clashing noise made by all the candles hung up in his shop.

The accounts brought from Norway inform us, that the same observations were made there, almost at the same time.

LIX. An Account of the Earthquake, Nov. 1, 1755, as felt in the Lead Mines in Derbyshire. By the Rev. Mr. Bullock. p. 398.

The following is an account of the earthquake, which happened at the lead mines on Eyam-edge in the peak of Derbyshire, on Saturday the 1st of Nov. 1755, about 11 o'clock in the forenoon.

Francis Mason, the overseer, says, That he sat in a little room, about 40 yards from the mouth of one of the engine shafts. He felt one shock, which very sensibly raised him up in his chair, and caused several pieces of lime or plaster to drop from the sides of the room. In a field about 300 yards from the mines, there had happened a chasm or cleft on the surface of the earth, which was supposed to be made at the same time he felt the shock; its continuation from one end to the other, was near 150 yards, being parallel to the range of the vein on the north side: the depth of it was about 8 or 9 inches, and its diameter 4.

Two miners say, that at the aforesaid time they were employed in carting, or drawing along the drifts the ore and other minerals to be raised up the shafts. The drift where they were working, is about 60 fathoms, or 120 yards deep, and the space of it from one end to the other upwards of 50 yards. They were suddenly surprised by a shock, which greatly terrified them. They durst not attempt to climb the shaft, lest that should be running in on them, but consulted what means to take for their safety. While they were thinking of some place of refuge, they were alarmed by a shock much more violent than the former; which put them in such a consternation, that they both ran precipitately to the other end of the drift. Soon after they were again alarmed by a third shock; which, after an interval of about 4 or 5 minutes, was succeeded by a fourth; and about the same space of time after, by a fifth; none of which were so violent as the second. They heard after every shock a loud rumbling in the bowels of the earth, which continued for about half a minute, gradually decreasing, or appearing at a greater distance. They imagined, that the whole space of time, from the first shock to the last, was about 20 minutes. They remained about 10 minutes in the mine after the last shock; when they thought it advisable to examine the passages, and to get out of the mine, if possible. As they went along the drifts, they observed, that several pieces of minerals had dropped from the sides and roof, but all the shafts remained entire, without the least discomposure. The space of ground at the mines, wherein it was felt, was 960 yards, being all that was at that time in work.

2. *Account of the Earthquake at Lisbon,* Nov. 1, 1755, in Two Letters from Mr. Wolfall, Surgeon.* p. 402.

Since the beginning of the year 1750, we have had much less rain than has

* This city suffered greatly by an earthquake in 1531.—Orig.

ever been known in the memory of man, excepting the last spring: the summer has been cooler than usual, and for the last 40 days, fine clear weather, but not remarkably so. On the first instant (Nov. 1755,) about 40 minutes past 9 in the morning, was felt a most violent shock of an earthquake: it seemed to last about the 10th part of a minute, and then came down every church and convent in town, together with the King's palace, the magnificent opera-house, joining to it; in short, there was not a large building in town that escaped. Of the dwelling houses, there might be about one-fourth of them that tumbled, which, at a very moderate computation, occasioned the loss of thirty thousand lives. The shocking sight of the dead bodies, with the shrieks and cries of those who were half buried in the ruins, are only known to those who were eye-witnesses. It far exceeds all description, for the fear and consternation was so great, that the most resolute person durst not stay a moment to remove a few stones off the friend he loved most, though many might have been saved by so doing: but nothing was thought of but self-preservation; getting into open places, and into the middle of streets, was the mot probable security. Such as were in the upper stories of houses, were in general more fortunate than those that attempted to escape by the doors; for these were buried under the ruins with the greatest part of the foot-passengers: such as were in equipages escaped best, though their cattle and drivers suffered severely; but those lost in houses and the streets, are very unequal in number to those that were buried in the ruins of churches; for as it was a day of great devotion, and the time of celebrating mass, all the churches in the city were vastly crowded, and the number of churches here exceeds that of both London and Westminster; and as the steeples are built high, they mostly fell with the roof of the church, and the stones are so large, that few escaped.

Had the misery ended here, it might in some degree have admitted of redress; for though lives could not be restored, yet the immense riches that were in the ruins, might in some part have been digged out: but the hopes of this are almost gone, for in about 2 hours after the shock, fires broke out in 3 different parts of the city, occasioned by the goods and the kitchen-fires being all jumbled together. About this time also the wind, from being perfectly calm, sprung up a fresh gale, which made the fire rage with such fury, that at the end of 3 days all the city was reduced to cinders. Indeed every element seemed to conspire to our destruction; for soon after the shock, which was near high water, the tide rose 40 feet higher in an instant than was ever known, and as suddenly subsided. Had it not so done, the whole city must have been laid under water. As soon as we had time for recollection, nothing but death was present to our imaginations. For 1st, the apprehensions of a pestilence from the number of dead bodies, and the general confusion, and want of people to bury them, were very alarming: but the fire consumed them, and prevented that evil. 2d. The fears of a famine were very great; for Lisbon is the store-house for corn to all the country, for 50 miles round: however, some of the corn-houses were happily saved, and though the 3 succeeding days to the earthquake an ounce of bread was worth a pound of gold, yet afterwards bread became moderately plenty, and we were all happily relieved from our starving condition.

The 3d great dread was, that the low villainous part of the people would take an advantage of the confusion, and murder and plunder those few who had saved any thing. This in some degree happened; on which the King gave orders for gallows immediately to be placed all round the city; and after about a hundred executions, among which were some English sailors, the evil stopped. We are still in a state of the greatest uncertainty and confusion, for we have had in all 22 different shocks since the first, but none so violent as to bring any houses down in the out-skirts of the town, that escaped the first shock; but nobody yet ventures to lie in houses; and though we are in general exposed to the open sky for want of materials to make tents, and though rain has fallen several nights past, yet the most delicate tender people suffer their difficulties with as little inconvenience as the most robust and healthy. Every thing is yet with us in the greatest confusion imaginable: we have neither clothes nor conveniences, nor money to send for them to other countries. All Europe is deeply concerned in the immense riches and merchandises that are lost, but none so much as our own nation, who have lost every thing they had here. Few English lives have been lost in comparison of other nations, but great numbers wounded; and though we have 3 English surgeons here, but unfortunately without either instruments, bandages, or dressings, to relieve them. Two days after the first shock, orders were given to dig for the bodies, and a great many have been taken up and recovered. Mr. W. lodged in a house where there were 38 inhabitants, and only 4 saved. In the city prison 800 were lost. 1200 in the general hospital, a great number of convents of 400 in each lost; the Spanish ambassador with 35 servants. It fortunately happened, that the King and the Royal Family were at Belime, a palace about a league out of town. The palace in town tumbled the first shock, but the natives insist that the inquisition was the first building that fell down. The shock has been felt all over the kingdom, but along the se -side more particularly. Faro, St. Ubals, and some of the large trading towns are, if possible, in worse situation than here; though the city of Porto has quite escaped.

It is possible, that the cause of all these misfortunes came from under the western ocean; for a captain of a ship, a very sensible man, told him that he was 50 leagues off at at sea; that the shock was there so violent as greatly to injure the deck of his ship; it occasioned him to think that he had mistaken his reckoning, and struck upon a rock, and they instantly hawled out their long-boat to save themselves, but happily brought the ship, though much injured, into this harbour.

The shocks lasted between 5 and 7 minutes. The very first shock was extremely short, but then it was as quick as lightning succeeded by two others, which, in the general way of speaking, are mentioned all together as only one shock. About 12 o'clock we had a second shock. Mr. W. was then in the Terra do Paço, or King's palace-yard, and had an opportunity of seeing the walls of several houses that were standing, open from top to bottom, more than a quarter of a yard, yet close again so exactly as to leave no signs of injury.

3. *Abstract of Two Letters, by John Mendes Saccheti, M.D., F.R.S. dated from the Fields of Lisbon, on the 7th of November, and the 1st of December,* 1755. p. 409.

The day before the fatal earthquake the atmosphere, and light of the sun, had the appearance of clouds and notable offuscation, and more strong and visible at the actual time of the great shock, which was by undulation, and lasted from 6 to 8 minutes. It ruined not only this populous city, but all the southern part of the country of Estremadura, and a great part of the kingdom of Algarve. The earth opened in fissures in several parts, but neither fire nor visible smoke came out of it. The water in the sea rose several times, and in a few minutes made 3 fluxes and refluxes, rising above the greatest spring-tides 2 spawns, or 15 English feet.

4. *Abstract of a Letter from Mr. J. Latham, dated at Zsu-queira, Dec.* 11, 1755, *to his Uncle in London.* p. 411.

I was on the river on Saturday the 1st of November, with a gentleman going to a village 3 miles off. In a quarter of an hour the boat made a noise as if on the shore or landing. About 4 or 5 minutes after, the boat made a noise as before, which was another shake. We saw the houses tumble down on both sides of the river. In Lisbon, a convent on a high hill fronting the river, the most part of it came down, a great many were killed and buried in the ruins; many tumbled neck and heels in the water, others ran down to the river, up to their middle and necks. A strong northerly wind blew from shore, which covered the water with dust, and in our boat we could scarcely see one another; and it entirely hid the sun from us for some time. The wind soon dispersed the dust, the shaking seemed over. In about three quarters of an hour we came to the village, where we were called ashore, and met several gentlemen, who came out of the city on horse-back, but so frighted, that they did not know what was the matter. In a quarter of an hour after our landing, the village was alarmed with another shake. We got down to our boat; in a moment the river rose so high as obliged us to take to our heels, and run for our lives into the fields and high ground, the water flowing across the road, which, from the low tide, was above a quarter of a mile; the ships were whirled about, and several people taken into the water, others driven ashore and dashed to pieces. From the high grounds we could see the sea at about a mile's distance come rushing in like a torrent, though against wind and tide. A fine new stone quay in Lisbon, where the merchants land their goods, where at that time about 3 thousand people were got out for safety, was turned bottom upwards, and every one lost; nor did so much as a single body appear afterwards. It being a holy-day, great numbers of the natives being at their devotion in convents and churches, whose large buildings suffered most, it is computed about 60 thousand souls, and a hundred and odd of the foreigners, and all sorts of cattle perished. The religious houses being illuminated with wax-lights, and the images dressed, by the shakes were set on fire by night, in several places, and by Monday morning entirely consumed, with the rich furniture of convents, nunneries, and nobility's houses, and all the merchants and tradesmen's goods, besides jewels, gold, plate, and coined money. There have been a great many shakes by nights and days: even on the 8th of December was felt a strong one: it was much more violent in some places than others. The ground was opened; in some places you might put your hand down broad-ways, and not feel the bottom with a long stick. A sea port, called St. Ubal's, was entirely swallowed up, people and all.

5. *Observations made at Colares,* on the Earthquake at Lisbon, of the 1st of November 1755, by Mr. Stoqueler, Consul of Hamburg.* p. 413.

The 1st of November, the day broke with a serene sky, the wind continuing at east; but about 9 o'clock the sun became dim, and about half an hour after we began to hear a rumbling noise, like that of carriages, which increased to such a degree as to equal the noise of the loudest cannon; and immediately we felt the first shock, which was succeeded by a 2d and 3d; on which, as also on the fourth, were seen several light flames of fire issuing from the sides of the mountains, resembling what is observed on the kindling of charcoal. In the spot on which he remained till the 3d shock was over, he observed the walls to move from east to west.

In the afternoon of the 31st of October, the water of a fountain was greatly decreased: on the morning of the 1st of November it ran very muddy, and after the earthquake it returned to its usual state, both in quantity and clearness. Some fountains, after the earthquake, ran muddy, some decreased, others increased, others were dried up; and one, that with the earthquake was

* It is about 20 miles from Lisbon, and lies behind the rock, about 2 miles from the sea.—Orig.

dried up entirely, returned 2 days after to its usual state. In some places where there was no water, springs burst forth, which continued to run. On the spot of Varge, and river of Macaas, at the time of the earthquake, many springs of water burst forth, and some spouted to the height of 25 palms,* throwing up sand of various colours, which remained on the ground. On the hills, numbers of rocks were split, and there were several rents in the ground, but none considerable. On the coast pieces of rock fell, some of them very large, and in the sea sundry rocks were broken: the most noted are those called by the sailors Sarithoes, or Biturecras, of which one was only broken off at the summit, the other all to pieces.

Between these rocks and the main, the coasting vessels sailed at low water; and now you may go to them at low water, without wetting your feet. From the rock called Pedra de Alvidrar, a kind of parapet was broke off, which issued from its foundation in the sea. In a swamp or lake, which received a good deal of water in winter, and was not dry in summer, the earth rose; for there is now scarcely the appearance of a hollow, which was before to the depth of six or seven palms; it now remains even with the adjacent ground. In other places, by the change of the currents it appears that the earth was moved, so that some spots are more elevated, others more depressed than before.

6. *Concerning the Earthquake at Oporto in Portugal, Nov. 1, 1755. By a Letter from that Place.* p. 418.

Saturday Nov. 1st, we had such a terrible earthquake here, that we were afraid of being swallowed up alive, though it has done but very little damage. It began about half an hour past 9 o'clock in the morning, like thunder, or rather the rattling of a coach over stones, and my own house, as well as most other people's, during the first shock, which was a very terrible one indeed, was just as if in a convulsion, which lasted 7 or 8 minutes, and every thing shook and rattled in it all the time, as if it was coming down; which frightened people so much, that a great many ran into the streets, where I plainly saw the earth heave up. At 6 o'clock at night there was another great shock. The river also rose and fell surprizingly every quarter of an hour, for upwards of 4 hours at least, 4 or 5 feet, and sometimes more; and some saw the river in some places open, and throw out a vast deal of wind, which was very terrifying.

Abstract of Two Letters to Mr. Plummer, Merchant in London, from Oporto, concerning the Earthquake felt there. p. 419.

This morning, Nov. 1, 1755, between 9 and 10 o'clock, this city was alarmed with the terrible shock of an earthquake, which continued violently for 5 or 6 minutes, but has done no further damage than the overturning some pedestals

* The Portuguese palm is about 9 inches.—Orig.

from the tops of some churches, and splitting the walls of some old houses. The shock was perceived in the river, among the shipping, by a sudden flux and reflux of the tide, but no damage was done. During the time of the earthquake, and indeed preceding it, was heard a hollow dreadful noise.

Abstract of a Letter from Madrid to the Spanish Consul in London. p. 423.

Nov. 1, soon after 10 o'clock, there was very sensibly felt a great earthquake: according to the common opinion, it lasted 5 or 6 minutes. Every one at first thought that they were seized with a swimming in their heads; and afterwards that their houses were falling. The same happened in the churches, so that the people trod each other under foot in getting out; and those who observed it in the towers were very much frightened, thinking that they were tumbling to the ground. It was not felt by those who were in their coaches, and very little by those who walked on foot.

Of the Earthquake at Cadiz, Nov. 1, 1755, in a Letter from Mr. Benjamin Bewick, Merchant there. p. 424.

Nov. 1, just before 10, the whole town was shaken with a violent earthquake, which lasted above 3½ minutes. The water in the cisterns, which are underground, washed backward and forward, so as to make a great froth upon it. Every body ran out of the houses and churches, in a terrible consternation, but no damage was done, as all the buildings here are excessively strong. An hour after, looking out to sea, we saw a wave coming at 8 miles distance, which was at least 60 feet higher than common. Every body began to tremble; the centinels left their posts, and well they did: it came against the west part of the town, which is very rocky; the rocks abated a great deal of its force. At last it came upon the walls, and beat in the breast-work, and carried pieces of 8 or 10 tons weight, 40 and 50 yards from the wall, and carried away the sand and walls, but left the houses standing, so that only 2 or 3 persons were drowned. Every one now thought the town would be swallowed up; for though this was run off, yet with glasses we saw more coming. When the wave was gone, some parts, that are deep at low water, were quite dry, for the water retired with the same violence that it came with. These waves came in this manner 4 or 5 times, but with less force each time; and about one the sea became more calm, but was still in a boiling motion. Every thing was washed off the mole. The bay was full of barrels, and boats, and timber; but no damage was done to the shipping. The walls have suffered very much. Some of the towns about us have suffered a great deal more than we, by the falling of houses and towers.

Of the Earthquake at Cadiz. By Don Antonio d'Ulloa, F.R.S. p. 427.

Nov. 1, we had here an earthquake, the violence of which was not inferior to that which swallowed up Lima and Callao, in Peru, towards the end of October 1746. It happened in very fine weather, at 3 minutes after 9 in the morning, and continued 5 minutes, and consequently near twice as long as that of Peru, the duration of which was only 3 minutes. If every thing was not destroyed here, it seems particularly owing to the solidity of the buildings. The inhabitants had scarcely begun to recover from their first terror, when they saw themselves plunged into new alarms. At 10 minutes after 11 they saw rolling towards the city a tide of the sea, which passed over the parapet of 60 feet above the ordinary level of the water. At 30 minutes after 11 came a 2d tide; and these 2 were followed by 4 others of the same kind, at 50 minutes after 11, at 12 o'clock 30 minutes; 1 o'clock 10 minutes; and 1 o'clock 50 minutes. The tides continued, with some intervals, till the evening, but lessening. They have ruined 100 toises of the rampart; part of which of 3 toises length, and of their whole thickness, were carried by the torrent above 50 paces. A great number of persons perished on the causey, which leads to the isle of Lesu. Seville has been greatly damaged. St. Lucar and Cheres have likewise suffered much; and Conel is said to be entirely destroyed.

An Account of the Earthquakes that happened in Barbary, inclosed in a Letter from General Fowke, Governor of Gibraltar. Communicated by Philip Lord Viscount Royston, F.R.S. p. 428.

At Tetuan the earthquake began, the 1st of November, at 10 in the morning, and lasted between 7 and 8 minutes; during which space the shock was repeated 3 different times, with such violence, that it was feared the whole city would fall down; but the only damage that resulted was the opening or parting of some of the walls of sundry houses. It was likewise observed that the waters of the river Chico, on the other side of the city, and those of a fountain, appeared very red.

At Tangier, the earthquake began about the same time, but lasted longer than at Tetuan; the trembling of the houses, mosques, &c. was great, and a large promontory of an old building near the city gate, after 3 shocks, fell down to the ground, by which 5 shops were demolished; the sea came up to the very walls, a thing never seen before, and went down directly with the same rapidity as it came up, as far as the place where the large vessels anchor in the bay, leaving upon the mole a great quantity of sand and fish. These commotions of the sea were repeated 18 times, and continued till 6 in the evening, though not with such violence as at the first time. The fountains were dried up, so that there was no water to be had till night: and as to the shore-side, the waters came up half a league inland.

At Arzila, it happened about the same time, but the damage was not great. At the coming up of the sea 7 Moors, who were out of the town walls, were drowned; and the waters came in through one of the city gates very far. The water came up with such impetuosity, that it lifted up a vessel in the bay which, at the water's falling down to its centre again, fell down with such a force upon the land, that it was broke to pieces; and a boat was found at the distance of 2 musket-shots within land from the sea.

At Salle, there happened very great damage, several houses having fallen down. The waters came up with such rapidity, that they came into the city, and at their falling down, great quantities of fish were found in the streets, and many persons were drowned: 2 ferry-boats overset in the river, and all the people on board were also drowned; and a great number of camels, that were just then going for Morocco, were carried away by the waters.

At Fez, vast numbers of houses fell down, and a great many people were buried under the ruins. At the Scloges, a place where the Barbarians live, not far from Fez, a mountain broke open, and a stream issued out as red as blood.

At Mequinez, a vast number of houses fell down, and a great many people of both sexes were buried under their ruins; the convent of the Franciscan friars fell down to the ground, but the friars were saved.

At Saffe, several houses fell down, and the sea came up as far as the great mosque, which is at a great distance from the sea.

At Morocco, by the falling down of a great number of houses many people lost their lives; and about 8 leagues from this city, the earth opened, and swallowed up a village, with all the inhabitants, (who were known by the name of the sons of Busunba) to the number of about 8 or 10,000 persons, with their cattle of all sorts, as camels, horses, horned cattle, &c. and soon after the earth was closed again, in the same manner as it was before.

At Fez and Mequinez, on the 18th of November there happened another earthquake, which was more violent than the first, and lasted till break of day the 19th; during which time great numbers of houses fell down at Fez; many people of both sexes were buried under their ruins; and as to Mequinez, there are but few houses left standing. The people killed by the falling of the houses, besides the wounded, are numberless; and in the part of the town called the Jews' Habitation, only 8 persons were saved.

At Sarjon Hills, one of the hills was rent in two; one side of which fell on a large town, where there was the famous sanctuary of their prophet, known by the name of Mulay Teris; and the other side of the said hill fell down on another large town, and both towns and the inhabitants were all buried under it.

The famous city of Tasso was wholly swallowed up; no remains left.

This last earthquake was likewise felt at Tetuan and Tangier, but without any other damage than that the fountains of Tangier were dried up for 24 hours.

Of the Earthquake in the Island of Madeira, Nov. 1, 1755, in a Letter from Dr. Tho. Heberden, to his Brother Dr. Wm. Heberden, F.R.S. p. 432.

Nov. 1, 1755, in the city of Funchal, on the island of Madeira, at half an hour past 9 o'clock in the morning, was perceived a shock of an earthquake. The first notice was a rumbling noise in the air, like that of empty carriages passing hastily over a stone pavement; immediately the floor moved with a tremulous motion, vibrating very quickly; the windows rattled, and the whole house seemed to shake. The shock lasted a full minute; during which the vibrations, though continual, abated and increased twice very sensibly, in point of force. The noise in the air, which had preceded the shock, continued to accompany it; and lasted some seconds after the motion of the earth had entirely ceased; dying away like a peal of distant thunder rolling through the air. The direction of the shock seemed to be from east to west.

About an hour and half after the shock had ceased, the sea, which was quite calm, was observed to retire suddenly some paces, and rising with a great swell, without the least noise, as suddenly advancing, overflowed the shore, and entered into the city. It rose full 15 feet perpendicular above high water mark, though the tide, which ebbs and flows here 7 feet, was then at half ebb. The water immediately receded again, and after having fluctuated 4 or 5 times between high and low water mark, the undulations continually decreasing, it subsided, and the sea remained calm.

In the northern part of this island the inundation has been more violent, the sea there retiring at first above 100 paces, and suddenly returning, overflowed the shore, destroying or damaging several houses and cottages, forcing open doors, and breaking down the walls of several stores or magazines, and carrying away in its recess a considerable quantity of grain, &c. Great quantities of fish were left on the shore, and in the streets of the village of Machico. All this has been the effect of one sole undulation of the sea, it never flowing afterward so high as high water mark; though it continued fluctuating much longer there, before it subsided, than here at Funchal, as the fluctuation and swell was much greater here than it had been farther to the westward, where in some places it has been hardly, if at all, perceptible.

Another Account of the same Earthquake at Madeira. By Mr. Charles Chambers. p. 435.

This account contains no other particulars than the foregoing.

Of the late Earthquakes of Nov. 1, *and Dec.* 9, 1755, *as felt at Neufchatel in Swisserland. By Mons. De Vautravers, F.R.S.* p. 436.

The dreadful earthquake of the 1st of November last has been perceived even in this country, though very faintly. It turned some of our rivers suddenly muddy, without any rain, and swelled our lake of Neufchatel to the height of near 2 feet above its natural level, for the space of a few hours.

The 9th of this month (Dec.) we felt a much more severe shock of an earthquake. It happened a little before 3 o'clock in the afternoon, with a vibratory motion from west to east; another from east to west, and a third from west to east again. Some chimnies fell in at Cudrefin; the bell in the tower at Morat rung 2 strokes. The shock was severer in lofty places than it was in low grounds. The lake of Morat, immediately after the earthquake, sunk 3 inches, and remains still in the same depression. The same earthquake was felt the same day, at the same hour, at Basil, Berne, Fribourg, Geneva, and all over Swisserland; as likewise at Besançon in France.

Of the Earthquake felt at Geneva, Dec. 9, 1755. By Mons. Trembley. p. 438.

The earthquake of Nov. 1, was felt at Lyons. It is said that the waters retired for some moments at the end of the lake of Geneva; and that a motion was observed in those of the lake of Zurich. On the 9th of this month, (Dec.) a little before half an hour after 2 in the afternoon, in very fine and very calm weather, there was felt here in all the houses in general a very great shock of an earthquake; but it did no damage. The motion was particularly remarked in looking-glasses and windows. Those who were sitting perceived that their chairs shook; and many thought that they were going to fall. The sick felt the motion in their beds. The bells in the rooms of several houses rang. The bell of the clock in the tower of the isle of Rhone rung several times. The motion was felt even on the ground floor of houses. It was felt at Nion, Morges, Lausanne, Berne, Zurich, and perhaps more strongly than here. Three shocks were in fact felt within the space of about a minute. During the first a noise was heard like that of a cart passing over a pavement.

Of the Earthquake felt at Boston in New-England, Nov. 18, 1755. Communicated by John Hyde, Esq. F.R.S. p. 439.

Tuesday, Nov, 18, 1755, about half an hour past 4 in the morning, Mr. H. was awaked by the shaking of his bed and the house; the cause of which he immediately concluded could be nothing but an earthquake, having experienced one before. The trembling continued about 2 minutes. Near 100 chimnies are levelled with the roofs of the houses: many more, probably not fewer than 12 or or 1500 are shattered, and thrown down in part; so that in some places, especially on the low loose ground, made by encroachments on the harbour, the streets are almost covered with the bricks that have fallen. Some chimnies, though not thrown down, are dislocated, or broken several feet from the top, and partly turned round, as on a swivel; some are shoved on one side horizontally, jutting over, and just nodding to their fall: the gable ends of several brick buildings, perhaps of 12 or 15, are thrown down, and the roofs of some houses are quite broken in by the fall of the chimnies: some pumps suddenly dried up; the convulsions of the earth having choaked the springs that supplied them, or altered their course. Many clocks were also stopped by being so violently agitated.

Of the Earthquake felt in New York, November 18, 1755, *in a Letter from Cadwallader Colden, Esq.* p. 443.

A few minutes past 4 in the morning, Mr. C. was awaked with the shock of the earthquake. He plainly heard the noise like that of carts on pavements, going to the eastward, with now and then a noise like the explosion of a great gun at a distance. It was felt about 4 o'clock at Philadelphia, and half after 4 at Boston, and was more violent to the eastward than the westward, and there was an eruption at a place called Scituate, about 20 or 30 miles to the southward of Boston. The summer and autumn had been unusually dry for some days before the earthquake, though the sky was perfectly calm and serene, the air was so light, that the smoke of the town by falling down was offensive to our eyes, as we walked the streets. In the last remarkable earthquake, which happened about 17 years before, and nearly at the same time of the year, the weather preceding it was much the same as now, attended with the falling of the smoke in the town.

Of the Earthquake felt in Pennsylvania, Nov. 18, 1755, *in a Letter to Mr. Peter Collinson, F. R. S.* p. 444.

Abour 4 o'clock this province was pretty generally alarmed with the shock of an earthquake. It gradually increased for 1 minute to such a degree as to open the chamber door, by drawing the bolt of the lock out of the staple. Some people thought they felt its continuance 5 or 6 minutes, but the writer thinks it did not exceed 1, nor was it less. He felt the shock of the 2 earthquakes in England; but they were little in comparison to this.

LX. Of Four Undescribed Fishes of Aleppo. By Alex. Russel, M. D.* p. 445.

Of these fishes Dr. Russel brought the drawings and descriptions from Aleppo.

* Alexander Russel, M.D. was born in the city of Edinburgh about the year 1714; where his father practised the profession of the law with great reputation. After the usual course of grammatical study in the High School at Edinburgh, and afterwards in the University, he was placed with his uncle, an eminent physician in the same city. In the years 1732, 3, and 4, he attended the lectures of the various professors, and having finished his studies, he settled about the year 1740, at the city of Aleppo, where he was greatly esteemed by the English factory. He acquired great celebrity in his profession, and was frequently consulted, not only by the Greek, Armenian, and Jewish inhabitants of that region, but even by the Turks themselves, who are said to have held him in high esteem, and to have placed great confidence in his opinion.

In 1755 he returned to Britain, and settled in London, where he composed his well-known work the History of Aleppo, of which a second edition has lately been published under the care of his brother the late Dr. Patrick Russel, author of the splendid work on Indian Serpents and Fishes. To Dr. Alexander Russel we owe the introduction of the true scammony, as well as that highly elegant shrub the arbutus andrachne into the Botanic Gardens of England. About 1759 he was chosen physician to St. Thomas's Hospital, and was also elected a F.R.S. He attained a very considerable degree of eminence in his profession, and maintained a great integrity of character. He died Nov. 28, in the year 1768.

Fig. 11 and 14, pl. 15, seem to be quite new genera; and 12 and 13, though they belong to the same genus with the mystus, described by Gronovius in his Mus. Ichthyologic. p. 34, N° 83, and p. 35, N° 84, yet are species of that fish which has not hitherto been described.

The fish, fig. 11,* resembles much in shape the Silurus Rondeletii, and has no scales. Its length, from the nose to the tip of the tail, is 20 inches; weight 20 oz.; but they are of different sizes. The head and back are of a black colour. The lateral line runs quite from the head to the tail, on the middle of the side; below which, to the belly, the colour gradually changes into a dark purple; of the same colour is the under part of the head. The head is flat, and near 5 inches in length. The body roundish, till within a few inches of the tail, where it grows flat. The mouth is not so large in proportion as that of the Silurus; it has no tongue, and the structure of the mouth and palate agree exactly with the description of that fish. From the edge of the nostril on each side arises a small cirrus; and from the angles of the mouth 2 others, that are stronger, and twice as long. On the lower lip are 4 more, the 2 external being the longest. The eyes are situated near the corner of the mouth, close on the edge of the upper jaw. The branchiæ are 4 on each side, and all of them have a double row of sharp points, like the teeth of a comb. It has 2 fins, situated near the branchiæ, consisting of 7 radii, to the interior part of which is joined a pretty strong prickly bone: about an inch above the anus are 2 smaller fins. A long fin extends from a little way under the anus to the tail, as another of the same kind does from the neck all along the back: neither of these fins join with the tail, which is round at the tip, and composed of about 22 feathers. It is found in the river Orontes, and in some stagnant waters near it. The markets of Aleppo are plentifully supplied with it, from the month of November till the beginning of March. The flesh is red like beef, and of a rank taste; and, though for want of better, eaten much by the people, yet is esteemed unwholesome. The name it usually goes by is semack al aswad, which signifies the black fish. Its proper name however among the natives is siloor.

The fish fig. 12,† is about 4 inches long. The head is large and flat, the body oblong and compressed. Its colour is mostly of a dark silver. The eyes are large and protuberant. From the lower jaw arise 4 cirri; the longer measure one inch, the shorter 2 thirds of an inch. From the upper jaw arise 2 longer, each measuring 2½ inches, of a firmer texture than either those of the lower jaw, or 2 other small ones placed just by the nostrils. Between the 2 long cirri are 2 small tubuli. The whole of the cirri are of a white colour, excepting the 2 longest, which are of a darkish colour, like the upper part of the head. The

* *Silurus anguillaris.* Linn. † *Silurus cous.* Linn.

fins are 8 in number. Two by the gills, each furnished with a strong saw-like bone. Two small ones near the anus. One of 8 radii, situated half-way between the anus and the tail. One consisting of 7 radii on the back. Another fin, of a membranous and fleshy texture, arises from the middle of the back, and is continued all along to the tail. The tail is forked. It is found in the river Coic at Aleppo, where the fish in general are extremely small, in proportion to those of the same kinds found in other rivers, probably owing to the assiduity of the fishermen. It is called by the natives, zakzuk.

Fig. 13 represents a fish, which in its general form somewhat resembles the above. It is in length 3 inches. The head is rather flatter; the mouth has a more inferior situation, and is in proportion larger than that of the former fish; the eyes much smaller. The cirri, situated as in the other, are 8 in number, but much shorter those that rise from the upper jaw (being the longest) measuring only one inch; they are also flatter at their origin. They both agree in the number of their fins; neither has the saw-like bone in the fin of the back, but only in those near the gills. The fleshy fin of the back is much smaller than in the zakzuk, and rises at a much greater distance from the back fin. The colour is a pale silver marbled with grey; particularly the lower part of the fins and tail. The 2 larger cirri likewise marbled, the others white. These 2 fishes (fig. 12, 13) have no scales, and the palate and other structure of the inside of the mouth is like that of the silurus. This fish is also from the river Coic.

The fish fig. 14* has, on a slight view, so much the appearance of an eel, and, except its not being so fat, eats so like that fish, that though it is much oftener brought to the tables of the Europeans at Aleppo than any other fish found in the river Coic, it has never been suspected of being any ways different from the common eel; and yet on examination it will be found of quite another genus.

The head is long and small. The extremity of the upper jaw runs out to a narrow point, like the bill of a bird; on each side of which, a little distant from the extreme point, are 2 tubuli, or processes. As in the common eel, there are 2 fins at the gills. From the occiput, all along the ridge of the back, small prickles are placed at little distances, resembling the teeth of a saw; these terminate at the origin of a membranous fin, rising about 4 inches from the tail, and is continued, as in the eel, along the lower part of the belly to the anus, at which place are also found 2 or 3 prickles. The colour of the head and back is blackish, variegated with dark yellow spots. The lower belly white, changing gradually into a yellowish cast. The fin of the lower belly, near the anus, is yellow: the other half spotted with black. The length of the fish described was 11 inches.

* Ophidium Mastacembalus. (Gen. Zool.)

LXI. Of a Curious, Fleshy, Coral-like Substance. By Dr. John Albert Schlosser, M.D., F.R.S. With some Observations on it, by Mr. John Ellis, F.R.S.* p. 449.

Having hired some fishermen to dredge, to examine the small English coral, or corallium nostras of Ray's Synopsis, recent in the microscope; the first time they hauled in the dredge, the Dr. discovered a most extraordinary sea-production surrounding the stem of an old fucus teres: it was of a hardish, but fleshy substance, and more than an inch thick, of a light brown or ash-colour, the whole surface covered over with bright yellow shining and star-like bodies, which induced him to believe it to be an undescribed species of alcyonium. He put it immediately into a bucket of sea-water, expecting every moment that the polypes, which he thought lodged in those little stars, would extend and show themselves, like those of the alcyonium, N° 2 of Ray's Synopsis, commonly called dead man's hand, but after more than half an hour's fixed attention, the vessel lying very quiet all the time, he did not perceive the least appearance of any polypes: on which he brought them to shore in the sea-water, and then, by means of the microscope, discovered every one of those stars to be a true animal, and much more beautiful than any polype, but quite of a different structure. Every one of those stars is composed of many thin hollow radii, of a pear-shape form, from 5 to 12 or more in number, all united intimately at their smaller end: every radius appears broad at the extreme part from the centre, and a little convex in the middle of this raised broad part. When the animal is alive, there appears a small circular hole, which contracts and opens frequently. All the radii are of this structure; but their common centre, which is formed by a combination of all the small converging extremities, exhibits an opening of a circular, oval, or oblong figure, forming a kind of rising rim like a cup, which, when the animal is alive and at rest, contracts and expands itself to many different degrees, with great alertness and velocity, though sometimes it remains a great while expanded, or contracted. In all these holes, the central large one, as well as the smaller ones, which last he takes to be the mouths of the animal, he could not perceive any tentacula or claws on the outside; but by looking into them very narrowly, he saw something like very tender little fibres moving at the bottom of their insides. By comparing and examining all the various pieces he had collected of this fleshy substance, with its shining stars, he observed that the size and colour, as well as the very figure of these stars, varied greatly; but the structure of the leaf-like radii, and that of their mouths, and their motions, were perfectly the same in every one individual. Many of these bodies he found so thick and large,

* The substance here described belongs to the genus alcyonium, and is the *Alcyonium Schlosseri* of Linneus.

as to resemble the great branched madrepora coral, especially as they are generally to be met with covering and inclosing the stem and branches of this stiff, ramose fucus. Thus far Dr. Schlosser.

Mr. John Ellis adds the following:

Fig. A, pl. 16, expresses this alcyonium, surrounding the stem and branches of a fucus. I have called it alcyonium carnosum asteriscis, radiis obtusis, ornatum. Fig. B, part of a leaf of the common alga, or sea-grass, with 4 of these starry figures on it. Fig. C, one of the stars magnified. Fig. D represents the fucus, on which it grows, which I cannot find any where described. I have entitled it, in my collection of English fucuses, by the following descriptive name, fucus teres frutescens, germinibus arborum gemmas fructiferas referentibus.

I have had an opportunity lately of examining this curious, fleshy, coral-like figure in the microscope, and find that all the interstices between the stars are filled with eggs of different sizes, each adhering by one end to a very fine capillary filament. The smallest eggs are globular, and as they advance in size, they change to an oval figure; whence they assume the shape of one of the radii of the stars. In several of these stars I have observed a smaller radius, as it were, endeavouring to get into the circle; and notwithstanding their seeming connection in the centre as one animal, I believe I shall soon be able to show you, in a drawing from the microscope, that each radius is a distinct animal by itself.

LXII. Two Singular Cases of Diseased Knee-joints Successfully Treated. The first by Topical Applications; the second by Operation. By Mr. Joseph Warner, F. R. S. p. 452.

The species of tumors here meant, are those which are distinguished by the name of hydrops articuli, or the dropsy of the joint; of which Mr. W. observes, there are 2 different kinds. The one where the disease is situated in the membrana adiposa, and neighbouring parts on this side the capsular ligament. The other is that species of disease, where the fluid is contained within the capsular ligament, between the extremities of the thigh-bone, and the largest bone of the leg. The first species of tumor may be distinguished from the 2d by the touch; from the appearance of the tumor of the first kind, which is pale and uniform; from a want of fluctuation, and from the little or no pain attending it. The repeated use, for some weeks, of emollient fomentations, mercurial frictions, and gentle purges, has often been known to remove this disorder. At other times it has been found, that these applications have had little or no effect, but that the disease has given way to, and been totally removed by the use of perpetual blisters to the part affected; which should, in most instances, be continued for several weeks. At other times Mr. W. has known the Pisselæon Indicum, in English called the Barbadoes tar, to have so good an effect, by being applied every day to the joint for some weeks, even after every other remedy had failed, as to cure such a disorder of the kee-joint, as had hitherto been judged desperate: in which case there plainly appeared to be an enlargement of the bones, as well as a very considerable one of the integuments, and of the tendinous and ligamentous parts, but without any apparent inflammation. In this instance no extravasated fluid could be discovered; however, there was an immobility of the joint, and a considerable contraction of the hamstrings. The pain was extremely great, which the patient described as shooting through the ligaments of the joint, the knee-pan, the extremities of the thigh-bone, and those of the leg. He had a severe symptomatic fever, which had been of many weeks continuance, by which he was become greatly emaciated. The reason for Mr. W.'s giving so particular an account of the circumstances attending this fact, proceeded from his desire of recommending a trial of the same remedy, in the like cases; which, as far as he could judge from his own experience, might always be safely done where there was no degree of inflammation already formed on the integuments.

The 2d species of hydrops articuli, or that where the extravasated fluid is contained within the capsular ligament, may be distinguished from the first, from its deep situation; from the fluctuation which is felt on patting the knee on one side, while the other hand is held immoveably on the opposite side; from the degree of pain arising from the distension, which the capsular ligament suffers, in consequence of its contents; from the incapacity of bending the joint; and from the circumstance of its being attended with no general complaints of body, as well as from the sudden enlargement of the tumor; on the increase of which depends the degree of uneasiness in the part. This is very far from being the case in that kind of disease called the spina ventosa, which arises originally from the medulla and bone itself being diseased; whence proceed grievous pricking and throbbing pains, that usually come on previously to any visible enlargement of the part affected, or any discoverable quantity of fluid deposited in the joint; the difference of which symptoms resulting from the different diseases, is seen from the case which he describes, when it was judged necessary to cut more than once through the capsular ligament, in order to evacuate its contained extravasated fluid; which, contrary to the commonly received opinion of wounds of the ligaments being attended with certain destruction to the limb, should always be done under the like bad circumstances, in reasonable expectation of removing a complaint, which totally disables the patient, and too frequently terminates in the loss of the limb when neglected. And Mr. W. was the more inclined to recommend this practice, as he was convinced that this disease is out of the reach of such applications, as are of service in other diseases of these parts, whose situation is more superficial; that is, on this side the ligament, in which is contained the synovia.

[Then follows a detail of the case, the insertion of which in these Abridgements was deemed unnecessary, after the preceding observations, and account of the successful result of the operation of cutting through the capsular ligament.]

LXIII. Extract of a Letter from Mr. William Pye, dated Manilla, Oct. 1st, 1754, giving some Account of that Place. p. 458.

Manilla is one of the largest of the Philippine islands, and the city is much larger than Oxford; it has 2 universities in it, and is inhabited only by Spaniards. The houses are large, and built very strong; the ground-floor is stone; the walls of a prodigious thickness; all above is wood, and so contrived, that every piece of timber has a connection with each other, all over the house: they are let into one another, and joined together, that the earthquakes, which are very terrible and frequent, may not throw them down. The convents are likewise very strong and handsome. The suburbs are very extensive, and well inhabited.

In the year 1750 there was an earthquake here, which lasted 3 months, with almost continual tremblings, which at last broke out in an eruption, in a small island in the middle of a large lake, all round which the bottom is unfathomable. The third day after the commencing of the eruption, there arose 4 more small islands in the lake, all burning; and about a mile distance from one there is a continual fire, which comes out of the water, where there is no ground, for upwards of 100 fathoms deep. This happened but 4 years ago.

LXIV. An Essay on the Waters of the Holy Well at Malvern, Worcestershire. By J. Wall, M. D. p. 459.

[Reprinted in this Author's Medical Works, with some important additions relative to the chemical analysis of the Malvern-water, by his son Dr. Martin Wall, of Oxford.]

LXV. On the Case of a Man who Died of the Effects of the Fire at Eddystone Lighthouse. By Mr. Edward Spry, Surgeon at Plymouth. p. 477.

On the 4th of Dec. 1755, at 3 in the afternoon, Henry Hall, of East-stonehouse, near Plymouth, aged 94 years, of a good constitution, and extremely active for one of that age, being one of the 3 unfortunate men, who suffered by the fire of the lighthouse at Eddystone, 9 miles from Plymouth, having been greatly hurt by that accident, with much difficulty returned to his own house. Mr. S. being sent for found him in his bed, complaining of extreme pains all over his body; especially in his left side, below the short ribs, in the breast, mouth, and throat. He said likewise, as well as he could, with a hoarse voice, scarcely to be heard, that melted lead had run down his throat into his body.

Having taken the proper care of his right leg, which was much bruised and cut on the tibia, Mr. S. examined his body, and found it all covered with livid spots and blisters; and the left side of the head and face, with the eye extremely burnt; which having washed with linen dipped in an emollient fomentation, and having applied things used in cases of burning, he then inspected his throat, the root of his tongue, and the parts contiguous, as the uvula, tonsils, &c. which were greatly scorched by the melted lead. He ordered him to drink frequently of water-gruel or some such draught; and returning to his own house, sent him the oily mixture, of which he took often 2 or 3 spoonfuls.

The next day he was much worse, all the symptoms of his case being heightened, with a weak pulse; and he could now scarcely swallow at all. The day following there was no change, except that, on account of his too great costiveness, he took 6 drs. of manna dissolved in 1½ oz. of infusion of senna, which had no effect till the day following; when just as a clyster was going to be administered, he had a very fetid discharge by stool. That day he was better till night, when he became very feverish. The next day, having slept well the preceding night, and thrown up by coughing a little matter, he was much better. He began now to speak with less difficulty, and for 3 or 4 days to recover gradually; but then suddenly got worse; his pulse being very weak: his sides which grew worse daily from the first, now reddened a little and swelled; to which Mr. S. applied the gum-plaster. But all methods proved ineffectual, for the next day, being seized with cold sweats and spasms in the tendons, he soon expired.

Examining the body, and making an incision through the left abdomen, Mr. S. found the diaphragmatic upper mouth of the stomach greatly inflamed and ulcerated, and the tunica in the lower part of the stomach burnt; and from the great cavity of it he took out a large piece of lead of the weight of 7 oz. 5 drs. 18 grs. and of the shape of the bottom of the stomach.

It will perhaps be thought difficult to explain the manner, by which the lead entered the stomach: but the account which the deceased gave, was, that as he was endeavouring to extinguish the flames, which were at a considerable height over his head, the lead of the lantern being melted dropped down, before he was aware of it, with great force into his mouth, then lifted up and open; and that in such a quantity, as to cover not only his face, but all his clothes.

A Further Account of the Preceding Case. By Mr. Spry. p. 480.

Some persons having suspected the accuracy of Mr. S.'s statement in the preceding case, from imagining that the degree of heat in melted lead was too great to be borne in the stomach, without immediate death, or at least much more sudden than happened in this case; Mr. S. asserts the fact, not only by his own, and, if requisite, the oaths of others, but also by the following experiments, which from similarity of circumstances must not only render that probable, but in the most convincing manner the absolute possibility of his assertion.

He extracted in 3 pieces, from the stomach of a small dog, 6 drs. 1 scr. of lead, which he had poured down his throat the day before. The mucous lining of the œsophagus seemed very viscid, and the stomach much corrugated, though its internal coat was no-ways excoriated. The dog had nothing to eat or drink after; nor for 24 hours before the experiment, when, being very brisk, he killed him.

He also took from the stomach of a large dog, in several pieces, 6 oz. 2 drs. of lead, 3 days after thrown in. The pharynx and cardia of the stomach were a little inflamed and excoriated; but the œsophagus and stomach seemed in no manner affected. He gave this dog half a pint of milk just before he poured down the lead; very soon after which also he eat of it freely, as if nothing ailed him; which he daily continued to do, being very lively at the time he killed him.

From the crop of a full grown fowl, he in company with Dr. Huxham, F. R. S. extracted of lead one solid piece, weighing 2½ oz. with 9 other small portions, weighing ½ oz. which lead was thrown down the fowl's throat 25 hours before. The fowl was kept without meat for 24 hours, before and after the experiment, eating (being very lively just before they killed him) dry barley, as fast, and with the same ease as before. The mucus on the larynx and œsophagus was somewhat hardened. The external coat of the crop appeared in a very small degree livid; and the internal somewhat corrugated. The barley was partly in the œsophagus, though mostly in the craw, which was almost full with the lead. He took 2 oz. 1 scr. from the crop of another fowl, 3 days after the experiment, which fowl was very brisk to the last.

Allowing, for a further satisfaction, that the experiment be tried, it is requisite in making it, that the melted lead be poured into a funnel, whose spout being as large as the throat of the animal (whose neck must be kept firmly erect) will conveniently admit of, must be forced down the œsophagus, somewhat below the larynx, lest any of the lead might fall in it; and according to the quantity, either by totally, or partly obstructing the aspera arteria, cause immediate, or a lingering death; which accidents happening in his first experiments on 2 dogs, directed him to proceed in the above manner.

He had a dog with lead in his stomach, which he intended to keep, to prove how long he could live.

On the same Case of the Man, who Swallowed Melted Lead. By John Huxham, M. D., F. R. S. p. 483.

Our worthy commissioner, Fred. Rogers, Esq. sent the lead here mentioned, to Dr. H. 3 days after it was said to be taken out of the man (Hall) who was said to have swallowed it. He immediately sent for Mr. Edward Spry, an ingenious young surgeon, of Plymouth, who attended this Hall during his illness, and extracted the lead from his stomach (as was reported) when dead. Mr. Spry solemnly assured Dr. H. that he did actually take the lead, that was sent him, out of the man's stomach, and offered to make oath of it. This Hall lived 12 days after the accident happened, and swallowed several things, solid and liquid during that time; and he spoke tolerably plain, though his voice was very hoarse. And he constantly affirmed, that he had swallowed melted lead.

However, as the story seemed very extraordinary, and not a little improbable, Dr. H. did not chuse to transmit any account of it to the R. S. as he could have wished for more unexceptionable evidence; for Mr. Spry had no one with him, when he extracted the lead, but one woman, Philips, the daughter of Hall, and another woman, who were also in the house, not being able, as said, to see the operation, but immediately called in after it, and Mr. Spry showed them the lead. He sent a very sensible gentleman to inquire into this affair, and he had this account for them.

Mr. Spry was, to the best of his knowledge, a person of veracity, and he thought would not utter an untruth. But, what was more, on Wednesday he brought him a live young cock, into the crop or craw of which, he had the day before poured somewhat more than 3 oz. of melted lead. The cock indeed seemed dull, but very readily pecked and swallowed several barley-corns, that were thrown to him. He had the cock killed and opened in his view, and in the crop they found a lump of lead weighing 3 oz. and some other little bits of lead. He made no doubt the cock would have lived several days longer, if it had not been then killed. There seemed a slight eschar in the cock's mouth, occasioned by the melted lead, and the crop seemed as if parboiled. This experiment is very easily made, and seems to confirm the probability of Mr. Spry's account.

LXVI. A Further Account of the Success of some Experiments of Injecting Claret, &c. into the Abdomen, after Tapping. By Mr Christopher Warrick. p. 485.

The first case in which this experiment was tried, was that of the poor woman at Cubart, mentioned in the Transactions, N° 473, who was injected with claret and Bristol water, and about a week after the operation died suddenly. She was upwards of 50 years of age.

The 2d instance was that of a young woman of St. Kivern, who was about 25, and had been 3 times tapped in the common way. Here they made use of 2 punctures, according to Dr. Hales's method, as recommended in the Transactions, N° 478, and claret and tar-water for the injection; which was conveyed into the abdomen through one canula, while the dropsical lymph passed off through the other. A few hours after she complained of much pain in her bowels, and on drawing off the whole contents at once, she fell into a syncope, in which she remained till about 12 o'clock the next day, when she died. It may not be amiss to mention, that her breath was immediately affected by the tar-water, and the smell of it continued to her death.

The 3d instance being somewhat singular, Mr. W. thought proper to relate it in all its particulars. March, 1752, he was called to Flushing, a small town opposite Falmouth, to attend the tapping of a poor woman, who was about 40 years of age, and laboured, as was imagined, under an ascitical dropsy, occasioned by a suppression of her menses, that happened about a year before. She had been told of his successes with Jane Roman, and desired his assistance, together with Mr. Rice, Mr. Cudlip, and Mr. Lillicrap, of the same profession. She was a married woman, of a chearful temper, had never had a child, and to all appearance was a proper subject for the operation, she being never thirsty, and her extreme parts being of the natural size: the abdomen was likewise evenly and equally distended, and of a great magnitude; but the fluctuation was not altogether so manifest as might have been expected. From these circumstances they made no difficulty to resolve on the operation, and determined to try, at the same time, the efficacy of a subastringent injection. A sufficient quantity therefore of claret and Bristol water being got ready, Mr. Rice made the puncture; but on withdrawing the perforator, instead of lymph, nothing but a thick, ropy, gelatinous fluid came through the canula, in colour resembling red port wine, or rather grumous blood. The singularity of this did not however alter their measures. Two gallons of it were immediately drawn off, and half that quantity of claret and Bristol water injected in its stead. This they proposed to have repeated the next day, and as the circumstances of the patient would admit; and to continue daily, till the whole contents should be gradually discharged; fearing that a total discharge in the ordinary way would have brought on a syncope. But when they attended her again on the day following, not one drop of any fluid came through the canula; and a 2d and a 3d puncture was attended with no better success. Soon after this, the whole abdomen became painful and distended, frequent rigors came on, and a delirium, in about 12 hours, carried her off. On opening the body the day following, not one drop of any fluid was found in the cavity of the abdomen; an enormous cystis, which might have contained, when full, about 6 gallons, having completely filled the whole extent of it. There were likewise attached to the coats of it 5 large bodies of fungus flesh, the least of them larger than a man's fist. Each of these, when cut open, appeared to be divided into cells, full of white glutinous pus. This extraordinary mass adhered only to the fund of the uterus, and together with it, the fungus substances, and vagina, when taken out, entirely covered a middle sized pillar and claw tea-table. They now found, that in the night the canula had accidentally slipped out of the cystis; and that the operator, in making the 2d or 3d puncture, had fallen upon one of these fungous bodies, which gave occasion to the above-mentioned disappointment. On proceeding to a further examination of the abdomen and thorax, they found every thing sound, and in its proper state, excepting the posterior part of the right lobe of the lungs, which was full of purulent matter, and adhered to the pleura. Mr. W. adds, that the ovaria did not distinctly show themselves, so as to satisfy any inquiry about them; but this perhaps might be owing to the hurry or inaccuracy of the dissector.

Whether these miscarriages are sufficient to discredit a method of practice, which has the appearance of being the most rational one yet found out for managing a dropsy, Mr. W. leaves to the determination of better judges. The frequent miscarriages that happen in the ordinary way, seem sufficient to justify every attempt to render the success of it less precarious. If any further trials of it be made, he would beg leave to recommend its being done before the viscera are too much injured by the dropsical lymph; and if the evacuation be made at different times, with a view of preventing a syncope, that brandy, or some such liquor, properly diluted, be made use of instead of claret, which, as he apprehends by the heat of the body, may be apt to turn sour. It may be likewise proper that the head of the patient, during the evacuation, lies lower than any other part of the body.

As in the 2d instance above-mentioned, tar-water had been recommended by some gentlemen of the profession, then present, instead of Bristol-water, Mr. W. some time after the death of his patient, injected a pint of it warm into the belly of a small cur, to see how far the effect of it differed from that of claret and Bristol-water. The dog immediately fell into great agonies, and in about 2 hours died. The abdomen being opened, all the intestines were found greatly inflamed. He then tried claret and Bristol-water, also port wine and fountain water, on other dogs, after the same manner. Each of these injections was retained with little or no inconvenience, except intoxications: and in 48 hours the dogs became well again, the injection being entirely absorbed. It occurred to him in making these experiments, when the power of absorption seemed very considerable, how far it might answer in preventing a syncope, or for other purposes, that a fit quantity of a properly adapted injection be left undischarged, after tapping, which might be either absorbed or drawn off at proper intervals, as the strength of the patient might admit.

LXVII. On the late Discoveries of Antiquities at Herculaneum, &c. in Two Letters from Camillo Paderni, Keeper of the Museum Herculanei. Translated from the Italian by Robert Watson, M. D., F. R. S. Letter 1st, Dated at Naples, June 28, 1755. p. 490.

In April last, a little beyond La Torre della Nunziata, where stood the ancient Pompeii, in digging near the amphitheatre, there was discovered a marble capital of the Corinthian order. On making further trials, there were found 2 pilasters of white marble, about 10 feet high, fluted on every side, with capitals and bases of the Corinthian order. On one side of these pilasters have been found a series of 9 other pilasters, about 7 feet high, equally wrought with the larger: there were likewise 5 other pilasters on the side of the other great one, making in all 16; which are all of one piece, exclusive of the capital and the base, except one, which is composed of 2 pieces. They were all excellently preserved, and were standing; forming a portico before a building. All the buildings, which are in Pompeii, are of the same constitution with those of Herculaneum and Stabiæ; that is, of one story. The portico is continued on the sides, but the pilasters are not of marble, but of brick covered with stucco, and coloured with green, and are not fluted like those of marble. One only of the sides is yet undiscovered, and we must wait to see the side opposite to the front, and the rooms within, to be able to speak decisively.

The front was all painted in the grotesque manner; but little, and that ill preserved, remains. There were no ornaments of stucco, or marble; the walls indeed were coloured, and there were some small niches formed in the walls, each of which corresponded to one of the pilasters, and consequently there were 18 in number. In several of them were found certain figures, some of earth, others of marble, in this order; first was placed one of marble, then one of earth: those of marble were 9 small Hermæ, among which there is a Hercules crowned with oak, some satyrs, fawns and Bacchantes. Two of them are of the old red, and the other of the old yellow marble, and are of an indifferent style. Those of the baked earth consist of 4 figures. The first is a Barbarian king, who stands erect with his right hand under his chin in a pensive manner, and wears his chlamys clasped with a fibula on his right shoulder. But what makes this figure the more curious is, that the whole body forms a vase, on the back of which there is a handle to hold it by. Behind the head there is a little tube, through which water or some other liquor was poured in, and the mouth of the figure is open, through which the liquor was poured out. The height of it is about 10 inches, and the style rather low.

The 2d figure is of the same height and character, as to the workmanship; but what it represents, renders it singular. This figure seems sitting, with its legs stretched out, which are distorted like those of some dwarfs. It has a great head; the mouth, eyes, and nose, of which are extremely overcharged. It is dressed in the prætexta. On the breast is the bulla aurea, the string of which surrounds its neck, and is held with the right hand; with the left it holds the tablettes called pugillares, on which the ancients placed wax, and wrote on it with a style. These pugillares are exactly like those dug up at Herculaneum, and which are preserved in that museum. Besides, it bears a great priapus, and behind is seen the breech. This was made for a vessel, such as that described above, except that besides that the mouth of this figure is pierced, the liquor can also be poured from the priapus.

The third figure is entirely like the preceding, except in its dress, which is rustic, and bound round the waist with a cord, to which is fastened somewhat that cannot be made out, but which appears to be a little case to hold something: the rest is not overcharged, but is rustic. It holds in its right hand a loaf, and its left hand is covered with its dress, and, like the other, it shows its breech and priapus. Probably such vessels were used for drinking the liquor coming out of the priapus, this being not unusual with the ancients, as Juvenal in his second satire, gives us to understand; Vetreo bibit ille priapo.

The last figure represents the Roman Charity. She is sitting, and with her left hand embraces her father, and with her right presses the breast which her father sucks; who is expressed in this figure totally emaciated. This does not, like the others, form a vessel, but simply exhibits the story. The style is moderate, its height near the same as that of the others. This last groupe is covered with a varnish of glazing, like that which covers earthen plates and things of that kind. There were found, in the before-mentioned niches, 2 little busts of baked earth, of the same height; one wants the head. This is all that is found in that part of the building, which is supposed to be the front.

In a little closet, the dimensions of which are about 6 feet in length, and 4 in breadth, discovered the 13th of last month, was found a very fine tripod, about 3 feet high, extremely well preserved. In short, it is one of the most beautiful pieces of antiquity in the whole world. It is formed of 3 satyrs, young, and all exactly alike. Their heads are most beautiful, with a cheerful countenance, and the hair well disposed with a ribband, that surrounds the head. On the forehead stand 2 small horns, which are united. The right hand rests on the side of the body, and the left is open, with the arm somewhat extended. They have a great satyresque priapus. The legs are united, and they place their feet on round bases, which have been turned in a lathe, and then covered with leaf silver. Their tails are twisted round a ring, by which they are suspended. The 3 satyrs support with their heads the hearth of the tripod, which is of excellent workmanship, and has 3 moveable rings, which serve to remove the tripod from one place to another. One of these rings is wanting, and could not possibly be found. Whence we may suppose, that originally it was likewise wanting. On the hearth is another ornament united to its circumference, and forming a kind of radiated crown, which crown has also 2 handles, but not moveable. These serve to place the crown on the hearth. The bottom of the hearth is not of brass, like the rest of the tripod, but of baked earth. The above-mentioned closet, where this tripod was found, is all painted, and entire, with the ceiling unhurt. In the walls of it was a table of white marble, fastened in the wall itself, which might be called a side-board, and which was extended along the sweep of the room. On this table was found a crescent of silver, about 5 inches in diameter, and on the edge of its middle are 2 small holes to receive a string to support it. Perhaps this was an amulet, for we have another of the same metal, but smaller, with its supporter of silver, which has been long found. On the same table was another amulet of silver, about an inch in height, which represents Harpocrates. This figure has its finger near its mouth, the lotus on its head, and wings on its shoulders. On the right shoulder hangs a quiver, and its left arm holds a horn of plenty, and leans on the trunk of a tree, round which is a serpent, and at the foot of the trunk stands an owl. There was found a kind of fibula, which is of gold, and is extremely well preserved. Its form is round, and made like a large button. On the back there is a gold wire fastened to one side; the other end of which is fastened in a small piece of gold, soldered into the fibula. The whole is little more than an inch in diameter. There were found also 2 other figures; one is of marble, about a foot high, representing a woman; it is of no great value; the other is of ivory, but there remains nothing but the name, and a part of the face, by which may be perceived, that it is the work of an excellent Greek hand. All the rest consists as it were of minute leaves, which are so brittle that they cannot be united. Its height is about a foot.

There was also found in the same closet, on the same marble table, one of the most beautiful statues ever seen, and so admirable, that I know not how to begin to describe it. Its height is little more than 3 inches, by which you may conceive what pains have been taken with it. It stands on its feet, and is quite naked, and presents a priapus, which is not satyresque, with a most perfect contrast of attitude. One observes through the whole figure a most perfect skill in anatomy, where the smallest muscle is not lost, and at the same time it seems not dry or hard, but palpable flesh. It is of a noble and excellent stile. Its head is somewhat rustic, with a goat's beard and ears. It has a laughing countenance, turning its head with much grace, and brings its first finger of the left hand to its face. It extends and raises its right arm, which terminates in a manus impudica. Our Neapolitans, and I have seen the same in our peasants about Rome, frequently wear in their hair a pin, the head of which consists of such a hand; and they say, that they wear this against an evil eye; and in Naples some of these pins are worn by children. We have found several of these small hands at Herculaneum. It is observable, that these Priapi frequently had this hand; for among the many which remain under my care, there is one with human ears, and with this hand, which together with the whole arm forms a priapus. The head of the figure is covered with a cap, which is folded down behind; and its base is low and round, and well fitted. In fine this may be called one of the most excellent curiosities. In one of the other rooms there was a fine pair of scales, in which there are some remains of the strings made of a kind of fine coral, and the strings remain in some of the rings. There were found also many vessels of earth and fragments of metal.

In the ancient Stabiæ they go on digging; but it is long since any thing of value has been found there, except that in the beginning of this month 2 small statues of brass were discovered. One represents a Venus, but of no value. The other a Panthea with a rudder, horn of plenty, lotus, modius, and sickle. It is but of ordinary workmanship. Many vases of earth, some of glass, have been found. A great vessel of copper with a handle, a singular funnel, a beautiful little vase of rock crystal with its cover, and a simpulum or ewer; divers medals, as well silver as copper, well preserved, but common, and various pieces of leaden pipes, have also been fonnd there.

The same may be said of Herculaneum; for since the month of March, after the colossal bust of brass was found, they have discovered nothing of value, except one thing, which ought to make much noise among the learned, and which I believe to be the only one of its kind in the world. This is a little leg and thigh of metal covered with silver, and which is 5 inches long. On the external part of it is described a sun-dial formed on a quadrant, and as the thigh forms a quarter of a circle, the workman has taken the centre of this quadrant from the extremity or leg of the ham or gammon, and hence has drawn hour lines, which, with the lines that mark the months, form the usual compartments, some larger and others smaller, which are divided 6 by 6, as well in height as length. Below the inferior compartments, which are the less, are read the names of the months placed in 2 lines in a retrograde order, so that the month of January is the last in the first line, which bears the other 5 following months. In the 2d line are described the 6 other months in their natural order; so that the month of December is under January, and so the months shorter and longer, 2 and 2, have one common compartment for each couple. Almost on the edge of the right side, there is the tail of the animal somewhat bent, and this performs the office of the gnomon. On the extremity of the bone, that is, of the leg, or centre of the quadrant, there is a ring to hold the dial in an equipoise; and it is supposed that in that place was fastened its plummet, such as in the like dials is to fall on the present month, to determine the shadow of the gnomon on the horary lines. It is observable also, that as these dials were described on a plain surface, according to a fixed rule, the surface of this metal ham being in one plane concave, in another convex, one cannot easily guess what rule the workman used to describe a dial of so difficult a kind, on a surface so irregular.

I must not neglect to acquaint you with what has been found in a trial made at Cuma, where were situated some sepulchres, which afforded many curious things. In May last, our miners opened a tomb of the family Pavilia, which formed a small chamber. On the floor were 3 corses, or rather their bones, which were included in 4 pieces of the piperine stone. These 4 stones formed for each corse an oblong case. The engineer, who was present at the discovery, told me, that one of these bodies was all covered by a substance unknown to him; but from his account I comprehended what it was. The corse was covered with a cloth of amianthus, which, as it was large, remained in this situation all on a heap, but calcined by the salts of the earth, for which reason it was necessary to take it up in pieces, it being become extremely brittle. However, to be more sure of my opinion, I had a mind to try it in the fire, where it remained unchanged; whence there is no doubt but that it is amianthus. There were found a great many little pieces of paste as large as beans, which were taken by the miners for comfits but are the confection, which used to be put on dead bodies. They are composed of myrrh and other spices, and even now retain a very strong smell. There was found some cloth reduced almost to nothing, which had some ornament of gold embroidered on it, or rather wove into it, as is more probable from the gold thread. On the above-mentioned body were found some pieces of paper, for I have great reason to think it such from the trials, which I have made on the old papyrus, of which we have about 800 volumes. This paper on one side is coloured with red minium, on the other it is black.

Besides this paper, there were found a mirror of metal, and 3 tesseræ, or dice. Under the corse, or bones, was found a padlock, through which were passed 3 iron strigils, and another that was broken. It is remarkable, that in all the other sepulchres, that were opened at Cuma in the month of May, there were found a mirror, 3 tesseræ, strigils, and some very small fibulæ of bone. In the above-mentioned sepulchre was found a small lectisternium, or rather pulvinar deorum, which was very much decayed. It is mounted in iron. The ornaments which compose it being of ivory, the rust of the iron has as it were destroyed the whole. So that there were collected but a few remains of the 4 pillars, some pieces of the bands, which went round the frame, 8 pieces of ivory, of an oblong form, in each of which was engraved a figure of some unknown deity, all of the same design, but in a bad style; and two heads of a horse, which are fellows, and belong to the lectisternium, not unlike that great one of brass, which is now in the Royal Museum. There were found also several little vases of earthen ware, whose form is this: they have a long neck, with a mouth proportionably straight; the body is oval, which towards the bottom is so small, that they cannot stand upright. The misfortune is, that 2 of these vases, which are of oriental alabaster, and of the most excellent workmanship, are both broken in the middle.

Near this sepulchre there was opened another, belonging to the freed men of the Pavillia family. There we found many glasses and pieces of earthen ware, and two most beautiful earthen lamps. On one of them is a Hercules going to slay a serpent with his club, which he holds in his left hand. On the other is a priestess of Bacchus, which in one hand holds the sacrifical knife, and in the other the half of a victim. There are also 2 very small wine-glasses, which contain, the one a liquor of the colour of red wine, the other a liquor more limpid than white wine, but without any smell. In this tomb were found also the usual dice, strigils, mirrors, and fibulæ. The bones and ashes were in urns made of earth.

Four other sepulchres also have been opened, in all of which were found the usual strigils, mirrors, tesseræ and fibulæ. In one of them was found a little earthen urn with its cover. Within the same tomb was a small urn of glass elegantly made, containing the ashes of a child. Near the said urn were found several little things, which probably were the playthings of the child; these were two very small goblets of baked earth glazed, with a handle to each; two small water ewers, of the same materials, with ornaments; these also are extremely small; another vase of common earth, which forms a recumbent ox, on the back of which is a hole to receive the water, which was poured out through the mouth; and there is a handle on one side of the body. In this same sepulchre was found a monstrous priapus of red earth. This figure has wings, and is much overcharged. All these things, which I have described, are preserved by me in the Royal Museum, in a separate apartment from that in which is preserved what has been found at Herculaneum, Pompeii, and Stabiæ. I have already filled 8 chambers with antiquities; and because those are not sufficient, I shall begin to place many other things, which hitherto I have been forced to keep in confusion in other chambers, which are on the same floor. A single volume of the Papyrus is unfolded, being that which treats of music. At length the name of the author, who was called Philodemus, is found written twice, at the end of the piece. The name is written once in a small, and a second time in a large hand, and in a good Greek character. They are now beginning to open, or rather to unroll another manuscript; but hitherto without much success; from some fragments we may collect that it treats of rhetoric.

Dr. Watson makes the following Observations on the preceding.

I think it probable, that Philodemus, the author of this treatise on music, was the Epicurean philosopher of that name, who was, as Strabo informs us, a native of Gadara in Syria. He wrote many pieces in prose and verse, and his 10th book, περι των φιλοσοφων συνταξεως, is quoted by Diogenes Laertius. Indeed his sect, time, and abode, will allow of the supposition of his writings on music being at Herculaneum at the time of its destruction. He resided at Rome, and was the acquaintance of Tully, and the preceptor of Lucius Piso the consul. We learn from Asconius Pedianus, that it is Philodemus the Epicurean, of whom Cicero speaks with that admirable mixture of praise, and invective, and excuse, in his oration against Piso; where he says, that he knew him to be a man of elegance and polite literature; that it was from him that Piso learned his philosophy; which was, that pleasure ought to be the end of all our pursuits; that indeed the philosopher did at first divide, and distinguish the sense in which that maxim was to be understood; but the young Roman perverted every thing to make it favour his inclinations and pleasures; and the Greek was too polite and well-bred to resist too obstinately a senator of Rome. He then tells us that Philodemus was highly accomplished in philosophy, as well as polite literature, which other Epicureans were apt to neglect; that he wrote verses, which were so sweet, so elegant, and so charming, that nothing could exceed them; that he was betrayed into a too hasty friendship with Piso, from which he could not disengage himself without the imputation of inconstancy, and that, rogatus, invitatus, coactus, ita multa ad istum de isto scripsit, ut omnes libidines, omnia stupra, omnia cænarum conviviorumque genera, adulteria denique ejus, delicatissimis versibus expressit.

I have met with some epigrams of Philodemus yet extant, some of which are, in my opinion, most facetious, and elegant. We might have had many more, had not Planudes, as the scholia inform us, rejected such out of his collection, as he thought too loose and voluptuous. Horace seems to have had some of these epigrams in his eye more than once, when he wrote his 2d satire of the first book; particularly where he says,

—— hanc Philodemus ait; sibi, quæ neque magno
Stet pretio, neque cunctetur, cum est jussa venire.

Is not this almost a translation of the

και παρεχϐσα
Παντα, και αιτησαι πολλακι φειδομενη.

I will give the whole epigram, as a specimen of the style and manner of Philodemus; but must beg, that in reading the third verse you would recollect what Homer says of the girdle or cestus of Venus, that it contained all kind of delights and blandishments, love, persuasion, and desire.

Φιλοδημϐ επιγράμμα.

Μικκη και μελανϐσα Φιλαινίον, αλλα σελινων
Ουλοτερη, κ' αμνϐ χρωτα τερεινοτερη,
Και κεστϐ φωνευσα μαγωτερα, και παρεχϐσα
Παντα, και αιτῆσαι πολλακι φειδομενη.
Τοιαυτην στεργοιμι Φιλαινιον, αχρις αν ευρω
Αλλην, ω χρυσεη Κυπρι, τελειοτερην.*

Extract of the second Letter from Camillo Paderni, dated at Naples, July 29, 1755. p. 507.

A cameo of great excellence was found the 9th of this month. This cameo is in alto relievo. It is about an inch and a half long, and almost as much in breadth. It represents a half length of Ceres. The head is in profile, and has a noble and beautiful air. It is turned, together with the body, a little to the left. The left arm is a little raised, and holds in the hand some ears of corn. The right arm is lower, and close to the body. The right hand takes hold of part of a fine garment, or shift, with which the figure is in part covered. The head is adorned with a diadem; and the hair, which is of excellent workmanship, flows on her shoulders, tied with a single ribband, which rests on her neck. The stone, of which the head is composed, is pellucid, and the rest of the figure is cut out of a chalcedony by a Greek master; it was found at Stabiæ, where they continue to dig. In the same place were found also buried several vases of metal and glass very well preserved.

At Pompeii within these few days was found a most beautiful wine-strainer, small, but finely pierced, in a better taste than those already found, which are of brass. In this same place was dug up an ink-standish, with some of the ink, which I likewise preserved. There has been met with also an iron ax. There have been found, and they go on daily to find, many pictures. If the ancients had not dug in this place, we should have discovered many more things; for we find that they have taken away even some of the pictures.

* Since the death of the learned Dr. Watson, which happened March 2, 1756, soon after his translation of these two letters of Camillo Paderni, and his observations on the former, were read at the Royal Society, another epigram of Philodemus has been taken notice of, published at Leipsic in 1754, by the celebrated Mr. Reiske, which appears likewise to have been alluded to by Horace in the passage in part cited above from his second satire of the first book, ver. 120.—Orig.

LXVII. Of the Earthquake felt at Glasgow and Dumbarton; also of a Shower of Dust falling on a Ship between Shetland and Iceland; in a Letter from Dr. Robert Whytt, Prof. of Medicine in the Univ. of Edinburgh. p. 509.

The earthquake at Glasgow and Greenock happened in the night between the 30th and 31st of December, nearly at the same time. It was felt at Glasgow by almost every person that was awake, and out of bed, and also by some in bed, and who were not fast asleep. There were 3 successive shocks, or risings as it were of the earth. It was felt not only at Glasgow and Greenock, but also at many other places in the neighbouring country; particularly at Dumbarton.

By letters from a passenger on board a ship bound from Leith for Charlestown in South Carolina, it appears that on the night of the 23d or 24th of October last, when the weather was quite calm, a shower of dust fell on the decks, tops and sails of the ship, so that next morning they were covered thick with it. The ship at this time was between Shetland and Iceland, about 25 leagues distant from the former, and which was the nearest land. This shower was probably owing to the great eruption, which happened at mount Hecla in Iceland, in October.

LXVIII. Extract of a Letter from Mons. Bonnet, F.R.S. Dated at Geneva, Jan. 30, 1756, *concerning the Earthquake on the 14th of November,* 1755, *in Valais in Swisserland. Translated from the French.* p. 511.

Valais is thought to have been more shaken by the earthquake than our city and its neighbourhood. The earthquake felt here, happened Nov. 14, at 3 in the afternoon. It proceeded from the north, and lasted a minute. The earth opened on the mountain; and the opening was large enough to thrust one's hand in, and no bottom can be found. In another part of the mountain the earthquake opened a spring sufficient to turn 2 mills. The earth has been opened in another place. The opening is round, and no bottom can be discovered. The earth continues to shake almost every day, but these shocks are much gentler than the first.

LXIX. Extract of a Letter from Mons. Allemand, F.R.S. Translated from the French. Dated Leyden, Jan. 27, 1756. p. 512.

On the night between the 26th and 27th of the last month, December 1755, between 11 and 12 o'clock at night, there was a considerable earthquake on the frontiers of this country. It was felt at Liege, Maestricht, Nimeguen, Arnheim, and Breda. There were 3 different shocks, the last of which happened at about 4 in the morning, but without any noise or accident. I have been informed by letters from Swisserland, that several shocks were felt there, and that the salt-springs of Bevieux have been rendered more salt. At Amersfort, in the province of Utrecht, on the 15th of this month, was felt a shock of an earthquake, which occasioned great consternation, but no damage.

LXX. Of some Fungitæ and other Curious Coralloid Fossil Bodies. By Thomas Pennant, Esq.* p. 513.

Fig. 1, pl. 16, was found in the lime-stone quarries in Coalbrooke-dale, Shropshire, the greatest magazine of coralloid fossils that I am acquainted with. The length of this elegant body is equal to that drawn, and its greatest diameter, which is near the top, is about an inch and a half. It is exactly of the form of a pear, with a small portion of stalk remaining; and its whole surface is

* Thomas Pennant, Esq. was born in Flintshire in the year 1726. His father was a gentleman of good family and independent fortune.

Mr. Pennant has himself given us the chief particulars of his life in a small work which he pleasantly chose to write in the character of his own shade: it is entitled "The Literary Life of the late Thomas Pennant, Esq." In this publication he informs us that his zeal in the pursuit of Natural History was first excited by a present of Willughby's Ornithology, which was made to him by a relation, when he was about 12 years of age. In 1754 he was elected a Fellow of the Antiquarian Society, and in 1767 a F.R.S.; having distinguished himself by his ingenious and useful work the British Zoology, and other scientific publications. The British Zoology was at first undertaken for the benefit of a Welsh school, but the splendid nature of the work in its folio form seems to have operated to its disadvantage as an affair of profit, and it was never continued on a similar scale, but was republished in 4to, in which state it is too well known and esteemed to require particular description. In 1757 Mr. Pennant was, at the instance of Linnæus himself, made a member of the Royal Academy of Sciences at Upsal, and he continued to correspond with Linnæus till the age and infirmities of that illustrious naturalist obliged him to desist. In 1765 Mr. Pennant travelled into France, where he passed some time with the celebrated Count de Buffon. He went into Switzerland, where he commenced an acquaintance with Haller, and at Zurich with the Gesners, one of whom was the descendant of the famous Conrad Gesner. He then visited Holland, and at the Hague found the celebrated Dr. Pallas, with whom he ever after maintained a constant correspondence on subjects of natural history. In the midst of these his reigning pursuits he never neglected the company of convivial friends, or shunned the society of the gay world. Mr. Pennant lived some years after the publication of his Literary Life, during which time he still pursued, with as much assiduity as his increasing infirmities would permit, his usual course of study, and died at his seat at Downing in Flintshire in the year 1798. It remains to add, that Mr. Pennant's person was elegant, his manners in the highest degree polished, and what is of infinitely more importance, that his character was equally estimable.

The publications of Mr. Pennant are numerous, and are remarkable for variety of information, which is generally detailed in a very entertaining manner. His tours in Scotland, Wales, &c. are held in great esteem. His Indian Zoology contains descriptions, accompanied by plates, of a few of the rarer Indian animals, but was never continued to any farther extent. His "Outlines of the Globe," a vast work, has as yet been only published in part: of this the "Arctic Zoology" can hardly be too much commended: the parts relative to India, New-Holland, and some other regions have also appeared; and it is greatly to be wished that the whole of a work so much abounding in general as well as zoological and geographical information should at length be presented to the public.

covered with small shallow polygonal cells, the stalk excepted, which is perfectly smooth.

Fig. 2 is a small fungites from the same place, of the same size with the figure; the top is convex, and thick set with minute circular cavities; the stalk tends to a conoid form, and is coarsely striated lengthways.

Fig. 3 has a very deep cup-like cavity in it, the bottom of which is very finely radiated; the remaining part covered with small tubera, not unlike those that sometimes are seen in the insides of flints and pebbles. Externally it is irregularly cellular, but the stalk is striated.

Fig. 4 is a very singular body, and the most remarkably shaped fungites I ever saw, being exactly oval on one side, and flat on the other, without the least appearance of stalk. The oval or lower part is reticulated with polygonal cells, like fig. 1. The flat or upper part is striated semicircularly, the striæ passing from one side to the other, and then reverting.

Fig. 5 he received out of Italy, under the name of lapis subluteus Veronensis stellis majoribus. The surface is finely marked with star-like cells, which are elegantly striated from their centre; and their edges rise a little prominent. The lower part of this stone is of a conoid shape, and irregularly indented with coarse circular rugæ.

Fig. 6 was found at Coalbrooke-dale; is of a white colour, and very smooth both on the sides and top, without any appearance of striæ: but what renders this very singular, is the remarkable thinness, its greatest diameter not exceeding the 8th of an inch.

Fig. 7 was found at the top of one of the highest mountains in this county, near Caer-gwrle, in a reddish loamy soil, with various other diluvian remains.

It is of a conoid shape, but considerably incurvated; the sides are striated lengthways, and likewise circularly, but the circular striæ are much less frequent than the others. At the thicker end there appears to have been a deep cup-like cavity, the greatest part of which had by some accident been destroyed, but what remains is radiated with thin and very prominent ridges placed at equal distances from each other. On one side is a small flat fungites.

Fig. 8 is a fungites from Coalbrooke-dale, seemingly formed of 3 or 4 smaller, inserted one into the other. It has the same cavity on the top as the former, with a minute striated concha anomia in it. Fig. 9. This fungites is almost straight; has a small cup-like striated cavity on the upper end; is encompassed with prominent ridges on the sides; and is striated lengthways. Fig. 10. This species came from Piedmont, and differs from all the rest. It may be called an echinated fungites, having 6 orders of sharp-pointed studs running lengthways from top to bottom, and between each order appear some very minute longitudinal striæ. The upper part, instead of a cavity, is composed of several thin lamellæ rising above the sides. Fig. 11 is a Coalbrooke-dale production, and is a cluster of fungitæ, though only 2 appear in the figure. This varies from some of the foregoing in the shape of its head, in the middle of which is a shallow circular cavity, its sides rising a little prominent, and the striæ, which commence the inside, pass over the ridge, and are continued to the edges. Fig. 12 is from the same place. The cup-like cavity in this is pretty deep, and radiated with deep strigæ: and the sides are marked with very distinct ridges running lengthways, though sometimes interrupted by circular furrows.

LXXI. An Account of Inoculation, by Sir Hans Sloane, Bart. given to Mr. Ranby to be published, Anno 1736. p. 516.

Sir H. S. had heard by several reports from China and Guinea, but especially from Turkey, of the inoculation of the small-pox; and took an opportunity, when Dr. Wm. Sherrard was English consul at Smyrna, to desire the favour of him to inform him of the truth and success of it. In answer to which he told him, that the consul from Venice residing there, a physician, Dr. Pylarini, had taken particular notice of that practice, and had promised to satisfy him about it; which he did by a letter, which was printed in the Phil. Trans. in 1716, and he believed at Venice.

This notice lay dormant till Mr. Wortely Montague, (then ambassador from England at the Porte) and the Lady Mary had inoculated their son at Constantinople, and wrote about this practice, and the advantages of it, to the court and their acquaintance here, and afterwards brought into England their inoculated son, in perfect health.

The princess Anne, then princess royal of Orange, falling ill of the small-pox in such a dangerous way that her life was doubtful, the late Queen Caroline, when princess of Wales, begged the lives of 6 condemned criminals, who had not had the small-pox, in order to try the experiment of inoculation upon them. But Mr. Maitland, who had inoculated at Constantinople, declining for some reasons to perform the operation, lest it should be lost, Sir H. wrote to Dr. Terry at Enfield, who had practised physic in Turkey, to know his opinion and observations about it; who returned him this answer, that he had seen the practice there by the Greeks encouraged by their patriarchs; and that not 1 in 800 had died of the operation. On his speaking to Mr. Maitland, he undertook the operation, which succeeded in all but one, who had the matter of the small-pox put up her nose, which produced no distemper, but gave great uneasiness to the poor woman. After their recovery, in order to obviate the objection made by the enemies of this practice, that the distemper produced by it was only the chicken-pox, swine-pox, or petite verole volagere, which did not secure persons against having the true small-pox, Dr. Steagertahl, phvsician to the late king, and Sir H. joined their purses to pay one of those who had it by inoculation in Newgate, who was sent to Hertford, where the disease in the natural way was epidemical and very mortal, and where this person nursed and lay in bed with one, who had it, without receiving any new infection.

To make a further trial, the late queen Caroline procured half a dozen of the charity children belonging to St. James's parish, who were inoculated, and all of them, except one (who had had the small-pox before, though she pretended not, for the sake of the reward) went through it with the symptoms of a favourable kind of that distemper.

On these trials, and several others in private families, the late queen, then princess of Wales, (who with the king always took most extraordinary, exemplary, prudent and wise care of the health and education of their children) sent for Sir H. to ask his opinion of the inoculation of the princesses. He told her royal highness, that by what appeared in the several essays, it seemed to be a method to secure people from the great dangers attending that distemper in the natural way. That the preparations by diet, and necessary precautions taken, made that practice very desirable; but that not being certain of the consequences which might happen, he would not persuade nor advise the making trials on patients of such importance to the public. The princess then asked him, if he would dissuade her from it: to which he made answer, that he would not, in a matter so likely to be of such advantage. Her reply was, that she was then resolved it should be done, and ordered him to go to the late King George the first, who had commanded him to wait upon him on that occasion. He told his majesty his opinion, that it was impossible to be certain but that raising such a commotion in the blood, there might happen dangerous accidents not foreseen: to which he replied, that such might and had happened to persons, who had lost their lives by bleeding in a pleurisy, and taking physic in any distemper, let ever so much care be taken. Sir H. told his majesty he thought this to be the same case, and the matter was concluded on, and succeeded as usual, without any danger during the operation, or the least ill symptom or disorder since.

Sir H. had been consulted with on the like occasion by many, and was of opinion, that since it is reckoned, that scarcely 1 in 1000 misses having it some time in their life, the sooner it is given them the better, notwithstanding the heat of summer, or cold of winter; the danger being greater from falling into the distemper naturally, than from the heat or cold of either.

What he had observed, which he thought material, is not to inoculate such as have any breakings out on their faces, soon after the measles, or any other occasion, by which the small-pox were likely to be invited, and come in the face in greater number, and so make the distemper more dangerous. Bleeding in plethoras, or gentle clearing of the stomach and intestines, are necessary; and abstinence from any thing heating, about a week before: and nothing else needful by way of preparation; and very little physic during the course of it, unless accidents happen.

[Then follows a description of the operation of inoculation, which at that time was very rude, and consisted in making an incision into the skin of the arm about 1 inch long, and afterwards applying a dossil dipped in the variolous matter, and keeping it on for 24 hours, covered with a plaster, &c.]

Of above 200 that he had advised before the operation, and looked after during it and its consequences, but one had miscarried, a son of the duke of Bridgewater, in whose family this distemper had been fatal, where the eruption of the small-pox was desperate, notwithstanding it was perfectly safe in his sister, who had undergone the same preparations, and was inoculated the same day, and with the same matter used for her brother.

On the whole it is wonderful, he observes, that this operation, which seems so plainly for the public good, should, through dread of other distempers being inoculated with it, and other unreasonable prejudices, be stopped from procuring it. One thing he had observed, that though the persons inoculated were advanced in years, it was equally successful as in younger persons.

LXXII. Of an Extraordinary Agitation of the Water in a small Lake at Closeburn, in the Shire of Dumfries. By Sir T. Kilpatrick of Closeburn, Bart. p. 521.

About a quarter before 9 on Sunday morning, Feb. 1, 1756, we were alarmed with an unusual motion in the waters of Closeburn-loch. There was first a strong convulsion and agitation of the waters from the west side of the loch towards the middle, where they tossed and wheeled about in a strange manner. Thence proceeded 2 large currents formed like rivers, which ran with rapidity beyond all description, nearly contrary ways, one from the middle to the south-east, and the other to the north-east points of the loch. There they were stopped short, as the banks are pretty high, and obliged to turn, which occasioned a prodigious tumbling and agitation at both ends of this body of water. There was likewise a current, which rose sometimes considerably above the surface near the west side, that frequently ran with great velocity 100 yards to the southward, and returning in a moment with as great velocity the other way. In the next place, there was a tossing of the waters in the ponds, which were more or less moved as the agitations of the loch came nearer this side, or kept at a greater distance from it. These agitations and currents continued, without intermission, for about 3 or 4 hours, when they began to abate a little in their violence, though they were not quite over at sun-set. This strange phenomenon was renewed on Monday morning a little before 9, and lasted for an hour and a half; but the motion of the water was not near so violent as the day before. There was no wind all the time.

LXXIII. Letters on the Irregularities of the Tides at Chatham, Sheerness, Woolwich and Deptford, in Feb. 1756, communicated by George Lord Anson, F. R. S.*

Letter I. From Mr. M. Godden. Dated Chatham-yard, Feb. 23, 1756. p. 523.

Mr. G. remarks on the irregularity of the tides, having taken particular notice of them by the Lys, a French ship, having broken from her moorings 3 times in that week. The first time was on Thursday the 12th instant, at about 10 in the morning, it being then about high water, or rather ebb; so that they could not get her off that tide, but attended and hove her off the next, at about 9 at night, which was sooner than expected by an hour and a half. They then put her to another mooring, and about half past 11 the same night, she broke from them also, and came on shore near the dock, it being then a small matter ebb, so that they could not get her off that tide, but attended her the next, till half past 11 on Friday morning to do it, it then being about the time of high water, but could not; the tide being not so high by 5 or 6 feet as it was the tide before, though it should have been higher, as they were increasing. And he further took notice at the same time, that the tide was at a stand several minutes, and then flowed again near a foot in height before it ebbed, and the next tide, at half past 9 at night, they got the ship off, though they did not expect she would have floated till near 12: and again in transporting her up to her moorings, there was little or no tide ran from 10 to 12, which was about the time of high water; which they greatly wondered at, as it was quite calm. All which irregularities he imagined to be owing to the wind, having had very hard gales for most part of that week.

Letter 2, from Mr. Mic. Monasty, dated Sheerness, Feb. 23, 1756. p. 525.

The day tide on the 13th instant was very remarkable; for it ebbed no more than 2 feet and a half for 4 hours after high water, when it was observed to flow again for a few minutes; then ebbed again, but so little, that at low water, we had 7 feet water at the stern of the dock, which is 5 feet more than was ever known to be. It blew very hard in the morning on the flood, with the wind to the southward of the west, and on the ebb in the afternoon the wind abated and veered to the north-west, to which he then, in part, attributed this phenomenon, as a northerly wind forces water into this river, and always makes high tides, and a southerly wind the contrary.

* The celebrated circumnavigator; he commanded the Channel fleet in 1747, when he captured 6 French men of war and 4 East Indiamen; for which and other services he was created a peer by George II. He was afterwards appointed first Lord of the Admiralty, and admiral and commander in chief of his majesty's fleets. He died in 1762, aged 65. The interesting narrative of his voyage round the world was composed under his own inspection, not by his chaplain as was long believed, but by Mr. Benj. Robins. The title, which became extinct on the death of his lordship, has been lately revived in the person of Thomas Lord Anson of Shugborough.

Letter 3, from Mr. Walter Taylor, dated Woolwich Yard, Feb. 25, 1756. p. 526.

The tides the last week, and even for some days this week, have been very irregular and unusual.

Feb. 9, wind s. tides very irregular. Feb. 10 and 11, the same. The 12th, the night tide flowed about 2 feet 10 inches higher than the morning tide. The 13th, the night tide flowed about 3 feet higher than the morning tide. The 14th, 15th, 16th, 17th, the tides more regular. The 18th, the flood came in much sooner than usual, and seemed to flow gradually at first, but between 1 and 2 p. m. the tide flowed several feet, as on a sudden, and continued flowing till ¼ past 3, being some time longer than it was expected it would, and they had a high tide. The 19th, this day's flood did not hold so long by a quarter of an hour as yesterday's, and not so much water by several feet. The wind being to the westward, and a frost, greatly checked the tide. Since which, the tides have been very regular.

In a 4th letter from Deptford-yard, similar irregularities were observed.

LXXIV. And the same in the River, near London, by a Letter from Robert Dingley, Esq. F. R. S., dated London, March 8, 1756. p. 530.

LXXV. Thoughts on the Rev. Dr. Hales's New Method of Distillation, by the United Force of Air and Fire. By William Brownrigg, M. D., F. R. S. Dated Whitehaven, Dec. 3, 1755. p. 534.

In the process of distilling sea water, as described by Dr. Hales, the great increase of vapour raised by his method, above what is raised by the common method of distillation, may be attributed chiefly to the violent agitation of the water contained in the body of the still, by the motion of the air continually pressed through it. Though the air, by attracting the watry particles, may also contribute to produce this effect. It is however certain, that a simple mechanical agitation of warm water will greatly promote its evaporation, by increasing its surface, from which the vapours arise, and by putting its heated particles in a brisker motion, thus exciting between them actions and reactions, and so disposing them to fly off in elastic vapours. Of this we have instances in warm water, when simply stirred about in vessels, or poured out of one vessel into another; from which the vapours visibly arise in larger quantities, than from the same water when it is not moved by such mechanical agitation.

This excellent invention of Dr. Hales may probably be applied to other purposes, besides that which he had principally in view, viz. the distilling of sea-water with greater ease and expedition, with less fuel, and in smaller vessels, than has hitherto been practised, for the benefit of navigators. It might be of singular use, if it could be applied in the fire-engine. The great expence of large boilers in the construction of that machine, and the vast consumption of fuel in the working of it, render its uses much less extensive than they would be, could those expences be contracted. But air cannot be applied in this engine, to increase the quantity of the elastic steam, since it would pass with the steam from the boiler into the cylinder, and prevent a vacuum from being there produced, and hinder the piston from moving in it.

A mechanical agitation of the water in the boiler of the fire-engine may however be produced by other means, so as that a larger quantity of steam may probably be raised, than can be effected in engines as commonly now constructed; by which means the expences of constructing and working those useful machines may perhaps be greatly lessened.

If, for example, the boiling water, instead of being agitated by air, as in Dr. Hales's method, was briskly stirred about by a wheel placed in the boiler of the fire-engine; it is probable, that by this means the quantity of elastic vapour raised might be considerably increased, and less fuel and a less boiler might then serve the purpose. The wheel might be turned round by the water drawn up by the engine; or might receive its motion from the beam of the engine by means of a crank; or a labourer might be employed in turning it round with the hand.

But the desired effect might, in all probability, be better produced by means of elastic steam driven briskly through the boiling water. The steam of water, as an elastic fluid, possesses many of the properties of common air. Like air, when driven briskly from the æolipile, it is observed to blow up fire; and when forcibly driven through water, will doubtless produce the same agitation, as is done by common air in Dr. Hales's experiment; and may probably have the like effect with air, in elevating a larger quantity of elastic vapours. In order to excite an agitation in the boiling water of a fire-engine, by means of elastic steam. Dr. B. then proposes various means for this end. He also shows how the steam from the boiler of such an engine may be greatly increased in its strength, by heating it, by causing some part of the pipe that conveys it from the boiler to the cylinder, to be kept red hot, by making it pass through a fire.

LXXVI. Of an Extraordinary Motion in the Waters in the Lake Ontario in North-America. From Governor Belcher's Lady; dated Elizabeth-town, New-Jersey, Oct. 22, 1755. p. 544.

I take this opportunity to acquaint you with a strange phenomenon of the lake Ontario, where general Shirley has posted himself with 2000 men, at fort Oswego. A person lately come from the camp reports, that about a fortnight since, that lake rose and fell 5 feet and half, 3 several times, in the space of half an hour.

LXXVII. Of an Earthquake felt at the Hague, on Wednesday the 18th of Feb. 1756. By Mons. Grovestins, Master of the Horse to his R. H. the Prince of Orange. p. 544.

On Wednesday morning, 12 minutes after 8, there was a shock of an earthquake. His chair received 5 successive shakes. The sconces in the chamber were also moved. Ten or 12 minutes after, he perceived a 2d shock, but not so strong as the former. The wind was S.W. Immediately after the earthquake it turned N.E. It was also felt at Maestricht and Utrecht.

LXXVIII. Of the Same Earthquake felt in Holland, Feb. 18, 1756, In a Letter from Mons. Allemand, Professor of Natural Philosophy at Leyden, and F. R. S. p. 545.

This article contains observations similar to the preceding one, and also remarks that the earthquake was felt throughout the whole territories of the republic.

LXXIX. Of the Earthquakes felt at Brussels; in a Letter from John Pringle, M.D., F.R.S. p. 546.

By a letter, which Dr. P. received from Dr. Brady, physician to the court at Brussels, he finds they felt in that city this winter 3 several shocks of an earthquake. The first was on the 26th of December; the 2d on the day following; and the 3d on the 18th of February; being the same day it was said to be felt on our coast, between Margate and Dover; but the hour is not mentioned. All these shocks he says greatly alarmed the inhabitants; but were otherwise attended with no bad consequences. Dr. Brady adds, that he was told by a gentleman from Liege, that the men who were at work in the coal-pits, and particularly in some of the deepest near that city, had assured him, that they heard the rumbling noise preceding the shock as over their heads; while those who were aboveground heard the same kind of noise as under their feet.

LXXX. On the Sinking of a River near Pontypool in Monmouthshire. By Mr. Edward Matthews. p. 547.

The 1st day of January 1756, a poor woman, living near the mouth of the river, sent her daughter for water, a great flood appearing in the river just before, who returned in surprise with the account, that it was dry. The river is called by the name of Frooyd, running between two steep hills, or woods, but not very high; it proceeds from water from the adjacent mountains, and seems penned up for the most part parallel and correspondent to those of the rocks, islands, and neighbouring continents. They contain stones of different sorts, minerals, metals, various petrified bodies, pumice-stone, lavas formed by volcanos. Istria, Morlachia, Dalmatia, Albania, and some other adjacent countries, as well as the rocks, the islands, and the correspondent bottom of the Adriatic sea, consist of a mass of a whitish marble, of a uniform grain, and of almost an equal hardness. It is that kind of marble called by the Italians marmo di Rovigno, and known to the ancients by the name of marmor Traguriense. This vast bed of marble, in many places under both the earth and the sea, is interrupted by several other kinds of marble, and covered by a great variety of bodies. There are discovered there, for instance, gravel, sand, and earth, more or less fat.

The variety of these soils under the sea is remarkable. It is to this that Dr. Donati ascribes the varieties observed with respect to the nature and quantity of plants and animals found at the bottom of the sea. Some places are inhabited by a great number of different species of plants and animals; in others, only some and let out precipitately, to cleanse the iron ore lying near the surface on the sides of these mountains, which greatly discolours the water, which at those times, and after heavy rains, is so rapid and violent, as to carry down prodigious quantities of large stones into another river called Avon Looyd. Mr. M. walked up the Frooyd on the bottom of the river, it being quite dry, up to the chasm, that now receives the water; it is about 20 feet wide; and when its banks are full, about 8 or 10 feet deep; but now filled up to 15 feet with stones carried in by the water. There is a lime-stone rock near the surface, about 2 feet thick, lying in large beds 2 or 3 feet square, more or less in some places, joined close in others. On one side of the river near this hole, are 3 pits sunk at the same time, the one within 10 yards, of which there was no appearance before; the other two at about 30 yards up the side of the hill, which have been observed, for many years, though nobody knew the cause of them, are now sunk some yards deeper, and some trees and shrubs, that were round the edge of the pits, with the ground on which they grew, are sunk down near the bottom. These pits at top are about 12 yards diameter, gradually narrowing to a centre, in shape of a funnel or tun-dish. Under, it is supposed, is this cavity, through which the river now runs, extending itself in one place under the river Avon Looyd, at about a mile distance, where it broke out a few days after, in several places, on the opposite side of it, where were 3 small springs. The reason for this conjecture is, these springs were observed to be always clear till a few days after the sinking of this rock, but now continue to send forth large quantities of this water, which varies in colour like the water received in at the hole.

LXXXI. On the Agitation of the Waters, Nov. 1, 1755, in Scotland and at Hamburgh. Communicated by John Pringle, M. D., F. R. S. p. 550.

About 10 o'clock of the forenoon of Nov. 1, a gentleman at Queen's-ferry, a sea-port town on the Frith of Forth, about 7 miles higher up than Leith, observed the water rise very suddenly, and return again with the same motion, which he judged to be about 12 or 18 inches perpendicular, which made the barks and boats then afloat run forwards and backwards on their ropes with great rapidity; and this continued for 3 or 4 minutes, it being then calm; but after the 2d or 3d rush of water it was always less.

The following phenomena are well vouched to have happened at Hamburgh, the 1st of November 1755. In one of the churches many persons, that were present, observed an agitation of the branched candlesticks hanging from the roof, about 1 in the afternoon. In another church, the cover of the baptistery hanging from the roof was also remarked to be agitated; and the like motions are said to have happened in other churches. Also the water in the canal through the town, and in the river Alster, was agitated the same day. It is described first to have formed several gentle whirlpools, thence to have risen more and more impetuously, throwing about mud brought up from the bottom, and at last to have subsided with a copious white froth. The Elbe rose in some places still more violently.

LXXXII. Microscopical Observations: in a Letter from Edward Wright, Esq. dated at Paris, Dec. 26, 1755. p. 553.

It appears from the experiments of M. de Buffon and Mr. Needham, that animal and vegetable substances infused in boiling water, put into bottles completely filled, and so closely stopped that no air can enter, and even kept for some time in hot ashes, that in case there should be any latent ova of insects they may effectually be destroyed; yet it appears from the said experiments, that such substances, notwithstanding such precautions, afford microscopical animalcules of various kinds, and that sooner or later, according to the greater or less degree of exaltation in the substances. Hence they conclude, that there is a real productive force in nature, by which these animalcula are formed.

Having read the accounts of these experiments, Mr. W. was desirous to make some of the same kind, which he accordingly did, in the summer of the year 1752. Though the greatest part of the animal substances, on which he made any experiments, treated in the manner above-mentioned, yielded, sooner or later, great numbers of microscopical animalcules; yet most of the vegetable substances, whether from the coldness of the season, which was not very favourable that year, or through some fault in preparing the infusion, entirely failed, and underwent a fermentation, without ever giving the smallest signs of any thing endowed with life.

May 1, 1752, at 11 o'clock forenoon, Mr. W. made an infusion of dried millepedes, or wood-lice, such as are commonly kept in the apothecaries' shops. These he put unbruised into a small phial, so as to make it half full; then poured on them as much boiling water as filled it neck and all, stopped it with a well masticated cork, and put it into a pocket, where it was kept in a mild degree of warmth. He let it remain till 10 o'clock the same evening, when he examined a drop of the infusion with the highest magnifier of a very good microscope made by Mr. Clarke of Edinburgh. He found the whole swarming with oblong, slender, flattish pellucid animalcules, pretty nearly of the same breadth throughout the whole length of their bodies, and without any appearance of a tail, fig. 13, pl. 16, all evidently of the same kind, though not all of the same length and dimensions, extremely vivid, and, as appeared, spontaneous in their motions, which they performed in all directions in an undulatory, vermicular way.

Observing the speedy appearance of these animalcules, he wished to know, in how short a time they might be produced; for which purpose, May 3d, he made just such another infusion, putting it into his pocket, as before, and an hour afterwards laid a drop of it before the microscope, while it was as yet milk warm. He observed a very few of these minute bodies moving about briskly in the fluid. An hour after this more of them appeared; and before the end of the 3d hour, the infusion contained a great number of them. They continued however to increase in numbers for an hour or two afterwards, when the infusion seemed to have produced all that it was capable of.

June 3d, he made an infusion in the same way of unbruised cantharides, and in much about the same time found the whole swarming with animalcules of the same kind as those of the infusion of millepedes. These bodies, which at first appeared larger than those in semine masculino, were very soon decomposed into smaller ones, to speak according to the doctrine of Messrs. Needham and Buffon, or, as others would rather incline to express it, succeeded by smaller ones, these again by others still smaller, and so on, until in a few days, the highest magnifier of the microscope could exhibit nothing distinct to the eye. The same substances infused in rectified spirits of wine, or other spirits, showed none of these bodies; and a few drops of such liquors, or of a solution of fixed or volatile alkaline salts, poured into the infusions, instantly destroyed the animalcules.

Mr. W. declines inquiring, whether these animalcules are produced by the decomposition of the substances in which we observe them, which, according to Mons. de Buffon contain a number of living organic particles, or, according to Mr. Needham, a vegetating force in every microscopical point, capable of forming secondary combinations, microscopical plants, zoophytes or animalcules, according to the greater or less degree of exaltation, which the several substances have attained. Or whether they proceed from ova formerly existing in the substances, and capable of enduring a great degree of heat, without being destroyed, the germs of which are sooner or later developed according to the fitness of the nidus, as is the opinion of the learned and ingenious Dr. Parsons, in his treatise on the analogy between the propagation of animals and that of vegetables: as by entering into a discussion of these different sentiments, a large volume might be written without perhaps going to the bottom of the matter. Mr. W. therefore only observes, that whichever of these opinions we embrace, thus far seems to be certain, that the earlier or later appearance of microscopical animalcules, is always in proportion to the degree of tendency to putrefaction in such substances as afford them. This is the case not only with them, but likewise with maggots in meat, which every body knows are produced from the eggs of flies. The two substances millepedes and cantharides, on which the above observations were made, are very putrescent, and the infusions of them soon stunk abominably.

Castor, though an animal substance, and seemingly very much exalted, treated in the same manner as the above-mentioned substances, viewed by the microscope every day, and kept for several months, afforded no animalcules, nor seemed to have undergone the smallest change; which confirms what the ingenious Dr. Pringle has observed, that it is antiseptic; and adds weight to the observation made above, that the appearance of such animalcules denotes a tendency to putrefaction. Hence Mr. W. thinks that such microscopical observations, made with accuracy, might be usefully applied in the investigation of the septic and antiseptic qualities of animal and vegetable substances; since in this way the first motion of putrefaction may be discovered, before it manifests itself otherwise.

Mr. W. subjoins a few remarks concerning exaltation, which seem to deserve attention. All exaltation, he observes, appears to be a certain modification of the salts and oils of bodies: a proper degree of it favours growth and vegetation, and sustains animal life: a greater degree of it, which he calls the putrefactive exaltation, and to which all organized bodies tend more or less, decomposes all such bodies, and favours the production of microscopical animalcules, or the developement of the ova from which they may be hatched. A still higher degree of exaltation puts a stop to this process, as also to vegetation, and in certain circumstances even to animal life, as happens with regard to all acrid chemical preparations, &c. whether of the animal or vegetable kingdom.

Those who imagine that all salts and oils hurt the vegetating force of matter, have fallen into a great error; for whence can such a vegetating force proceed, but from a due mixture and modification of the salts and oils with the earthy principle, which every one allows to be of itself inert? It is true indeed, that a very large portion of salts or oils renders substances antiseptic, or very slow either of vegetation or putrefaction, as is well known with regard to sea-salt, a large quantity of which preserves substances from putrefaction; though, as Dr. Pringle observes, a smaller one rather forwards that process, as it does likewise vegetation. Castor, which as Mr. W. formerly observed, is antiseptic, seems to owe this quality only to a large quantity of a sluggish fetid oil, which it contains.

LXXXIII. On the Cure of a Paralytic Arm, by Electricity; by Cheney Hart, M. D. p. 558.

[This was a case of paralysis rheumatica, cured by electricity used conjointly with other remedies.]

LXXXIV. Observations made at Guadaloupe on the Brimstone-hill, in French La Souffriere, in that Island. By John And. Peyssonel, M. D. Member of the R. A. of Sciences of Paris, &c. and F. R. S. Translated by Dr. Maty. p. 564.

The Island of Guadeloupe is not the only one of the American Antilles, that has volcanos and mines of brimstone; few are without them; they are found in Martinico, Dominica, St. Christopher's, St. Lucia; all which islands produce sulphur, pumice-stones, and other substances usually found in volcanos. The mountain, on which M. P. made his observations, is called La Souffriere, or Brimstone-hill, because it contains ores of sulphur; and its summit constantly emits smoke, and sometimes flames. It is very high, and forms a kind of truncated cone. It rises above the chain of mountains that occupy the centre of the island, and run through all its length from north to south. This conical mountain is about 3 leagues from the sea-shore, east, west, and south, and therefore almost in the middle of the southern part of the island. In ascending, it is soon observed that the woods differ in kind; the trees are smaller, and are no more than shrubs at the top, that is, on a level with the other mountains. Here you meet with none but mountain-mangles, whose wood is crooked and bends downwards, and their bark is a true jesuit's bark. Having arrived at the spring-head of the river of galleons, south of the brimstone-hill, at the place called the Three Springs, the waters were so hot as not to be borne. The neighbouring ground smokes, and is full of brown earth like the dross of iron. In other places the earth is red like colcothar, and even dyes the fingers; but these earths are tasteless. Near these 3 burning hot springs are some others, that are lukewarm, and some very cold. They put some eggs into the hot ones, and they were boiled in 3 minutes, and hard in 7.

Going on, about the length of 400 paces, they began to get sight of the windward, or of the eastern coast of the island. Having passed this mountain of the 3 rivers, and the valley between it and the Brimstone-hill, they began to ascend the latter, where they were obliged to help themselves with their hands, feet, elbows, and knees, and to hold by the fern, aloes, and other plants, some of which were prickly, and very troublesome. They were about an hour and a half getting up to the height of about 500 feet, when they reached the gulf, at the place whence the smoke issues. This place is at the foot of a steep bank, and may be about 25 toises in breadth: there is no grass to be seen, nothing but sulphur and calcined earth; the ground is full of crevices, which emit smoke or vapours; these cracks are deep, and you hear the sulphur boil. Its vapours rising yield very fine chemical flowers, or a pure and refined sulphur. It is chiefly found in those places where the earth lies hollow, and on the chinks or funnels you see the spirit of sulphur run down like fair water, and you breathe an intolerable smell of brimstone The ground is loose, so that they could thrust their canes up to the head, and when drawn out they were as hot as if they had been plunged into lime when slacking. Hastening out of this dangerous situation, they continued climbing to the top of the mountain, keeping to the east, or windward. When at the summit, they discovered another gulf or funnel, that opened some years since, and emits nothing but smoke. The top of the mountain is a very uneven plain, covered with heaps of burnt and calcined earth of various sizes; the ground smokes only at the new funnel, but appears to have formerly burnt in many places: for they observed abundance of these crevices, and even gutters, and very large and deep chinks, which must have burnt in former times. In the middle of this flat is a very deep abyss, or precipice. It is said, there was once a great earthquake in this island, and that the Brimstone-hill took fire, and vomited ashes on all sides, and this mountain cleft in two; when probably this abyss or precipice opened. Perhaps the volcano having been fired by lightning, the salts of the earth joined with the sulphur produced the effect of gunpowder, and occasioned this dreadful earthquake. The mountain having split, cast forth ashes and sulphureous matters all around, and from that time no earthquake has been felt in the island. This abyss, in the middle of the flat, is behind two crags or points, that rise above the mountain, and on the north side answers to the great cleft, which goes down above a thousand feet perpendicular, and penetrates above a hundred paces into the flat, and is more than 20 feet broad; so that in this place the mountain is fairly split, from the top down to the basis of the cone.

From the top of this mountain there is a most delightful prospect. You discover below the islands of Martinique, Dominica, Marigalante, and the whole extent of Guadeloupe. Those of St. Vincent, St. Kits, and even St. Martin, are said to have been seen from the top of this mountain. Montserrat, Antigua, Nevis, Radonde, and several other islands were very distinctly observed. The air at top is bleak and sharp, but the cold not very intense. Here the party had only time to examine the great cavern and the great cleft above it, and then withdraw to the habitation whence they came, being very weary; for in coming down they were often obliged to slide, sometimes sitting, sometimes lying on their backs, and holding by the fern. They were often almost buried by tumbling into holes. They met with abundance of nests of black devils, a kind of sea-birds, that come from the north, and hatch their young on this mountain.

Any quantity of brimstone might be fetched from this mountain, even ship-loads. It might be refined on the spot, or made up into lumps to be sold, and shipped in the ore, if necessary; but it is too cheap a commodity to be worth gathering up in a country, where the price of labour is so high from the scarcity of hands. Bright yellow brimstone with a greenish cast might be gathered round the vent-holes of the burning gulf, also large quantities of fine natural flowers, or very pure sulphur. What passes in this mountain may be called a natural analysis and distillation. The brimstone takes fire in the centre of the earth, as in chemical operations, when the mixture of spirit of nitre and oil of turpentine suddenly produces a surprising heat and flame: in like manner an oily and sulphureous exhalation inflames and sends forth fires, which the ignorant vulgar take for shooting or falling stars. The flowers rise with the acid spirit, which being condensed by the cool air, falls down in drops. By fixing bell-glasses to the apertures of the funnels, one might collect a spirit, that rises naturally. One of them having thrust his cane too far into one of the funnels, and not being able to pull it out again, helped himself with the blade of his sword to catch hold of it. In an instant they saw the hilt quite wet, and the water dropping off, and when he drew it out, they were surprised to find the blade extremely hot.

LXXXVI. Of the Earthquake, felt Feb. 18, 1756, along the Coast of England, between Margate and Dover, in a Letter from Mr. Samuel Warren. Communicated by John Pringle, M.D., F.R.S. p. 579.

This earthquake happened a little before 8 in the morning. Many persons felt it by the shaking of their beds, &c. at Margate, Deal, Dover, Sandwich, &c.

LXXXVII. On the Stones in the Country of Nassau, and the Territories of Treves and Cologn, resembling those of the Giants-Causey, in Ireland. By Abraham Trembley, F.R.S. From the French. p. 581.

These stones were in a quarry, near Weilbourg in the country of Nassau, on the declivity of a hill; it had not been dug into above 20 feet deep, and 40 long. This quarry consists of a mass of stones of an almost regular form. He could not discover at what depth these stones extended under-ground. They appeared very near the surface of the earth, where the quarry lies. And there was a pretty considerable space of ground, in which the top of the stones appeared, and where it was easy to examine the shape of their upper ends. It is very far from being the same in all of them; but when a number of them are compared with one another, we find reason to conclude, that the hexagonal form is the most common. The more regular the figure of these extremities is, the more it approaches to that of a hexagon. The two ends of every stone appeared, for the most part, to have the same shape. The sides of the stone are of the same form with the ends, and are plain. Every stone is therefore a prism of a certain number of sides. They are from 3 to 8 sides, and of all the intermediate numbers. The length of the prisms is unequal, from 2 to 5 feet long. The thickness of them is not at all more equal: it is of 9 inches and under. Many of them form a pillar by lying one upon another; all their ends and joints plain. The pillars, formed by several of those stones, are placed exactly one against the other, without having any void between them. They are in a situation almost perpendicular. On breaking these stones, their colour appears clearly to be black. It is a kind of pretty hard basaltes. It strikes fire with steel; and it appears to be very like that of the Giants Causey in Ireland.

This stone must be very common in the country of Nassau. At some leagues distant from Weilbourg, is an old castle almost entirely built of it. In going from Weilbourg to Coblentz in the electorate of Treves, he observed on the road thither, in the towns and villages through which he passed, that this basaltes was made use of in the buildings and pavements. He made the same remark in his journey from Coblentz to Cologn through Bonne. He found a pretty large heap of it in a village 3 leagues from Bonne. In continuing his journey along the Rhine, in his way to Bonne, he saw in the river, the waters being pretty low, a rock, which stood a foot or two out of the water, which was a mass of those prisms of basaltes, the heads of which appeared; and which he concluded was the top of a natural mass of the stone. Hence he was convinced that there were quarries of it along the Rhine. In coming near Bonne, the parapet-walls along both sides of the high road, are found built of these basaltes stones. There are many of them in the old walls of the ramparts of Bonne and Cologn, and in the pavements of those cities. Some authors mention quarries of this basaltes in Upper and Lower Saxony, and in Silesia.

Those who have made observations on salts, and inquiries into stones, minerals, and metals, know how common crystallizations are in nature. A very great variety are found in searching mountains, visiting caverns, and descending into mines. There are few of the naturalists, accustomed to these researches, who shall observe the basaltes above mentioned, but will be inclined to consider them as so many crystallizations.

LXXXVIII. Account of a Work published in Italian by Vitaliano Donati, M.D. containing, An Essay towards a Natural History of the Adriatic Sea. By Mr. Abraham Trembley, F.R.S. From the French. p. 585.

In this work, Dr. Donati examines both the earth and the sea, and even the soil under the sea, to discover their fossils and other productions. His inquiries have enabled him to determine, that there is very little difference between the bottom of the Adriatic sea and the surface of the neighbouring countries. There are at the bottom of the water, mountains, plains, vallies, and caverns, just as on the land. The soil consists of different strata placed one upon another; and particular species are found; and lastly, there are other places, in which neither plants nor animals are to be met with. These observations not only point out the affinity and resemblance between the surface of the earth and the bottom of the sea; but may likewise contribute to discover one cause of the varieties which are observed in the distribution of the marine fossils found in the earth. Dr. Donati remarked in that vast mass of marble, which is common to the bottom of one part of the Adriatic sea, and to the neighbouring provinces towards the east, a multitude of marine bodies petrified; some of which are so united to the stony substance, that they are scarcely to be distinguished. He found in some places human bones petrified, which form one mass with a mixture of marble, red earth, and stalactites.

One of the objects, which most excited the attention of our author, was a crust, which he discovered under the water in divers places, and for a great extent. It is a composition of crustaceous and testaceous bodies and beds of polypes of different kinds, confusedly blended with earth, sand, and gravel. They are found at the depth of a foot or more, entirely petrified and reduced to marble. At less than a foot deep they approach nearer to their natural state. And at the surface of this crust, they are either dead, though extremely well preserved, or still living. This observation demonstrates, that stones or petrifactions may be formed, and actually are formed, in great quantities under the water.

It is to be remarked, that these crustaceous and testaceous bodies and beds of polypes, are every where mingled in the utmost confusion with each other: which shows a striking resemblance between the crust discovered at the bottom of the sea, and those of the marine bodies petrified, found in many parts under the earth, and especially in Italy. If these marine bodies petrified are naturally in that confusion in the sea; if they were born and die; and if they have been petrified in that state; it is highly probable, that those which are found underground in the strata in such confusion, are likewise placed naturally in the same manner under the sea, when it covers them, and not by means of extraordinary events, such as volcanos and earthquakes, as has been conjectured.

The more these bodies and beds of polypes multiply, the more their exuviæ and skeletons contribute to enlarge this crust discovered at the bottom of the sea. Dr. Donati remarked, that in several parts it formed very considerable banks, and of a very great thickness. Hence it follows that the bottom of the sea is constantly rising higher and higher. Divers other causes contribute to it. Snow and rain-waters bring down from the neighbouring mountains, into the sea, a great quantity of earth and stones. The waves, beating against the shores of the continent and islands, detach many masses, which are spread upon the bottom of the sea. The rivers carry the mud with their waters into the sea, at the bottom of which that mud deposits itself. From the rising of the bottom of the sea, that of the level of the water naturally follows. Dr. Donati furnishes us with a great number of facts in proof of this. He observed, that at Venice, in Istria, and in Dalmatia, the level of the waters is several feet higher than it was formerly. This elevation of the waters is observed only on the northern and eastern coasts of the Adriatic. The sea seems on the contrary, to abandon the western coast, that of Italy. This Dr. Donati has showed by many very interesting facts.

He proceeds then to the observations, which he made upon the plants and animals of the Adriatic sea. He begins with some general reflections on the nature of both. On this occasion he treats of the question concerning the resemblance between plants and animals, and in general of the chain, which these different organised bodies form by the affinity between them established by nature. In mentioning the facts, which show this imperceptible transition from the class of animals to that of plants, he seems inclined to believe, that these facts are most frequently to be met with in the waters.

After having given a description of several very curious marine plants, he proceeds to the beds of polypes. He gives this name to all those organized bodies, known under the name of coralline bodies; and which were, for a long time, ranged under the class of plants. He then mentions different bodies, which he calls plant-animals, and animal-plants, according to the characters which he found belonging to them, and which bring them more or less near to one or other of these general classes.

LXXXIX. On a Parthian Coin, with Characters on the Reverse resembling those of the Palmyrenes. By the Rev. John Swinton, M. A. of Christ-Church, Oxon, F. R. S. p. 593.

Some years before, Mr. S. met with a small brass medal, in but indifferent conservation; which he discovered, he thinks, by comparing it with others, to be a Parthian coin. This medal, he apprehends, exhibits the head of Vologeses the 3d, adorned with a beard and a tiara, after the Parthian manner, with a beta behind it, which seems to point out the place in which it was struck. The reverse presents a strange sort of instrument or machine, which perhaps may be imagined to represent a key, besides some traces of characters in a great measure defaced, and which he thinks are 4 entire Palmyrene letters.

XC. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society, by the Company of Apothecaries, for the Year 1755, pursuant to the Direction Sir Hans Sloane, Baronet, by John Wilmer, M. D., &c. p. 607.

This is the 34th annual presentation of this kind, completing to the number of 1700 different plants.

XCI. On the Earthquakes felt at Turin, Dec. 9, 1755, and March 8, 1756. By Dr. Vital. Donati, Prof. of Botany at Turin. From the Italian. p. 612.

The cause of earthquakes is unknown to me. The ancients have observed, that earthquakes were accompanied with some particular meteor, and some remarkable alteration in the air. Such alterations have been observed at the time of the late earthquakes. Who knows, whether an electrical force be not capable of moving more than a quarter of our globe? I have communicated this notion to father Beccaria, and I found him almost entirely convinced of it.

On the 9th of December, at half an hour after 2 in the afternoon, a shock of an earthquake was felt here at Turin; but not a considerable one, so that a great number of persons did not perceive it. For my own part I felt it very sensibly, being then in the University-pulpit raised very high. The chair, on which I sat, was thrown by the shock from one side of the pulpit to the other, in the direction of south to north. This shock lasted between 4 and 6 seconds. Some minutes after came another shock, but it was extremely slight. Its direction was likewise from south to north. I have been informed from Milan, that about the same hour, and on the same day, a shock of an earthquake had been felt. The waters did not rise, and yet a good deal of motion was observed in those of the lakes. For 3 days the waters rose from underground in the lower apartments of the houses situated near the east gate. The springs that water the lands in the country, became more copious.

On the 28th of December at 6 o'clock, according to the Italian way of reckoning, a slight earthquake was felt at Padua.

On the 8th of March, at half after 11 in the morning, in the French way of reckoning, I felt 2 shocks directed from above downwards, but they were very slight.

CII. Of a Continued Succession of Earthquakes at Brigue in Valais. Written by the Rector of the College of Jesuits at Brigue. From the Latin. p. 616.

Valais, and especially Brigue, have almost every 10 years felt earthquakes, but never any so considerable as in 1755. For in that year, on the 1st of November, which was so fatal to Portugal, we felt Brigue several times shaken, and particularly on that very day. And from that time, especially in the night, the walls were perceived by many persons to tremble; whence they justly apprehended still greater shocks of an earthquake. On the 9th of December, about 2 in the afternoon, the earth at first made a great noise, and seemed, as it were, to give a signal for immediately retiring. This was, not long after, followed by repeated, but slight motions. At a quarter after 2, the earth was again shaken, and a much louder noise heard: at last, a little before half an hour after 2, all Valais seemed on the point of destruction; for the earth began not only to tremble, but to send forth a horrible noise, and to shake all the buildings with so violent a motion in the space of 2 pater nosters, that the houses inclined on each side alternately, and rocked like a cradle: almost all the chimnies were thrown down; all the churches suffered very great damage; the towers gaped; a considerable number of walls fell down; and stones of all sizes poured down from all the buildings, so that no house at Brigue escaped some injury.

The whole neighbourhood suffered the same calamity, especially Glisa and Natria. In the latter, the roof of the parish church fell at the same moment; and at Glisa, the large church, and especially the tower, were greatly damaged. For a great part of the wall of the tower being removed out of its place, fell on the roof of the church, and broke it, and demolished the side altar under it. At Brigue both the church and college of the jesuits suffered very considerably. Part of the roof of the former fell down; and all the walls of the college were much cracked. In some places the earth opened and immediately closed again; and water rose from the ground several feet high. Some fountains also ceased running; and not a few, never seen before, have flowed from that time.

From the 9th of December to the 21st, the shocks were repeated every day, but still fewer and less violent. On the 21st, at 4 in the morning, Brigue was so much shaken, that every body was justly frightened: but no damage was done, except the falling down of some stones. From the 21st to the 27th, we felt the earth moved twice or thrice every day at different times. On the 27th, at half after 2 in the afternoon, Brigue suffered a shock almost equal to that on the 9th, but of a shorter duration, and attended with scarcely any damage. On the 28th, about 6, A. M. there were 2 slighter motions. The 29th was the first day free from disturbance. On the 30th, at one in the night, the houses were greatly shaken, so that some chimnies, which had been before damaged, now fell.

On the 2d of January, 1756, at half after 9 at night, there was a slight shock. On the 3d, a little before 10 in the morning, there was another gentle one; but none till the 6th, before 8 at night, when a pretty considerable shock happened. On the 7th, about 5 in the evening, were two more, as also on the 8th at half after 8 at night. For the 3 following days all things were quiet. On the 11th, at 3 in the morning, and again about 8, and on the 12th and 13th were some few shocks, but slight. On the 14th, at half an hour after 2 in the morning, every thing was put into such an agitation, as is inexpressible; but the damage was but small, as the motion lasted but 3 or 4 seconds. On the 15th, at half an hour after 5 in the morning, there was a slight shock. It is observable, that on this day, and generally for 3 or 4 hours before the earthquake, we observed a gentle trembling to precede, and the winds which were before violent, to subside of a sudden: and that the motion seemed always to be propagated from the south to the north. It is fact, that all the books in our library, though of a square form, were all thrown down from the south towards the north. I observed the same in the chasms of the ground, which were nearly parallel with the meridian. I often remarked likewise, that the Rhone grew turbid a little before the earthquakes; and I frequently took notice, in the evening after sun-set, very long clouds stretched out like a straight line, without any breadth, and extended from the south to the north. The earth in some places was broken into fissures, but not large ones.

The writer then goes on to state, that repeated shocks were felt, but gradually less violent, from the 18th of January till the end of the month; that on the 6th and 18th of February violent shocks were experienced, with slight intermediate ones; and that they were repeated slightly till the 26th, when they ceased.

CIII. Extract of a Letter of Mons. la Condamine, F. R. S. to Dr. Maty, F. R. S. Translated from the French. Dated Rome, March 11, 1756. p. 622.

The Abbé Barthelemi, who is here, has been at Naples. In the manner of going on with the manuscripts there, it will require above a century to open and paste them all. However it is done with great dexterity. But there is only one person employed in it. The Canonico Mazzocchi, who copies them, is very capable of that task. An academy of antiquaries is just founded at Naples, for explaining all the antiquities dug up at Herculaneum; but according to their method of discussing things in their assemblies, they will not explain 2 dozen antiquities in a year. They will alter their method, and find, that such kinds of works, and perhaps all others, are not to be done by a company. The Abbè Barthelemi has read very well a page, except a few words, which he had not time to study. The account of the manuscript on music is true.

The measures of the Abbé de la Caille, and those of Father Maire and Father Boscovich do not agree with the elliptical curve of the meridian, or with the circularity of the parallels. And the earthquakes felt on the same day on all the coasts of Europe, and in Africa and America, at Ancona, Morocco, Boston, and in the Baltic, may contribute to convince those who should doubt of it, that the earth has immense cavities, and that it is very heterogeneous, or rather of a very unequal density. Consequently its figure is a little irregular; or, if the curvature be such as the laws of statics seem to require in the hypothesis of homogeneity, that figure must be altered by changes happening in the internal parts of the mass. It was at first supposed to be spherical, and the orbits of the planets were considered as circular. It was afterwards found that they were elliptical, and the earth an elliptoid. Every step made in the study of natural philosophy has discovered some apparent irregularity, according to our manner of conception. The refractions, the aberration of light, the nutation of the earth's axis have all been reduced to a calculation. Afterwards was found out the irregularity of the refractions on small eminences, which perplex astronomers. The heterogeneity of our globe will puzzle the mathematicians; and earthquakes will perhaps do so more than all the rest. I have probably observed to you before, that I am convineed, that Italy was a chain of volcanos, of which we know only some of the links. I have found lavas exactly like that of Vesuvius in the whole way from Florence to Naples, and in places where there was no suspicion of volcanos. All the lakes of Italy, which I have seen hitherto, exhibit traces, not to say evidences, of this.

I begin to think that the whole earth is perhaps in the same case with its surface, and was thrown into the utmost disorder at some period of time, of which no remembrance has been preserved. Lazzaro Moro, a Venetian, has gone much further than I do: all the mountains, isles, and continents arose, according to him, from the bottom of the sea, by means of subterraneous fires. I never heard of his opinion till after I had formed my own conjecture, or rather verified the fact in part of the Apennine which I have passed through. I have had time only to run over the titles of his chapters.

CIV. On the Currents of Sea at the Antilles. By Dr. Peyssonnel, F.R.S. p. 624.

The coasts of these American islands are subject to counter-tides, or extraordinary currents, which render it very dangerous to chaloupes and other small craft to land; while at the same time the boats and ships in the roads are scarcely ever sensible of them, and seldom incommoded by them; nor do those which are out at sea appear to be affected by them. It is however certain that a regular wind constantly blows, in these parts of the torrid zone, from the tropic of Cancer, to the equinoctial line, from the east; inclining sometimes northward, and sometimes southward. This wind is called alizé, or trade-wind, for reasons admitted by philosophers, and it draws the water westward, giving a total and uniform course to that immense quantity, which comes from the great river of the Amazons, and from an infinite number of other rivers, which discharge themselves into the ocean. These currents passing to the westward, go up to the American islands, then to the coasts of Jucatan and Mexico, and running round in the gulf, return into the great ocean, by the straits of Bahama, along the coasts of Florida, in order to pursue, in the north, the course ordained them by the Supreme Being. It is in this course the waters are known to run with an extraordinary rapidity; they pass between the great and little islands of America, in the great deeps, by an almost even and imperceptible motion; but against the shores and coasts of these islands, which form this archipelago, these currents are very sensible and dangerous; they interrupt the navigation, insomuch that it is scarcely possible to stem these tides to get to the eastward.

It often happens, that vessels steering from St. Domingo, or the other Leeward islands, to the windward ones, cannot absolutely accomplish it, and are therefore obliged to get out of the channel, and steer away to the northward, in order to tack up to the windward isles. These are daily observations, and well known to all navigators of America.

Besides these regular currents, there are others, called counter-tides, which are observable on the sea-coasts and shores. In places where these flow, the sea rises in an extraordinary manner, becoming very furious without any apparent cause, and without being moved by any wind; the waves rise and open very high, and break against the shore, with such violence, that it is impossible for vessels to land. These he thinks are chiefly caused by the pressure of heavy black clouds sometimes seen hanging over an island or the sea. As to other currents in the main seas, or in other particular situations, as the gut and the coasts of the Mediterranean, Dr. P. ascribes them to the action of the winds, &c.

Hurricanes are foreseen by a calm, and a frequent shifting of breezes from all points; the setting sun of a blood-red; little clouds moving with great rapidity; the sea-birds, called frigates, and many other kinds, quit the air, and seek the shore. By these signs, together with the season in which these happen the hurricanes are expected; proper precautions are then taken to avoid the fury of the winds; the houses are propped, the windows and doors are barred up, and papers and other valuable moveables are secured in chests. Soon after, a north breeze springs up, which comes to the north-east, and from south to south-east; the air is darkened by one continued thick cloud, which increases the horrors of the night: for it often happens, that these tempests come in the night, and continue all the next day. In the last hurricane he saw, the wind stood at north-east, and blew with such violence, that the largest trees were torn up by the roots, their trunks broken to pieces, and not a leaf left on those other trees which yielded to the fury of the winds; the houses were thrown down, and the tops of the sugar-mills, which are conical, and less susceptible of being thrown down, were crushed to pieces; scarcely any thing remained standing on the ground. These furious winds were accompanied with a violent rain, which resembled the mist made by the agitation of waves, or like waters kept up by the wind. The tempest lasts till day-light, and sometimes continues pretty far in the day. In that in 1740, towards 8 o'clock in the morning, it grew suddenly calm for a quarter of an hour, and then returned again blowing from the south, with such violence, that the buildings and trees, which were destroyed by the north wind before, were blown about, and moved by the first blast of that from the south. At the end of these there appears lightning, and we hear the noise of thunder: these are the signs of the tempest's being at an end; for the wind softens gradually, and all becomes quiet.

After these hurricanes the forests appeared only like a parcel of ship-masts or poles standing; all the trees being stripped of their leaves, and their branches broken off made a dreadful appearance, especially in these countries, where a perpetual verdure adorns the trees and fields. Every one is employed in repairing his losses, and mending the dismal remains of the frightful wreck.

XCV. Of the Lacerta (Crocodilus) ventre marsupio donato, faucibus Merganseris rostrum æmulantibus. By Mr. George Edwards.* p. 639.

What is most extraordinary in this species, and distinguishes it from all other crocodiles, is the narrowness of the beak or chaps, which appears like the bill of the bird called a goosander (merganser). It has small sharp teeth, of which he says no more, as he has given 3 very exact views of the head and beak; see fig. 14, pl. 16. Another particularity is a pouch or open purse in the middle of the under side of the belly, which seems to be naturally formed, with round lips and a hollow within, perhaps to receive its young in times of danger; as we find it in the American opossum. The opinion of Dr. Parsons too was, that the opening in the belly was really natural, it having no appearance of having been cut or torn open. In other respects it has all the marks common to alligators and crocodiles, viz. a particular strong square scaliness on the back, which in the young ones appear distinct and regular, but in the older ones lose their distinct form, and become knobbed and rough, like the bark of an old tree; and in having small, round, and oval scales on their sides, which in the young ones are no larger than rape seeds; and the belly is scaled, to appearance a little like the laying of bricks in a building. It has fins on the outsides of its fore and hinder legs, as other crocodiles have. It has also a great distinguishing mark of the crocodile kind, viz. two rows of fins on the upperside of the tail, which begin insensibly small at the setting on of the tail, and increase gradually as they advance toward the middle of it, where they become one row, and so continue to the end. The tail is roundish at its beginning, but from the middle, where the two rows of fins become one, it is flat like an oar. The fore feet have each 5 toes, the hinder feet only 4; which is also a mark of the crocodile; all the lesser lizards having 5 toes on each of their hinder feet. In the fore and hinder feet, the 3d and 4th toes only are webbed together. The eyes are very prominent. The head is covered with several large scales. The beak is finely creased transversely. As I have been very exact, says Mr. E. in my figure, which was worked on the copper-plate immediately from nature by my own hand, and in several different views, it will express more than can easily be conveyed by words. It appeared in the spirits all over of a yellowish olive colour, the underside lighter than the upper; the upperside having some dusky marks and spots, as represented in the print. This species Mr. E. believes, when at full growth, to be near, if not quite, as large as the common crocodile.

* This species is the *lacerta gangetica,* Linn. Gmel. It grows to a larger size than the Nilotic crocodile, and exclusive of the long and narrow form of the snout, it has nearly double the number of teeth: the specimen here described was so young as to have the opening of the umbilical vessels still remaining: otherwise it has no particular ventral cavity, as erroneously imagined by the author.

END OF VOLUME TENTH.

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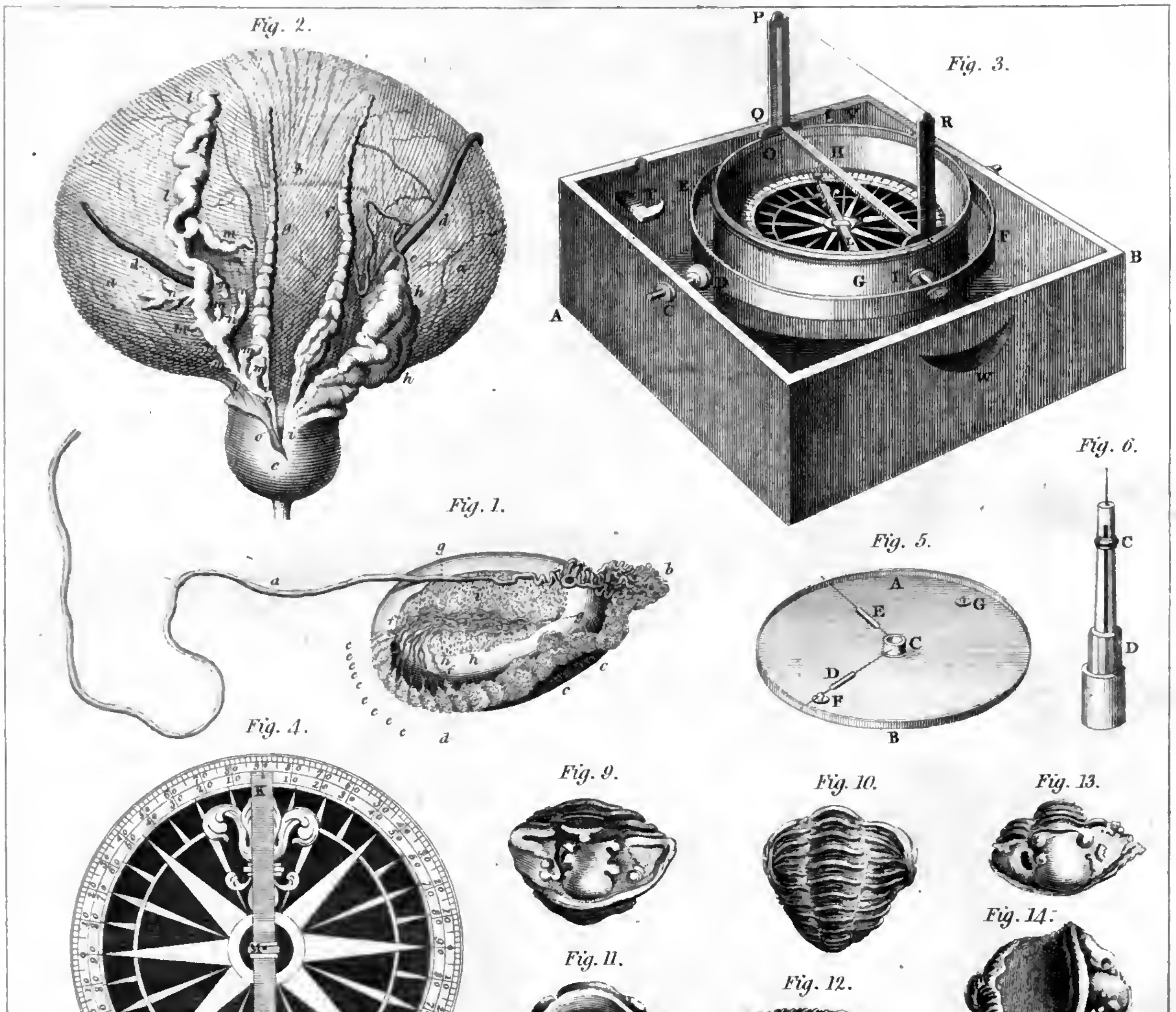

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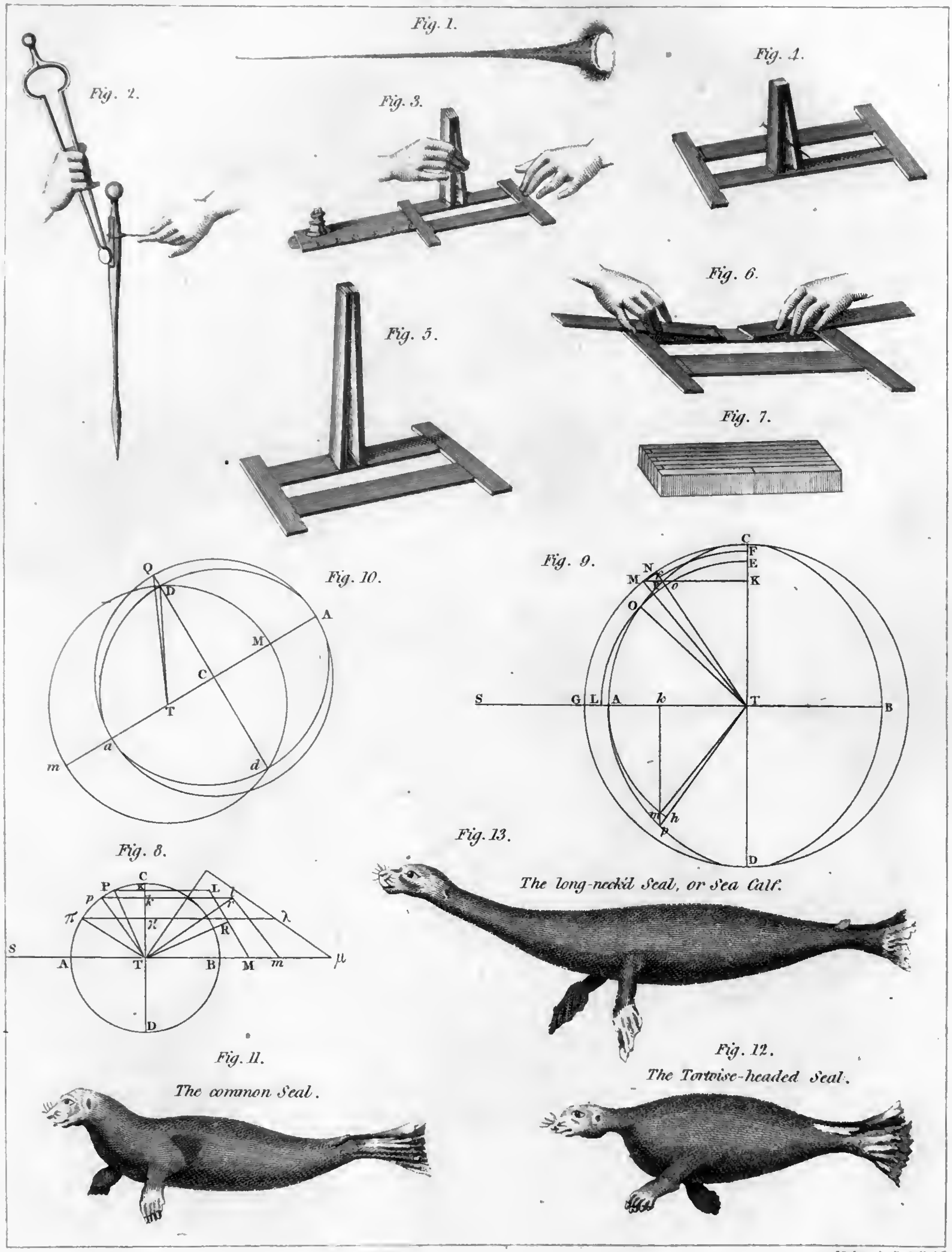

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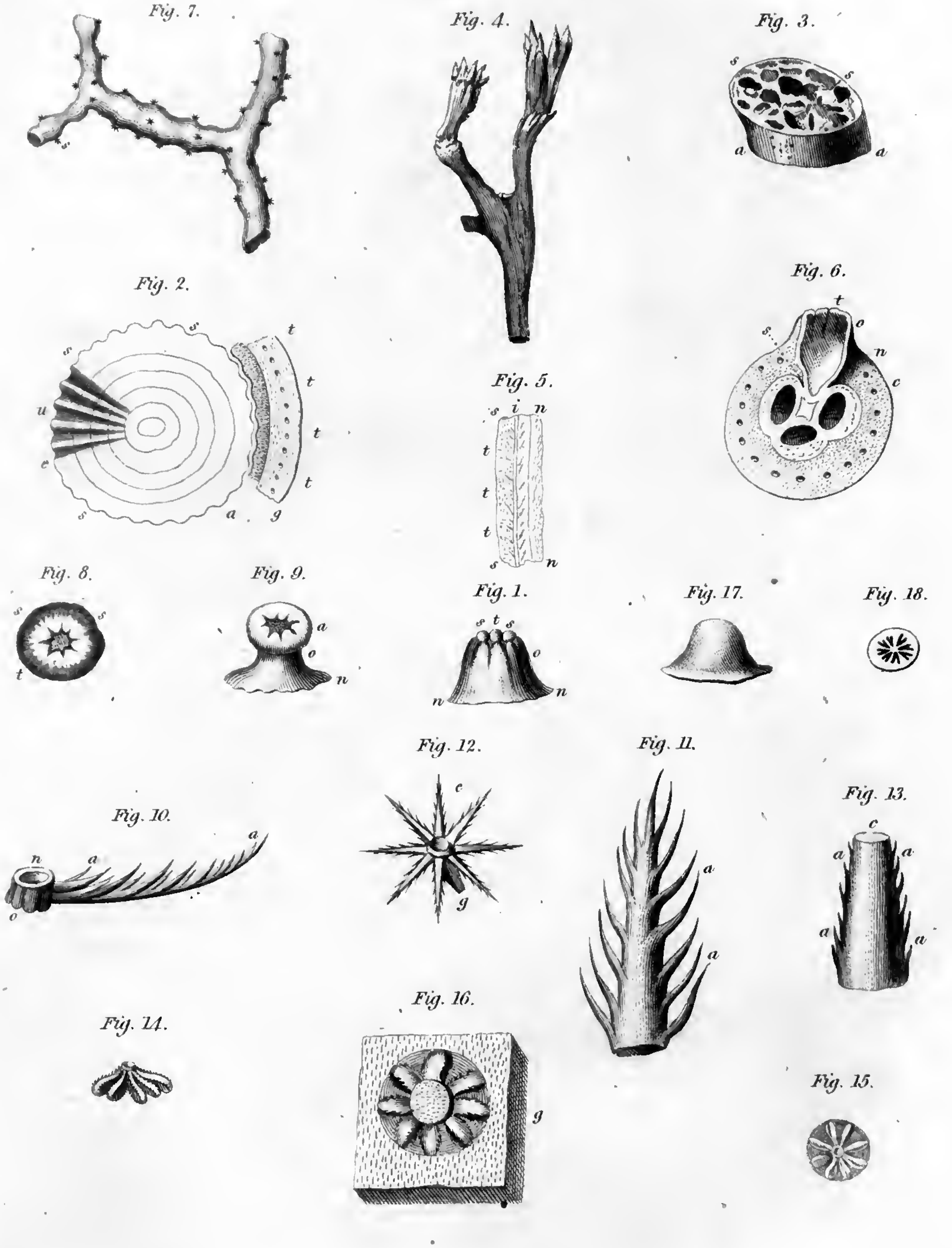

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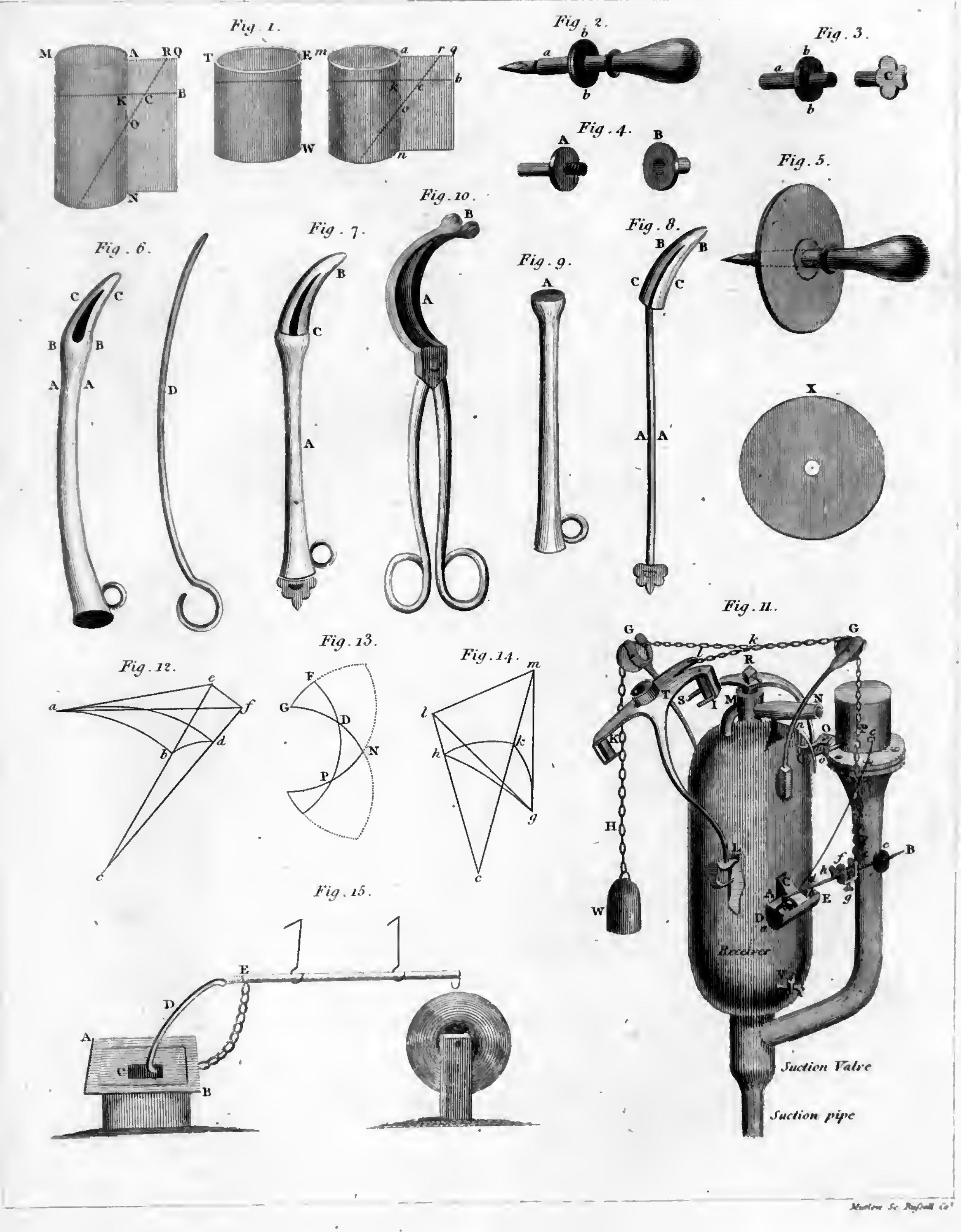

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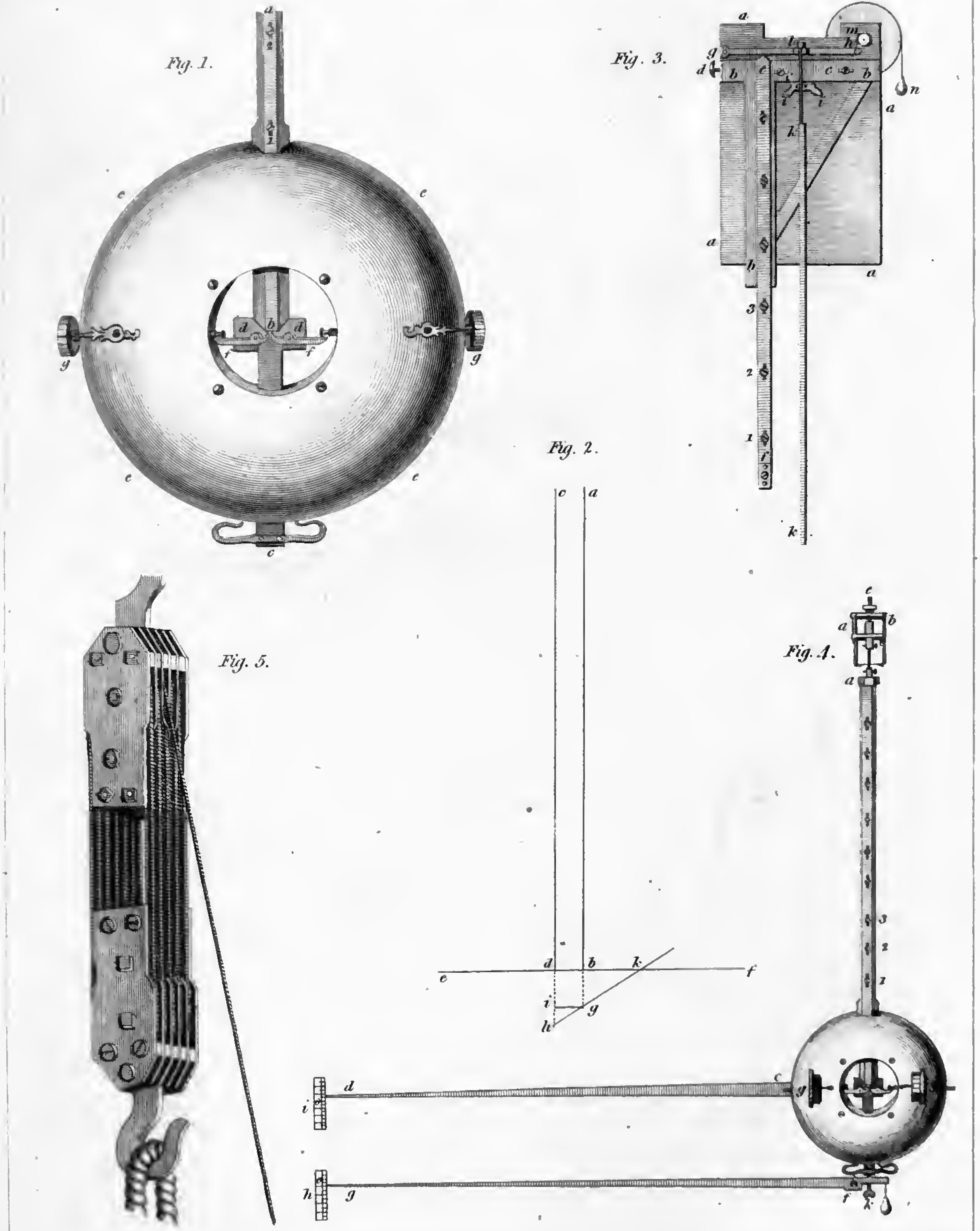

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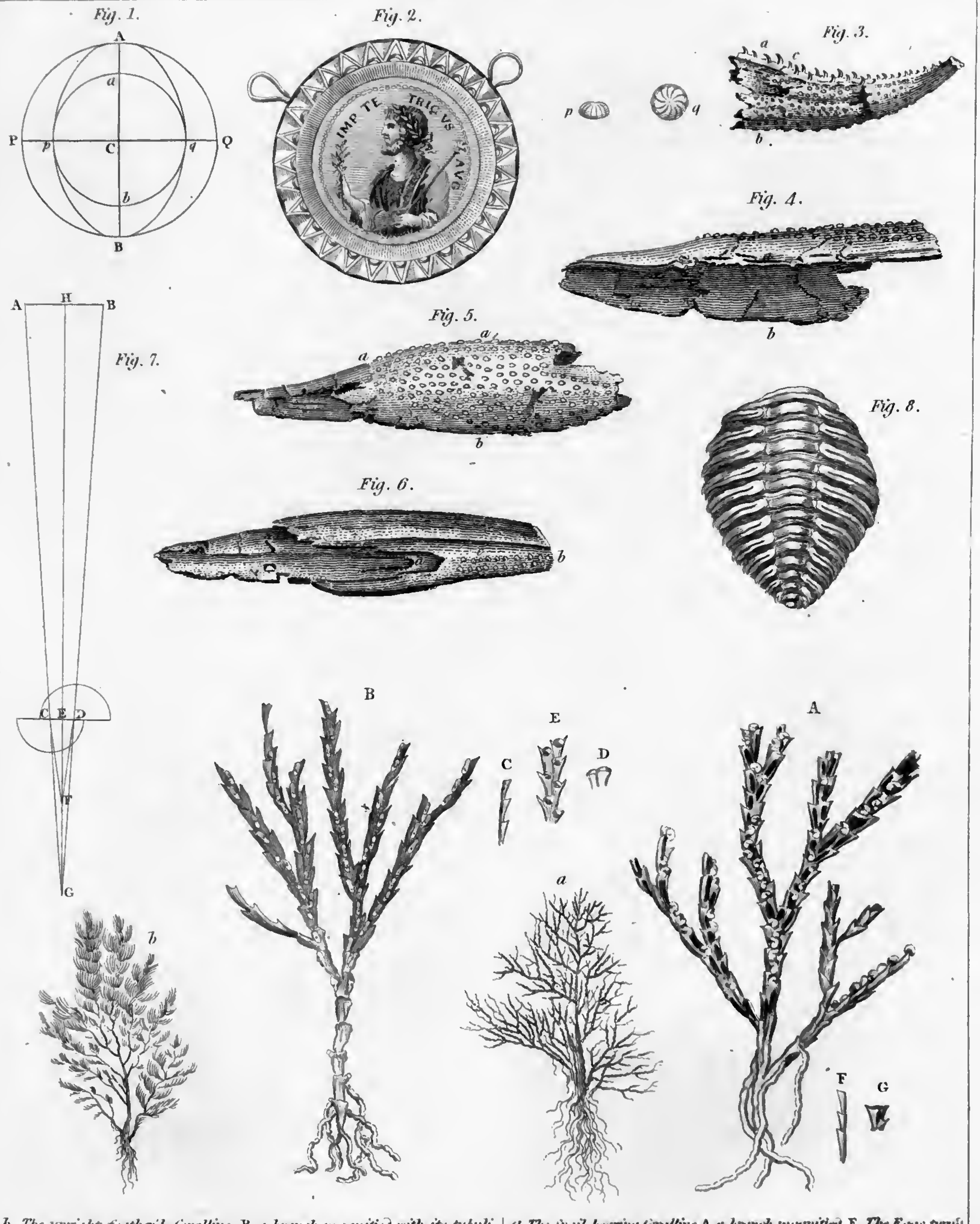
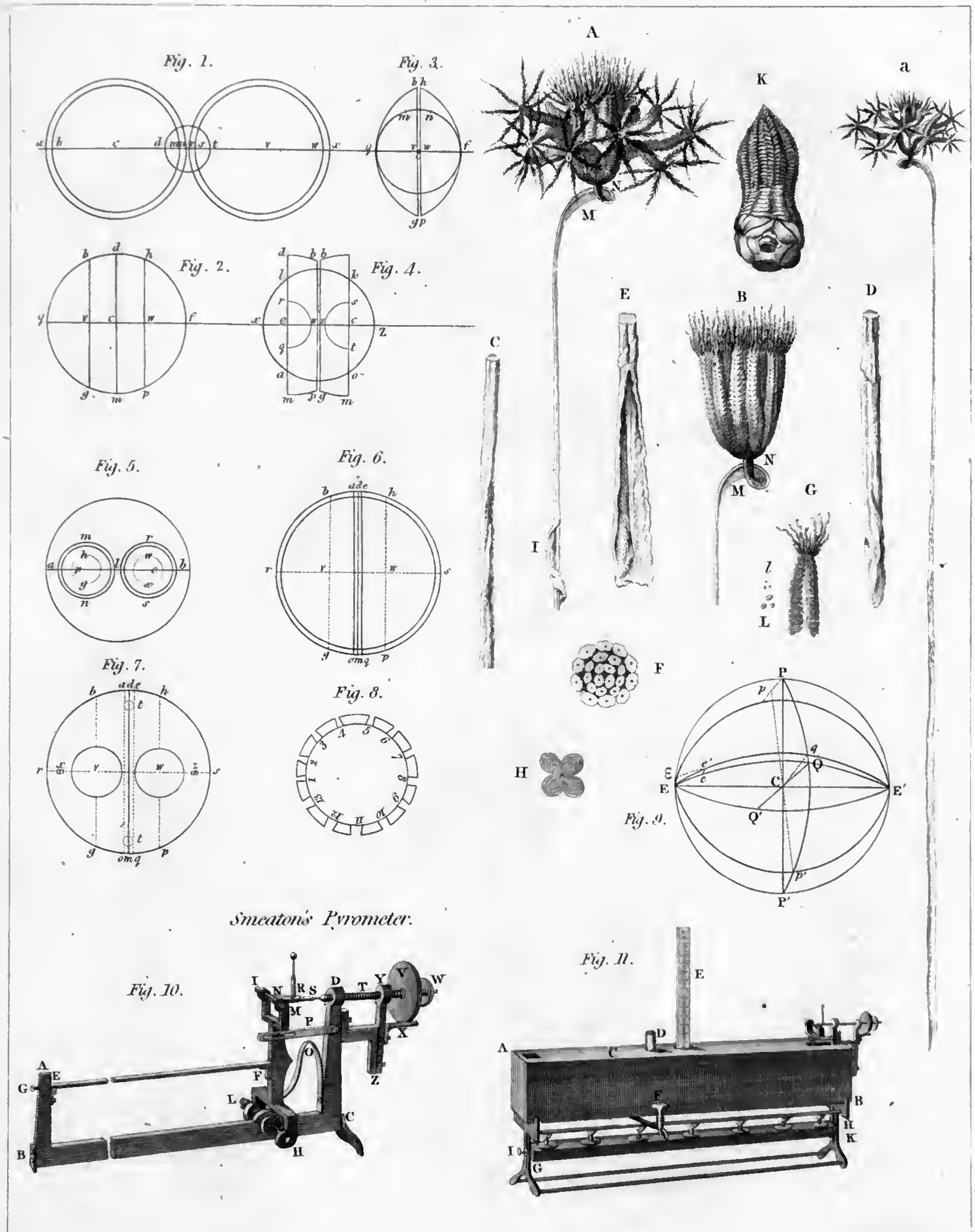


b. The upright featherd Coralline. B. a branch magnified with its tubuli. C. The upright section of the cells shewing the Eggs. D. The cross section. | *a. The Snail bearing Coralline. A. a branch magnified. E. The Eggs turnd to testaceous animals F. The upright section. G. The cross section.*

Mutlow Sc. Russell Ct.



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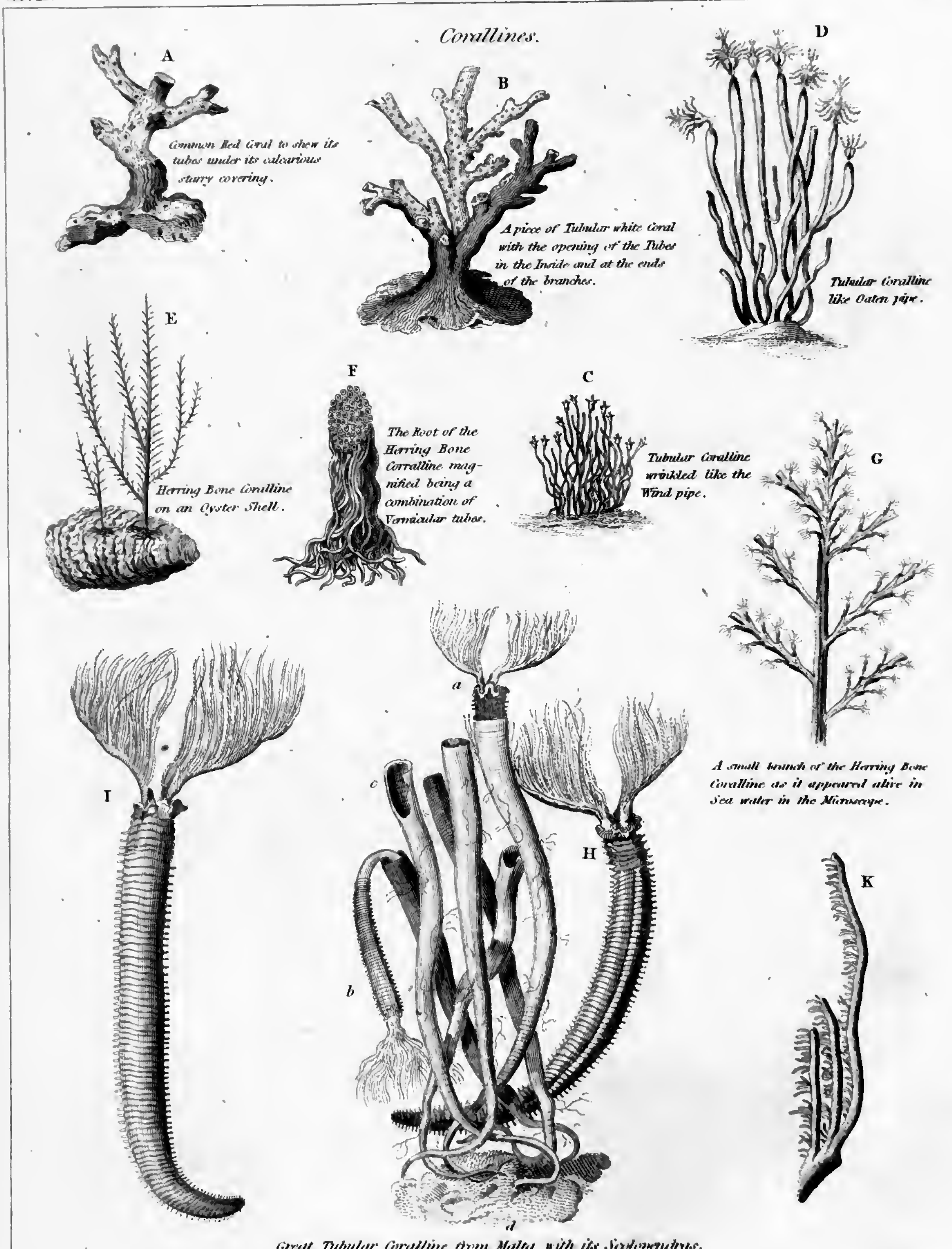


Great Tubular Coralline from Malta with its Scolopendrus.

Fig. 1.

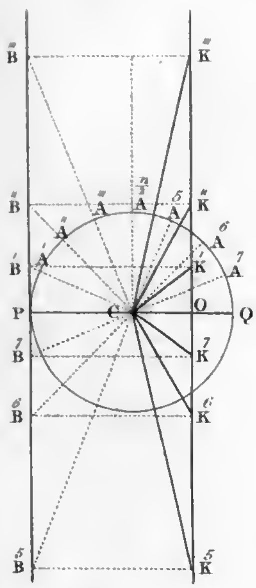
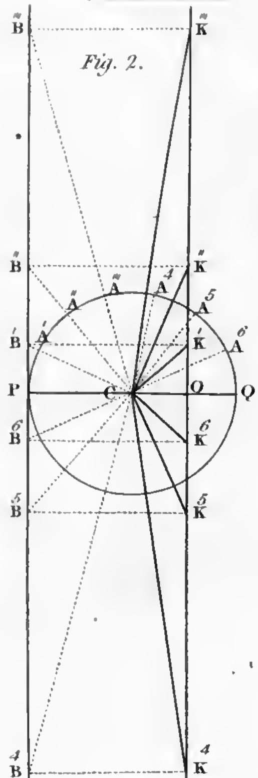


Fig. 2.



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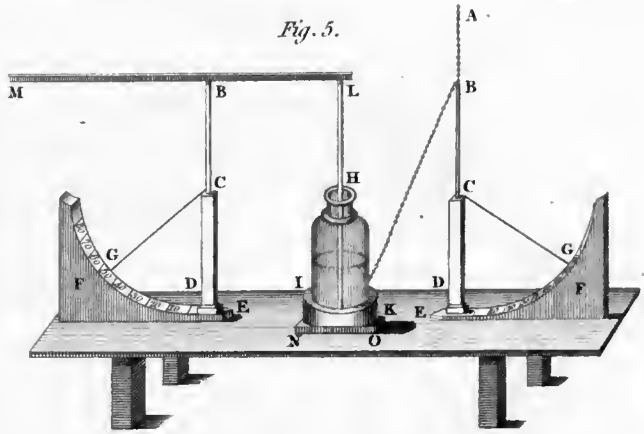

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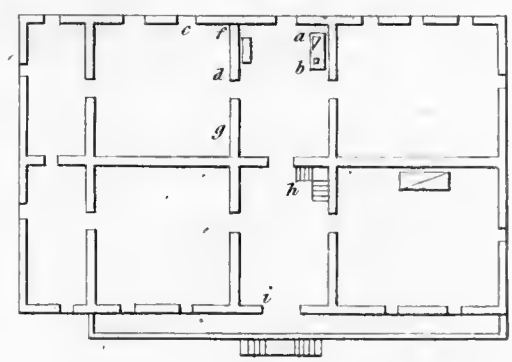


Fig. 4.

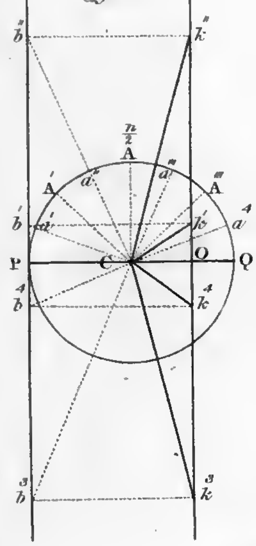


Fig. 3.

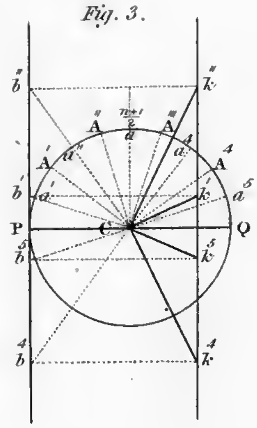


Fig. 8.

Fig. 9.

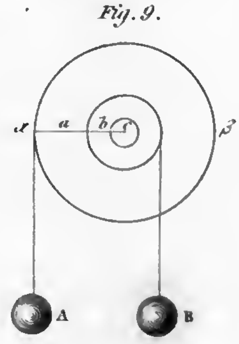


Fig. 7.

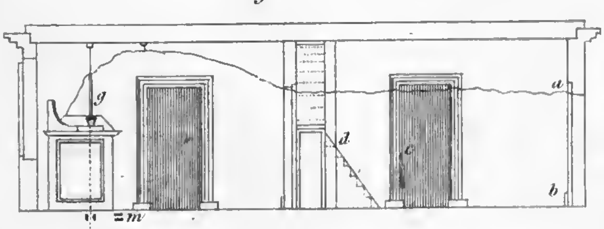
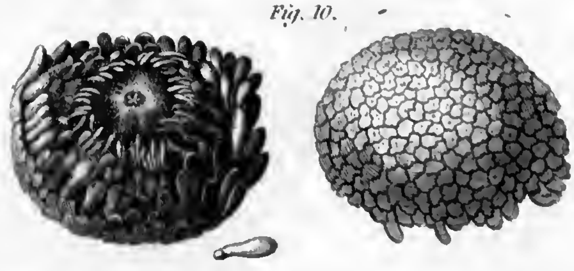


Fig. 10.



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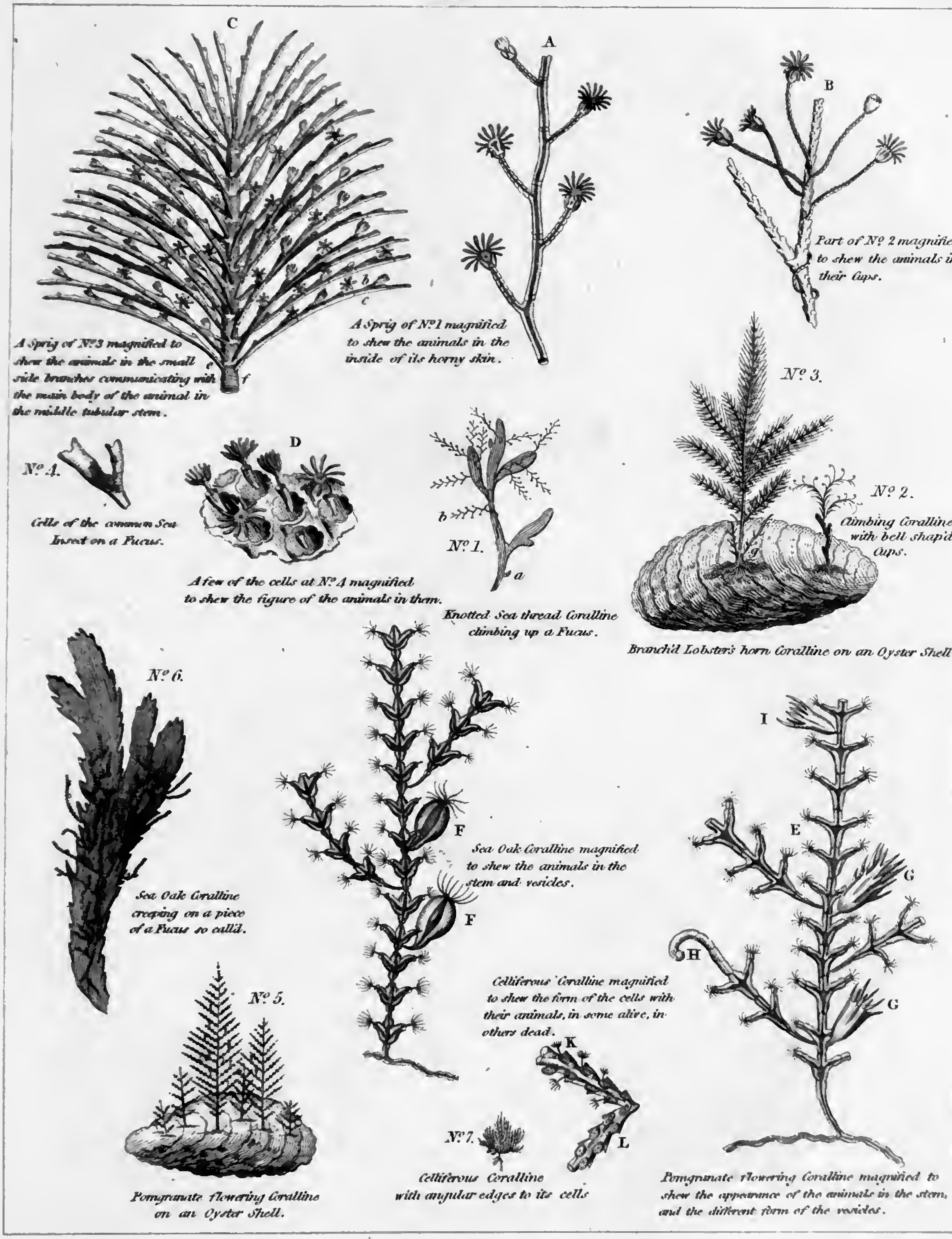

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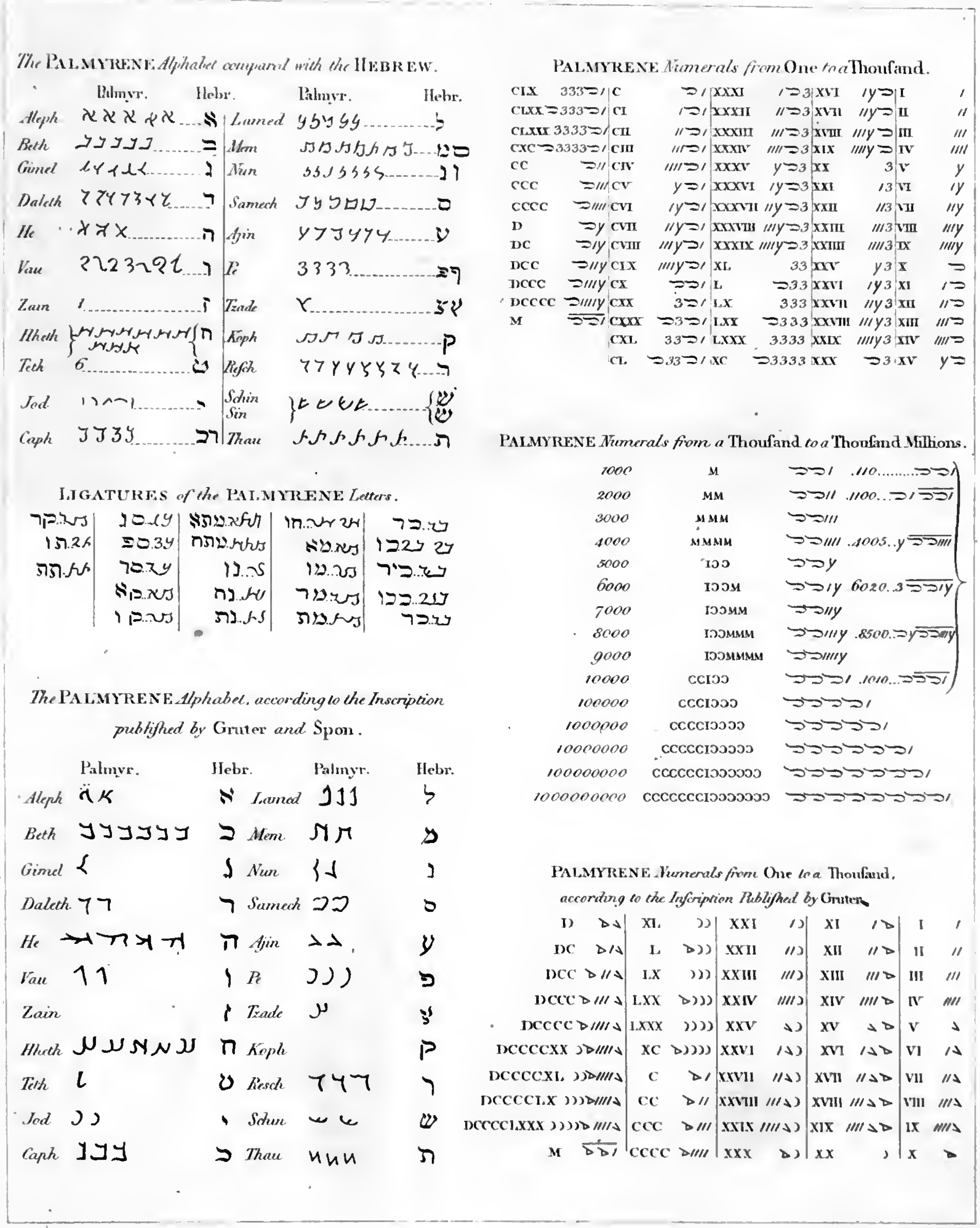

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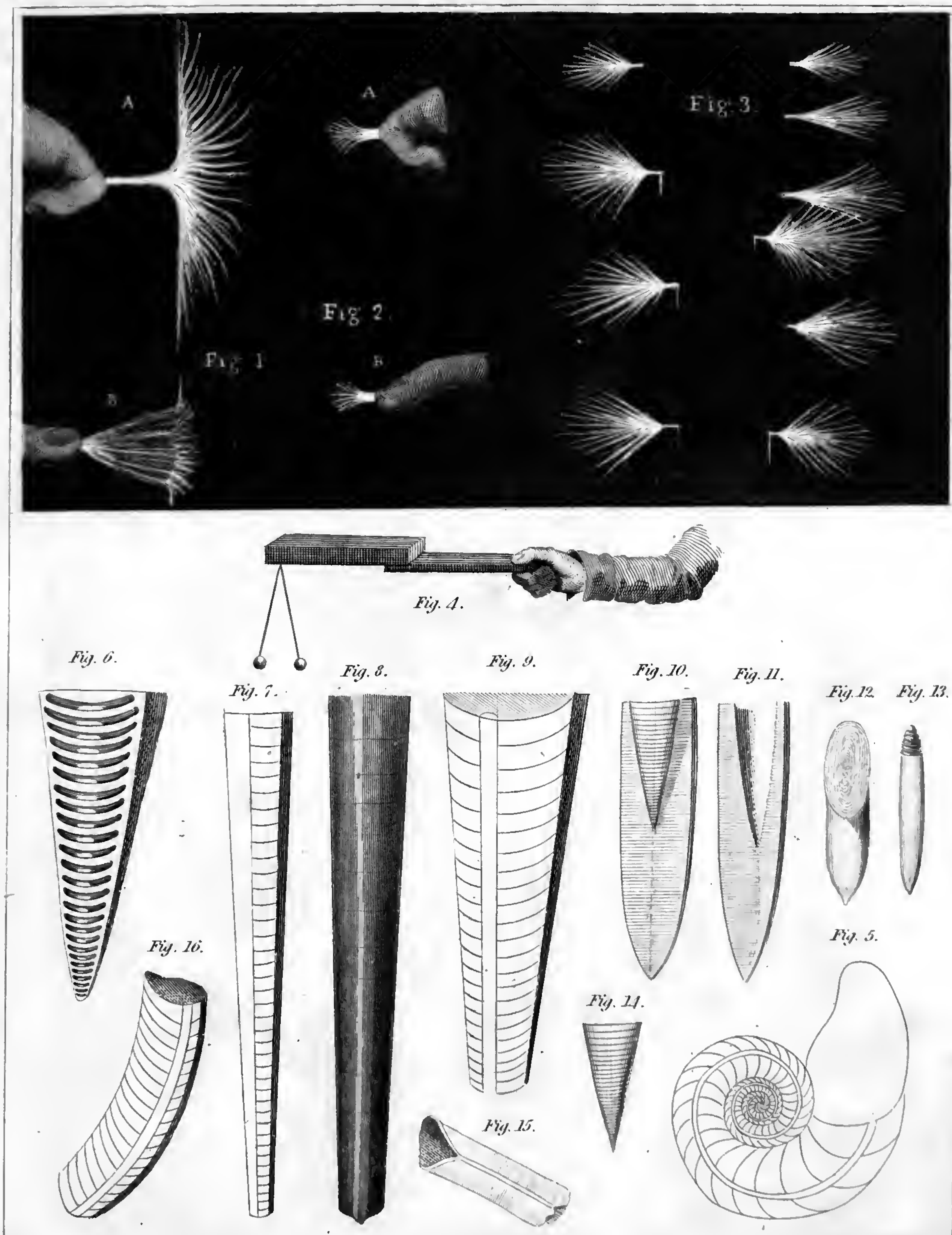

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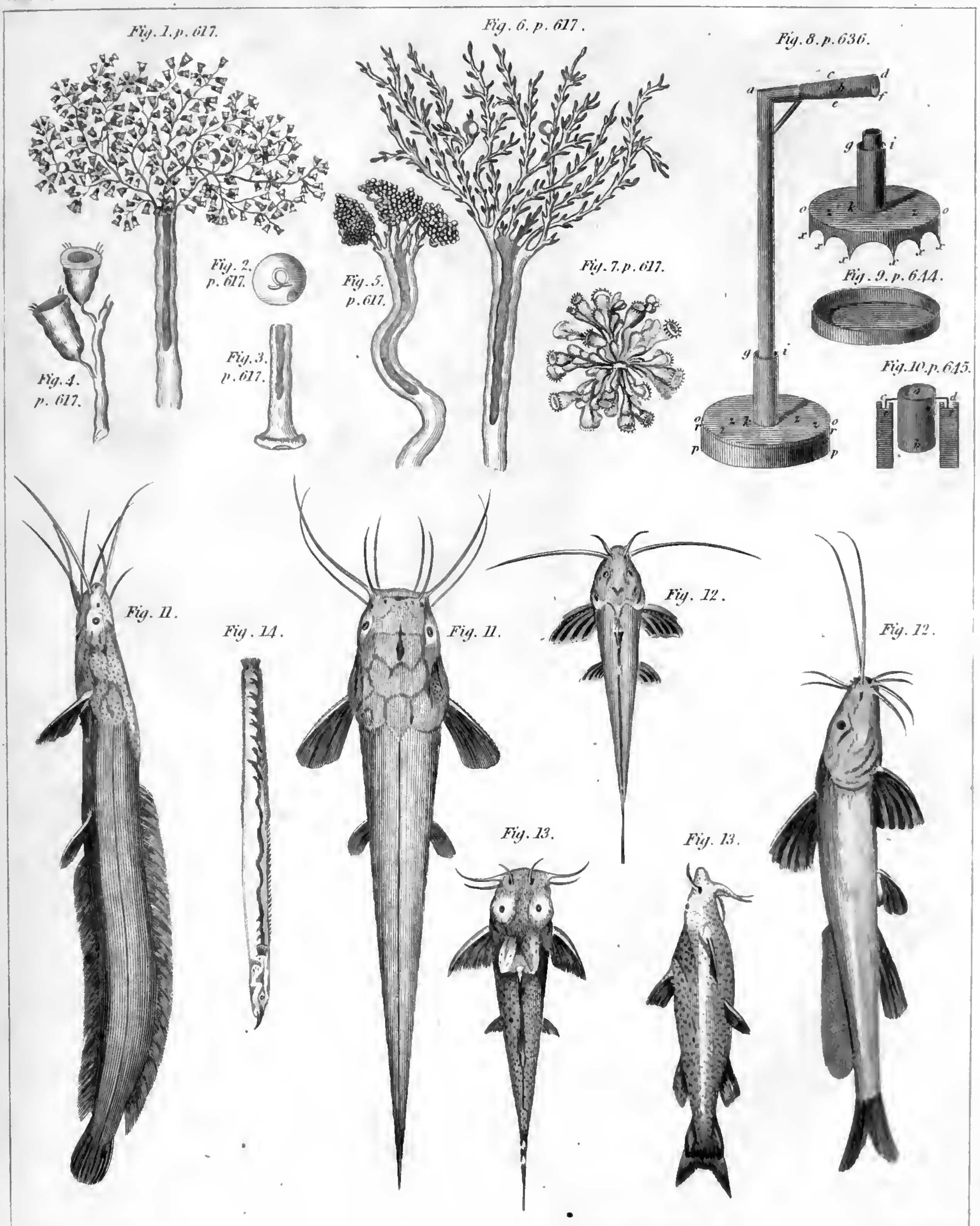
Fig. 1. p. 617.
Fig. 2. p. 617.
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Fig. 7. p. 617.
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Fig. 9. p. 644.
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Fig. 11.
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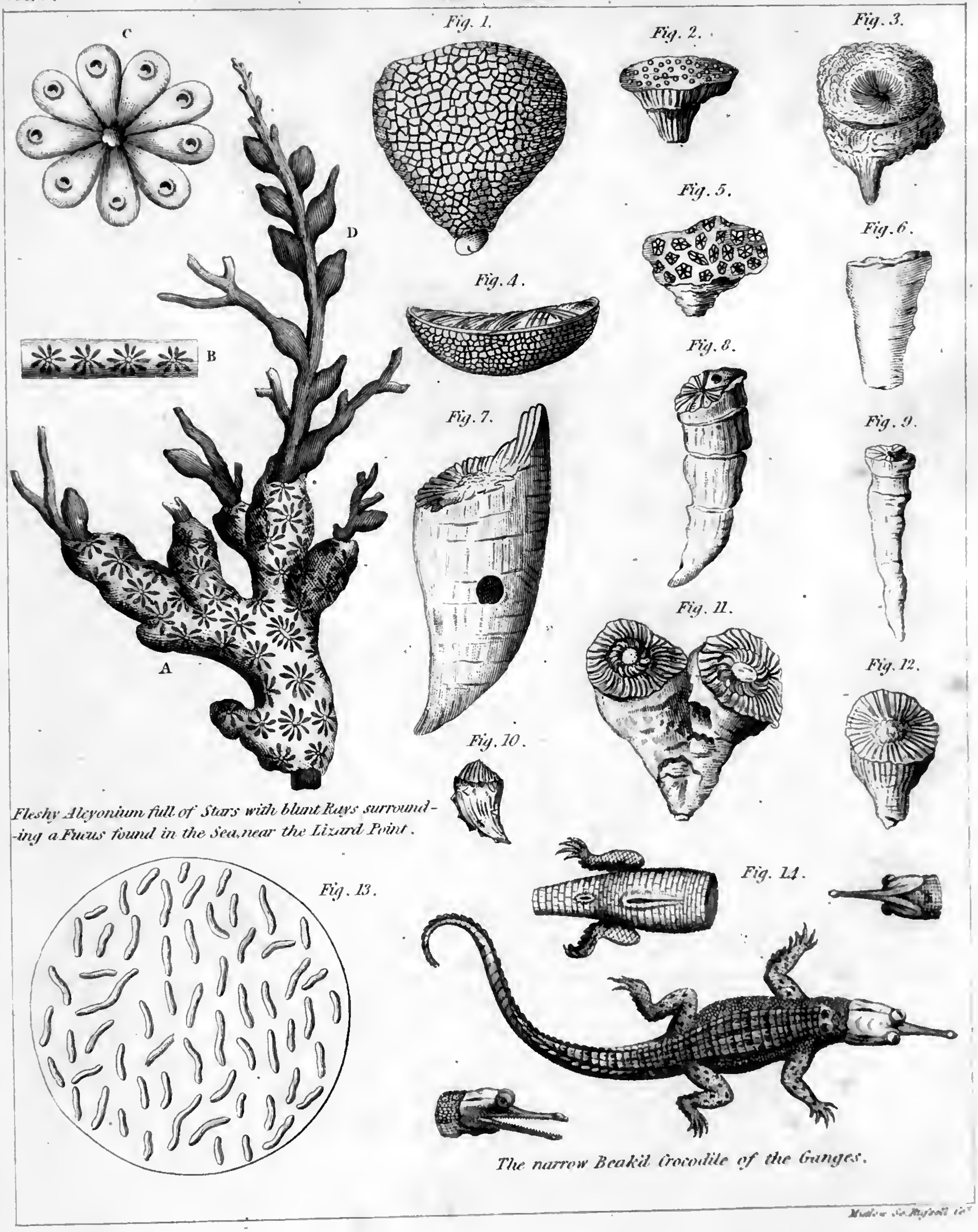
Fig. 13.
Fig. 14.



Fleshy Alcyonium full of Stars with blunt Rays surround-ing a Fucus found in the Sea, near the Lizard Point.

The narrow Beak'd Crocodile of the Ganges.

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